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Original Articles, Selections and Translations.

A NEW THEORY OF TRANCE, AND ITS BEARINGS ON HUMAN TESTIMONY.

By George M. Beard, M. D.

(Read before the New York Medico-Legal Society, November 1st, 1876.)

FOR ages there have been observed a group of symptoms connected with the nervous system, which, under various confusing and contradictory names, have been at once the wonder of the masses, the refuge of delusions, the scandal of law, and the opprobrium of science. Among the more or less incorrect and meaningless terms that either superstition or science has applied to these symptoms, I may specify as especially prominent, somnambulism artificial and spontaneous, mesmerism, animal magnetism, hypnotism, Braidism, catalepsy, ecstasy, and biology.

You will agree with me—all thoughtful minds of our time do agree—that the solution of the mystery suggested by these vague and one-sided phrases, is one of the most important problems of science; since it is of interest, and of the highest moment, not for itself alone, but on account of its relations to physiology and pathology in general, to the philosophy of popular delusions of all kinds and all ages, from

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the Delphic oracles to Brown's mind reading—and, what is perhaps most important of all, on account of its bearings on the principles of evidence, the estimate of human testimony which we are to consider this evening.

It is with the conviction that the problem presented by this subject can be, and is substantially solved, that these intricate and widely extending phenomena can be unified, harmonized, reduced to a science, and brought under the rule of law, that I have chosen trance as the subject of the present address.

In using the word trance to include the real phenomena represented or suggested by the above mentioned terms, I am not guilty of introducing a new term—for it is indeed a very old one—nor of wresting it from its original signification, for it has always been popularly used in the sense in which I here use it, although no one before me, so far as I know, has given it a full and formal definition.

It is necessary to understand, at the beginning, that in order to master the subject of trance, we must use deductive as well as inductive reasoning—in other words, we must reason from generals to particulars, drawing our conclusions from principles already established, as well as from particulars to generals. It is needful to insist on the validity of deductive reasoning, for without it no progress can be made in this branch of science; without it, indeed, it is a useless attempt to study subjects of this kind at all.

It was not long ago that an accomplished friend of mine, a physician, publicly remarked that it seemed to him the wiser course for scientific men to let these subjects alone, not to attempt their solution. He based his objections, perhaps unconsciously, on the fact of the impossibility of solving these problems by the inductive method.

Those who share these views may be reminded that all the sciences in their infancy, astronomy, physics, physiology, and chemistry, have been opposed on substantially the same ground; these things, it was claimed, cannot be understood by the human mind; therefore let us join ourselves to the idols of our ignorance, and let them alone. Grand as have been the results of the inductive method of investigation, as organized by Bacon, it has not displaced, and never will displace

deductive reasoning—and it is a fact of vast encouragement
for the future of human knowledge, that there is now begin-
ning to be a slow reaction in favor of a right use of the think-
ing, as well as of the observing faculties in scientific research.
Clear evidences of this reaction are found in the writings of
nearly all the leaders of scientific thought to-day. Mr. Mill,
in his logic, says truly that the progress of the future must be
made along the line of deductive reasoning; and, quite recently,
Haeckel, in his "History of Creation," has protested in most
vigorous language, against what I may call the tyranny of the
senses. The success of the inductive method has been so
brilliant that we have lost our heads; we have looked so long
at one side of the subject that it has not seemed possible that
it could have any other side. With the triumph of the tele-
graph and railway constantly before our eyes, as reminders of
what the modern world owes or thinks it owes to Bacon for in-
ductive investigation, we forget that, after all, the greatest con-
tributions to human knowledge have been largely the product
of deductive reasoning. Newton's discovery of the law of
gravitation was a deduction, not an induction; and, indeed,
astronomy itself is throughout a deductive science.

The Darwinian theory of natural selection, and, indeed, the
whole law of evolution, of which Darwinianism is but a
single factor, is simply an enormous deduction, which, so far
as it becomes accepted, must be regarded as a far greater
achievement than that of Newton's. Indeed, the Baconian
philosophy of induction is itself a deduction.

The objection may be pressed that deductive reasoning is
liable to error, and the oft-cited prediction of Lardner, that no
steam vessel could ever cross the Atlantic, may be brought to
mind; but all this is but to say that it is human to err, for in
all methods of seeking the truth, there may be mistakes. But
if any comparison were to be made between the fallacies of
these two modes of reasoning, it would be found that where
deduction makes one blunder, induction makes a thou-
sand; and for this reason, if for no other, that everybody can
use, and does use induction, not only in science, but in all the
ordinary affairs of life, while deduction can be mastered only
by a few, since it requires, in all its higher phases, a power of

reasoning that is but rarely possessed or acquired. Difficult
as it is to find those who can make correct scientific obser-
vations, it is incomparably more difficult to find those who can
make just scientific deductions. On this subject Buckle re-
marks: " For one person who can think, there are at least a
hundred persons who can observe; for an accurate observer is no
doubt rare, but an accurate thinker is far rarer." Liebig, dis-
cussing the same subject, says: " Our attaching too high a
value to the mere fact is often a sign of a want of ideas."

So far as the senses are concerned, they deceive all of us
every hour and every moment. The delusion that the senses,
any one, or all of them, can be trusted, is, with the advance of
culture and the development of better notions of the principles
of evidence, fast dying out. With the man whose mind is
trained to right conceptions of the worth and worthlessness of
human testimony, seeing is not believing, but doubting; for
what is all human science but a correcting of the errors,and a
supplementing of the defects of the senses. The sight is, on the
whole, the best of the senses, but in civilized lands only fools
trust it. The savage, casting his eyes upward, sees a blue arch
filled with lights but a little way off and rolling around the
world; the astronomer sitting in his study, deduces that these
lights are worlds millions of miles away, and that the earth
rolls around the sun. It is not the retina, but the brain be-
hind the retina, that really sees, or, in Goethe's language, the
eye sees what it brings the means of seeing. If any man
wishes to blunder, let him but open his eye and believe what
he thinks he sees. Indeed, the much-lauded habit of ex-
clusively trusting the senses, and of making all knowledge a
matter of induction, is not, as some believe, a result of the
Baconian philosophy, but is rather a slowly passing away
survival of savagery. Every barbarian is an imperfect Ba-
conian.

The infinite errors that enter the brain through the doors of
the senses, can be, in a considerable degree, guarded against,
and reduced to a minimum, by the proper use of the
reasoning faculties. All experts in any branch of science, or
of art, consciously or unconsciously correct the defects and un-
certainties, and positive errors of the senses, by the trained

intellect made especially watchful by long study and practice, in special lines of investigation. In all important and difficult matters, therefore, the only testimony that is of any value is expert testimony; if that cannot be obtained, we have no way of knowing anything of the subject.

In the special topic under consideration, the truth is only obtainable by experts and expertness. This most difficult department, in its present state of development, is only mastered after years of study, and on the basis of a thorough knowledge of the physiology of the involuntary life. The non-expert, in studying subjects of this kind, is very apt to believe what he sees and hears, or thinks he sees and hears, and concludes and reports accordingly; the expert, in the presence of the same phenomena, asks and definitely answers the question, whether the image on the retina is subjective or objective; whether he sees all or but a part of what is needful to get the exact truth; whether all things that are seen are seen in their due relations as to time, space and mass; and he also tests the results thus sifted by the established principles of deductive reasoning.

Normal condition.   Ordinary sleep.   Trance.
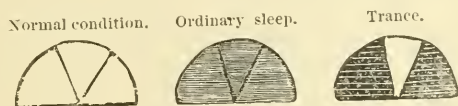
The accompanying cuts will aid somewhat in making clear the general distinction between the normal condition, ordinary sleep, and trance, as regarded by this hypothesis. The cuts are not, however, to be understood as in the light of an attempt to exactly localize any of the cerebral faculties, or to indicate the relative quantity of active and inactive regions in the trance. It is probable that in some forms of trance, nearly the entire brain is active, but enough, is suspended in its activity to cause all the symptoms.

The theory of the nature of trance which I have to offer, and which I now present for the first time, is that it is a functional disease of the nervous system, in which the cerebral activity is concentrated in some limited region of the brain, with suspension of the activity of the rest of the brain, and consequent loss of volition. It is the prime requisite of a scientific

hypothesis that it should account for all the phenomena embraced under the department to which it applies. The hypothesis that trance is a morbid state, consisting in a concentration of the cerebral force in some limited region of the brain, the activity of other portions being meanwhile suspended, seems, as I hope to be able to demonstrate, to account for all the real phenomena of this state, all its different forms and stages.

Before bringing this theory to the test of accounting for all the phenomena, a few general remarks on trance seem to be necessary; and all the more since the current notions on this subject, among scientific men, and even among physiologists, are mostly crude, one-sided, and positively erroneous.

Trance, like other functional nervous diseases, may be induced either physically or psychically; that is, by influences that act on the nervous system, or on the mind; more frequently by the latter, sometimes by both combined. The special exciting causes that may induce this state are, therefore, practically infinite.

Among the physical causes are injuries of the brain, the exhaustion of protracted disease or of starvation, or of over exertion, anæsthetics, alcohol and many drugs, and certain cerebral diseases. Ordinary sleep may act as an exciting cause, as is illustrated in the somnambulistic form of trance. Under the psychical causes are included all conceivable influences whatsoever, that may powerfully excite any emotion, or group of emotions. The majority of the cases of trance come under this head.

Trance may, for the sake of convenience of description, be divided into four varieties: the *spontaneous*, the *self-induced*, the *emotional*, and the *intellectual* trance. In strictness, these varieties may, to a certain extent include each other, and in using these terms, this fact should be borne in mind. Thus, the intellectual trance is spontaneous, although the majority of the cases of spontaneous trance are not also intellectual. The self-induced trance may be partly emotional, but it is not entirely so.

A typical form of spontaneous trance is natural somnambulism, or sleep-walking; a term which is vaguely used by many

writers to include all phases of trance, excepting those which are produced by performances of mesmerizers, which are called cases of artificial somnambulism. In sleep-walking, the cerebral activity, which, during ordinary sleep is more or less lowered throughout the brain, is suddenly concentrated in some limited region; the cerebral equilibrium being spontaneously disturbed through the subjective action of dreams, the subject, under the dominion of this restricted region of the brain, the activity of the rest of the brain being suspended, runs or walks about like an automaton, with exaltation of the sense of touch often, and of the co-ordinating power, as is shown in their capacity for balancing in difficult and dangerous positions, and climbing on heights where in the normal state he would not venture. Other senses may be sealed entirely, as in other forms of trance.

Under self-induced trance are comprised those cases where the subject can bring himself into this state at will, either suddenly, or gradually. Of such subjects it may be said that they will to lose their wills; or it would be nearer the truth to say that they voluntarily put themselves under influences where the involuntary life becomes supreme.

All genuine trance preachers and speakers—and many of them are genuine—represent the self-induced variety. I have studied the case of a famous trance preacher, who tells me—and his statements are in accordance with the laws of trance, although he does not at all understand the true philosophy of his own experience—that when he began to go into this state, the first symptom was only a thrill, or electric shock through his arm, then with more practice the whole arm became convulsed, then the whole body, until in time exaltation of the faculties of imagination, and of language, were developed, and he became a most successful performer before audiences.

The current slang of spiritualistic circles in which mediums are described as "fully developed," or "partially developed," or as "developing," has this basis of truth in it that it oftentimes needs practice to acquire the habit of readily, and at will, entering the trance.

There is such a thing as the habit of being entranced. After one has been a number of times thrown into this state

by any procedure or influence—whatever it may be—he seems
to grow more and more susceptible to the influence of that
special procedure or influence, and will be entranced by it quite
likely, while many other equally good or better methods fail.

Some have a habit of falling into trance spontaneously at
regular intervals; there is indeed, in this disease, the same per-
iodicity oftentimes that has long been observed in neuralgia.
Somnambulism, or sleep-walking, is a form of trance that may
become periodic.

One of the most noted writers on spiritualism has been ac-
customed to induce the trance by starving himself, or, at least,
by living very low. This is a good method, but it is slow and
painful, and may be harmful; for the brain, through want of
just nutrition, gets into a chronic pathological state, when its
equilibrium is readily upset—in nautical language it is thrown
off the center, the imagination becomes abnormally active, and
the subject is oftentimes borne to heaven, where he sees and
hears many wonderful and beautiful things, and reports ac-
cordingly. The visions of Swedenborg, and of Mahomet, possi-
bly—for it is difficult to obtain any detailed information in
regard to the heroes of distant ages that will bear scientific
scrutiny—as well as the ecstacies of many of the mediæval
saints and recluses, and the bright experiences of the dying
everywhere, find here their pathological explanation.

Under emotional trance are included cases that are caused
by the so-called mesmeric performances, or through the feel-
ings of fear, wonder, reverence and expectation, however ex-
cited. The majority of the cases of trance come under this
head; for every one is endowed with these emotions, and in
the greater part of the human race they are the controlling
elements in character—they are especially active and irritable
in the young, and in those who have passed out of childhood,
and have not yet reached full manhood, or womanhood; they
are present, however, in force at all ages, (for in this respect
all men are women and all women are children), and in the
strongest and most intellectual minds they are apt to be in
constant and usually successful rebellion against the authority
of reason—any influences, therefore, that excite any, or all, of
these emotions, may be regarded as exciting causes of trance.

The emotions through which trance is most likely to be excited, are fear, reverence, wonder, and expectation. Among the numberless conditions or circumstances that are liable to excite these emotions, it is needful to specify but a few that are of a representative character, are most familiar and most readily verified, such as standing on a height, or on the track as a train is approaching, or a sudden alarm of fire in a crowded building; the imposing impression made by a pretense of supernatural power, as when it is claimed that the dead are raised, or that departed spirits are communicating with their friends, or that the sick are instantly healed, and similar miracles; and, lastly, and of least importance, the performances of so-called mesmerizers or hypnotizers, as passes to and fro, manipulations, concentrating the attention on some fixed point or bright object, as a button or nail in the wall, drinking of water believed to be magnetized, and so forth.

The one fact common to all these conditions is, that they exert some one or several emotions—fear, or wonder, or expectation—to such a degree that the activity of the rest of the brain is suspended while these emotions are abnormally active, and consequently the will loses control, and the subject acts automatically in response to external or internal suggestion, doing the very things he wishes to avoid doing, and unable to do what he most desires. If on the edge of a height, he becomes dizzy, and may fall; if fearful of being crushed by the approaching train, he cannot move a step; if seated in a _séance_ with supposed mediums, he sees and hears whatever he is told to see and hear, as flames, or light, or sound, and recognizes the faces of departed dear ones in a rubber mask, a pocket-handkerchief, or in a drop of water.

The almost universally held belief that the mesmeric form of emotional trance is caused by some force or fluid (animal magnetism) passing from the body of the operator into the body of the subject, is as far from the truth as any view on any subject possibly can be; and the fact that this view is held not by the masses alone, but probably by ninety-nine out of a hundred physicians and scientists, shows how one-sidedly this whole subject of trance has been studied.

In producing this form of trance, indeed, the presence of

the operator or magnetizer, so-called, is not needed at all;
any influence or circumstance that the subject expects will
put him into the trance is liable to produce that effect, par-
ticularly if, as is usually the case, the emotions of wonder and
fear are at the same time acted upon. Subjects in the mes-
meric trance are under the control of the external suggestions
of the operator, as expressed by voice or manner (not of his
silent, unexpressed will, as some imagine), because they go
into the state with that expectation; otherwise the operator
has no power over him. But there is no need of any operator
at all. Not much less erroneous and one-sided is the con-
clusion of Braid, who, in some respects, has studied the sub-
ject with intelligence, that fatigue of the eyes and power of
attention by looking upward intently at some bright object
—as a key or pencil—is specially important.* The fact is
that it makes no difference as such what you do to produce
mesmeric trance; it makes no difference who does it; it is a
subjective matter entirely, and all depends upon the emotions
of the subject, what he fears, expects, or wonders at. True
enough that a professor of vast fame succeeds in a greater
proportion of cases, but that is because the subject expects
great things of him, and all the needful emotions are created
in his presence. The operator may be an absolute ninny;
but if the subject believes in him, that is enough. He may
make the passes up or down, or crossways, with his fingers,
or with his hands, or he may make no passes at all; he may
sit perfectly still; his presence is not necessary; he may be a
thousand miles away; he may have been dead or buried a
thousand years; he may never have existed: if the subject
strongly believes that he exists, or that he is raised to life, and
expects or fears that he may have the power to put him into a
trance, entranced he will become. Let any one with some
reputation as a mesmerizer request the audience to stand up,
stating also that in a few moments they will be mesmerized,

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*These attempts to induce trance with keys, pencils, and so forth, will
fail almost always even with persons psychically and physically predis-
posed to go into that state, if the subject does not know or suspect that
something of the kind is to happen. I have proved this repeatedly by a
thorough series of experiments with the patients of Demilt Dispensary.

and in ten minutes let him permit those who can to sit down, and it will be found that perhaps one out of ten cannot sit down; they are thoroughly entranced, fixed to the floor, and are ready to do whatever they are told to do. If the statue of the mesmerizer had been on the platform instead of the mesmerizer himself, and if it could have spoken, and the audience could have believed in it, the results would have been just the same. One famous operator used to simply say to his subjects, "Sleep," and they passed into the trance.

In one of the most successful experiments of this kind that I ever witnessed, a number of individuals were seated side by side on a platform before an audience, and were simply directed to put one hand on the wrist of the other, close their eyes, and control their thoughts. In fifteen minutes at least one-third of those who tried the experiment were entranced.

To intellectual trance belong the extreme cases of what are commonly characterized as absent-mindedness; a state which is quite distinct from simple mental attention. The popular term absent-minded, as applied to those who become so absorbed in thought that they are unconscious of what is going on around them, and perhaps respond automatically to external suggestions or influences, is, in view of the theory of trance here advocated, a happy one; since it expresses, with partial correctness, the real state of the brain during an attack of that kind. A large portion of the brain is active, and, until aroused, is insensible to surroundings, and thus responds mechanically. The biographies of illustrious thinkers are filled with instances, some of which are probably correct, of this form of trance. Thus Walter Bagehot says of Adam Smith, the great political economist, that his absence of mind was amazing. On one occasion, having to sign his name to an official document, he produced, not his own signature, but an elaborate imitation of the signature of the person who signed before him; on another occasion, a sentinel on duty having saluted him in military fashion, he astounded and offended the man by acknowledging it with a copy of the same gestures.

In these intellectual trances, great thoughts have been, without doubt, evolved, that would have been impossible to the brain in its normal state.

The hypothesis I have here presented must, if true, account for the actual phenomena of trance in all its forms and stages, however induced. If there be a single fact of trance that is positively inconsistent with this hypothesis, then the hypothesis has no value. It can be shown that by this hypothesis all the facts of all forms and phases of trance are explained, unified and made harmonious; nay, more, that only by this hypothesis, is it possible to give any unity or solidarity to the phenomena of this state.

First. This hypothesis accounts for the loss of the control of the will and the automatism of trance, which is the first observed, and most distinguishing feature.

In his normal state man is, to say the least, nine-tenths a machine. As I have elsewhere said, the involuntary life—that which acts without the will, or in spite of the will—is the chief fact in human life, and in human history. It is the side of humanity that in some most important relations has been least studied, and in which are to be made very largely the physiological discoveries, and the advances of the future.

Comparing life to a wheel, as Dr. S. S. Laws has done, the voluntary functions may represent the narrow hub, while the involuntary functions are represented by all the area between the hub and the periphery. In this little inner circle lies all human responsibility and all the vast influence of punishment or reward; in all the rest of his functions, man is as much an automaton as a tree or a flower. Now, in the trance, this little inner circle, of what we call volition, is encroached upon by the involuntary life, and in the deeper stages is entirely displaced by it. The fully entranced person has no will; what he wishes to do he cannot do; what he wishes not to do he does; he is at the mercy of any external or internal suggestions. Every one who has had the nightmare knows what this feeling of powerlessness is, for the more he tries to run the firmer he sticks. A mesmerized subject, when he tries to do anything against the command of the operator, does just the opposite.

This hypothesis of the concentration of the cerebral activity in a limited region, accounts for the displacement of the will in this way. The will may be defined as the co-ordinated

activity of all the faculties of the mind, including, in general terms, the perception, the emotions and the intellect. Cerebro-physiologists will agree—all questions of phrenology, or cranioscopy, or minute specializations of functions aside—that the brain does not act as a unit, but that different parts are the organs of different faculties. When the cerebral activity is harmoniously diffused, as in the normal state, through all the different regions, the man is said to be under the control of the will. When the cerebral activity is concentrated in some limited region of the brain—say that devoted to the emotions, or that devoted to the intellect, the activity of the rest of the brain being suspended for the time—the man would have no will; he would be under the control of that group of faculties; he would be a conscious living automaton, as a fully-entranced person always is.

SECONDLY. This hypothesis explains why trance is an abnormal state. It shows that it must be a morbid pathological condition, and also shows in what this morbidness consists. The man whose mental faculties are mostly suspended, who has no will, but is under the control of some single faculty, is surely in an abnormal state, and in this respect the popular idea is correct. It is a functional disturbance relating only to circulation and innervation, and not causing structural changes, although it may be caused by structural cerebral diseases, and not ordinarily permanently affecting the health in other respects. The liability to trance, like the liability to various other functional disturbances of the nervous system, does not conflict with general good health and longevity.

If it seem a doubtful thing to class absent-mindedness, or a state of being "frightened to death" under diseases, it must be considered that, strictly speaking, there is no arithmetically defined line between health and disease, but that, as Allbutt has well observed, pathology is but the shady side of physiology.

THIRDLY. This hypothesis explains the difference between trance and ordinary sleep, which in some respects it so much resembles. Sleep is a normal state, a partial cessation of the activity of all the faculties, a lowering of the activity in all the regions, but not a suspension of the activity of any except the

will, which, as we have seen, is simply a co-ordinated action
of the faculties. When a person who is sleeping gets up and
walks in his sleep, in other words, passes into the somnambu-
listic form of trance, the change that takes place in the brain
is this: while sleeping, the activity of all the faculties was
lowered; on going into trance the activity of all the faculties
becomes suspended, and the entire cerebral activity is concen-
trated in some one faculty, or limited group of faculties.

Trance differs from ordinary sleep in the following features:

1. The performances of the trance are logical, coherent,
and consistent; while dreams are filled with extravagances
and absurdities, which to the sleeper seem entirely proper.
By the hypothesis of trance here presented, we should expect
that the dreams acted while in that state, however exalted
they might be, would be restricted and coherent, for they
must arise in a limited region of the brain, and are not like
the dreams of ordinary sleep, the scattered and confused prod-
ucts of all parts of the brain.

2. In trance some of the senses may be perfectly sealed.
The loudest noises are not heard, the most fragrant odors are
not observed, and there is no power of taste. While some of
the senses are thus utterly closed, others may be greatly ex-
alted. On the other hand, the soundest sleepers are awakened
by loud noises, or by sufficiently irritating the sensitive nerves.
This sealing of some of the senses is perfectly clear, if we al-
low certain regions of the brain to be entirely inactive. In
sound sleep all regions of the brain are active, although far less
so than in the waking state, but in no region is there com-
plete suspension of activity. The explanation of the exalta-
tion of the faculties will be given subsequently.

3. Trance subjects are capable of responding to suggestions
offered by a second party, or from any external source, and be-
come consciously obedient to those suggestions. Sleepers
present no such peculiarity; they respond to external suggest-
ions addressed to the senses, but not consciously or coherently.
The entranced person, according to this hypothesis, is a living,
active personality, more active in certain directions than when
in the normal state; and yet he is only a fraction of his nor-
mal self; consequently he is, or may be at the mercy of any

external suggestion that is offered.   He may not be able to re-
sist the external suggestion, as of the so-called mesmerizer, for
example, but he responds consciously and consistently through
that portion of the brain that is active, and without coming
out of the trance.   If one in ordinary sleep becomes fully
conscious so as to respond intelligently to external suggestion,
he wakes up.

4.   In some forms of trance there may be divided or double
consciousness.   The subject, on coming out of the trance, has
no recollection of his experience while in it.   On again enter-
ing the trance, he resumes the experience of the previous at-
tack where it left off, as though no active life had intervened.
If he have a habit of entering into the trance at certain times,
he really leads two quite distinct lives.   In sleep there is no
such continuity of existence, from one nap to another.   The
explanation of double consciousness is referred to under another
head.

In strictness, trance is not sleep at all; it is rather another
form of waking life, over which the will has little or no in-
fluence.   It does not rest one, rather it is exhausting, at least
in some of its phases, and with reason, for the mental and
physical functions are oftentimes exalted.

FOURTHLY.   This hypothesis explains the phenomenon of
dual life and double consciousness, which has been regarded as
one of the greatest and most inexplicable mysteries of trance.

In trance—even in the most profound instances ever ob-
served—there is probably always consciousness at the time,
but it is not always or usually remembered consciousness.   On
awaking, as on awaking from ordinary sleep, the dreams that
may have been active and numberless, fade as a cloud; pos-
sibly not even a glimpse of them may be caught and held be-
fore the mind long enough to become a permanent and recol-
lectible impression during the normal state.   But on resum-
ing the trance state, the exalted functional activity of the
region of the brain in which the cerebral force is concentrated,
is able to bring these impressions of the previous attack of
trance, forgotten during the intervening normal state, to con-
sciousness, and thus the subject carries on an independent
trance life, just as though there had been no intervening nor-

mal state. On returning to the normal state, the cerebral
force being again diffused through the whole brain, is insuf-
ficient to enable the subject to recall the experience of the
trance, but quite sufficient to enable him to recall the experi-
ences of his previous normal state. Thus he leads two lives—
the normal life and the trance life,—and they are independent
of each other.

That very great excitements, or unusual experiences or cir-
cumstances of any kind, may bring to consciousness impres-
sions supposed to be forgotten, is illustrated in the history of
every person at all susceptible; even a little trip in the country,
a walk in the woods, a sea-voyage, may have this effect in a
most interesting way. Extreme illustrations are the well
known experiences of the drowning, or of those in great peril
of any kind. A physician at one time under my professional
care, told me that he was once thrown in his carriage off a high
bridge. In the short interval between leaving the bridge and
reaching the ground beneath, forgotten events of his life
rushed before him, as in the case of drowning men.

It would also be consistent with this hypothesis, if the sub-
ject in the trance state should recall not only previous attacks
of trance, but also the general fact of having been in the nor-
mal state; for in the normal state the whole brain is active,
and in the trance state that portion in which the activity is
concentrated would be able to bring to consciousness the acts
of the normal life in which that same portion of the brain
must have participated. The case of Felida X., recently re-
ported by Dr. Azam, of Bordeaux, France, appears to have
been of this character.* In this case the first attacks appeared
in the fifteenth year, and at first lasted for but ten or
fifteen minutes. The first year, but about one-tenth of her
life was passed in these trance attacks; after the lapse of six-
teen years her life was about equally divided between trance
and health, until finally the former encroached on the latter,
until she passed nearly all the time in trance. Through all
these years of dual existence she always forgets, during her
normal life, the experiences of the trance; nothing seems to

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* A translation by Dr Tucker of Dr. Azam's report, appeared in the *Jour-
nal of Nervous and Mental Disease*, for October, 1876.

be competent to bring them into consciousness but the concentrated and exalted activity of the trance itself.

The case of Miss Reynolds, of Pennsylvania, the details of which were published in 1860, was a type of this feature of trance. On coming out of her first attack, which was at the age of eighteen, she forgot all about her former life, and she had no consciousness of previous existence; she did not know her father, or mother, her brother or her sister. After five weeks she fully recovered her normal state, and with it her memory of her life previous to the attack of trance, but not of the trance itself. For years she alternated between trance and health, leading two distinct lives. In trance she was gay and lively, full of fun, and fancy, and fond of social life; in the healthy state she was sedate and pensive, almost to melancholy, slow though sensible in her intellectual operations, and apparently destitute of imagination.

In these cases of dual life the trance life is the more brilliant and active in certain features, as by this hypothesis it naturally would be. In a case under the observation of the late Dr. J. R. Mitchell, a young girl in trance life was quick, energetic, and witty, and vivacious; in her normal life she was slow, indolent, and querulous.

FIFTHLY. The hypothesis explains the difference between the deeper stages of trance, and death, with which trance is sometimes confounded.

With this hypothesis of the pathology of trance before my mind, I have been accustomed to illustrate the difference between ordinary sleep trance and death, by pointing to a chandelier of gas-burners. When all the burners of the chandelier are fully lighted, that is the normal waking state; when all of the burners are turned down low, but not turned out entirely, as usually is the case in public halls, before the opening of entertainments, that is ordinary sleep; if I turn out entirely all the burners except one, and that one, as often happens, flames all the more brightly from increased pressure, that is trance; if all the burners are turned out entirely and permanently, that is death. The only hold on life which the deeply entranced person has, is through the activity of a limited region of the brain, through which feeble movements of

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the heart are sustained, the body being, in other respects, motionless. The popular belief that deeply entranced persons are liable to be buried alive is correct, but fortunately mistakes of this kind occur but rarely.

One person, it is said was laid out to be buried, all the preparations for the funeral having been made, of which he was fully conscious, and yet he could not at first speak, or cry, or wake, or move. By a desperate effort at the last moment, he succeeded in slightly moving one of his thumbs; the movement was observed and he was saved, and in time came out of his trance.

In the famous case of the late Rev. William Tennent, of New Jersey, a severe illness—some form of fever—was followed by profound trance, which so thoroughly simulated death that the time for his funeral was appointed, and the people were gathered in attendance. At this juncture his physician, who was also a special friend, insisted that he should not be buried at that time, because a tremor in the flesh, which had been observed when the body was laid out, still continued. A brother of Mr. Tennent became impatient at this suggestion, exclaiming: " What, a man not dead who is cold and stiff as a stake! " The suggestion of the physician, however, prevailed, another day for the funeral was appointed, and the people went away. For three days and three nights the doctor worked over his patient, trying, if possible, to find some other symptom of life than the mere tremor in the flesh, but failed. Again the friends appeared at the hour assigned for the funeral, and again the physician plead for time; at first for an hour, then for a half hour, and then for a quarter of an hour. At the last moment Mr. Tennent opened his eyes. His mouth was pried open, and through a quill, liquid nourishment was conveyed to his stomach, and by degrees the patient recovered; but at first he could not write nor read, and was only able to pronounce monosyllables. It was a considerable time—more than a year—before he gained the recollection of his past life, and his acquirements were fully restored to him. During his slow recovery he took lessons in reading and writing, as in his childhood. It is stated by his biographer that the return of his memory was marked by the sensation of a sudden shock in

his head, as he was reciting Cornelius Nepos. He clapped his hand to his head, and then it seemed to him that he had read that book before; the subsequent return of his recollection of his past life, though gradual, was perfect.

After his recovery, Mr. Tennent stated that the three days in which he had appeared lifeless seemed to him not more than ten or twenty minutes; and during that time he seemed to be transported, under the guidance of a superior individual, to heaven, where he saw an innumerable host of happy beings surrounding an " Ineffable Glory;" he was able to hear their songs and their hallelujahs, and he was conscious of joy unutterable.*

In cases of this kind the heart probably beats very feebly, and not always with sufficient force to be detected by ordinary auscultators.

Rosenthal, of Vienna, records a case of trance in an hysterical woman. She was declared dead by her physician. When Rosenthal saw her, the skin was pale and cold, the pupils contracted and not sensitive to light, no pulse could be detected, and there was relaxation of the extremities. Melting sealing-wax, dropped on the surface, caused no reflex movements. When a mirror was held before the mouth no moisture appeared. It was not possible to hear any respiratory murmurs, but in the cardiac region a feeble intermittent sound could be just detected on auscultation. The patient had been apparently dead for thirty-two hours. On examining the patient with the faradic current of electricity, Rosenthal found that the muscles of the face and the extremities contracted. After twelve hours of faradization, she recovered. Two years afterwards she was alive and well, and told Rosenthal that about the commencement of the attack she knew nothing, but that afterwards she heard people talk about her death, but she was powerless to help herself.

SIXTHLY. This hypothesis explains the exaltation of some of the physical and mental faculties in trance, and depression of others.

The exaltation of the physical and psychical faculties in trance cannot be questioned, but is readily demonstrated; and by

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* Life of Rev. William Tennent. New York, 1868.

this hypothesis receives an explanation that is both lucid and complete.

Representing, for the sake of comparison, the quantity of cerebral force in all parts of the brain by one hundred; if the activity of three-fourths of the brain is suspended, then the remaining one-fourth may be four fold more active than when in the normal state. That there should be such a concentration of cerebral force in a limited range of faculties, is in harmony with every day observed facts. Thus the intellect increases in vigor in any direction under exercise up to a certain point, and through over exercise becomes fatigued. In the brain are the centers of thought, of muscular motion, and of general and special sense. It would follow, therefore, that some one or several of the senses, or some one or several of the mental faculties, or some one or several groups of muscles might be exalted in activity, with entire suspension of the activity of other senses, faculties and muscles, according to the region of the brain in which the concentration of activity takes place. There is, therefore, no mystery in the frequently observed, though sometimes disputed fact, that entranced subjects can raise with ease weights which in their normal state they are unable to move. Mesmerized subjects sometimes exhibit this power. Persons entranced through fear, as by an alarm of fire, have been known to take up a stove and carry it out of the house; the next day they cannot, to save their lives, carry back that stove. The co-ordinating or balancing power may be so much exalted in somnambulists that they can climb without harm in most dangerous places. A case in point is related by Dr. Brown-Sequard of a young lady in Paris, who, every Sunday, at ten o'clock, was seized with ecstatic trance; and during the attack would get on a bed, put her toes on the edge, take an attitude of prayer, and offer supplications to the Virgin Mary. In this position, impossible to any one in a waking state, she would stay for a long time, fixed as a statue, her chest moving, her heart beating, and her lips uttering sounds, but the rest of the body absolutely still. The exaltation of the time telling power—which sometimes passes for second sight—has the same explanation as the exaltation of other faculties. Likewise the

nerves of general and special sense are all liable to be greatly
exalted in this state; the feeblest whisper in a distant room
may be readily heard, and one can read by a dimmer light
than is usually needed. The sense of touch may be so deli-
cate that when the sense of sight is sealed, the subject can
find his way from room to room without injury; and it is
claimed may, in some cases, recognize the presence of another
person near at hand, by the temperature alone, even where
there is no physical contact.

These exaltations of the normal senses are the bases of many
of the popular and professional delusions relating to "second
sight," "clairvoyance," "thought reading," and the like.
By this hypothesis also any of the mental faculties should be
liable to be exalted. Observation shows that not only the imag-
ination, but the reasoning faculty and command of language
are oftentimes greatly enlivened in their activity in this state,
as the performances of trance preachers illustrate. Weak
minded men and women, who in the normal state think little
and say less, are sometimes able, when entranced, to speak con-
tinuously, and almost if not quite eloquently, and with slight
apparent effort. While there has been much exaggeration of
the originality and value of these trance speeches, yet it can
not be denied that they are, with all their wildness of fancy
and repetition, and frequent senselessness, far beyond the
capacity of the same persons when not entranced. On return-
ing to the normal state, they may be utterly stupid or com-
monplace; their cerebral force, when diffused through the
whole brain, is unequal to even rapid and sustained small
talk.

The converse of exaltation, depression of some of the senses
and faculties of the mind, directly follows from this hypothesis
of concentration of cerebral activity; those senses and facul-
ties that belong to the entirely inactive regions of the brain,
must be for the time practically dead, as is found to be the
case in some forms of trance. Thus the sealing of some of the
special senses and general anæsthesia, making it possible to
perform without causing pain certain surgical operations, are
accounted for.

SEVENTHLY. This hypothesis explains all the familiar

physical symptoms of trance, such as flushing of the face, fixity of position, sighing respiration, accelerated pulse, involuntary convulsive movements, and marvellous and numberless hysteroid sensations.

The effects of trance on the pulse and respiration, and on the circulation in general, are what would be expected from the known inter-dependence of mind and body. The quite recently established fact of the existence of definite centers of muscular motion in the brain, however the fact may be interpreted, is of great significance in its bearings on this subject, since it shows clearly why convulsions so frequently accompany trance. The aphorism that I have elsewhere laid down, that when we think we move, was based on our knowledge of the existence of these centers of muscular motion, in that portion of the surface of the brain that is regarded as the seat of some of the mental faculties, and was first suggested to me while repeating the experiments of Hitzig and Ferrier in the electrical irritation of the brains of animals.

These convulsive movements in trance, as in hysteria and epilepsy, belong to the lighter phases, or to the coming in and going out of the attack; in the deeper stages the muscles are motionless.

Whether the entranced subject walks about, or remains fixed and immovable, probably depends on the nature of the dream that arises in the portion of the brain the activity of which is exalted.

The vague nervous sensations that often accompany the early and lighter phases of trance-sensations, as of electric shocks, of heat or of cold, of crawling or creeping, or twitching—are in harmony with what the neurologist every day observes in various temperaments where the brain in whole, or in part, is excited. In very many persons the simple expectation that their symptoms are to appear, is sufficient to bring them in full force.

EIGHTHLY. This hypothesis accounts for the illusions and hallucinations of trance.

Illusions, delusions, and hallucinations, are, as is established, the products of cerebral activity, and are frequently the symptoms of some abnormal state of the brain. There is no

proof that any other part of the body than the brain, as the spinal cord or nerves, can originate hallucinations, any more than there is proof that any other part of the body can originate the higher modes of conscious thought—all the facts and arguments that serve to establish that the brain is the organ of the mind in health, also establish that is is the organ of the mind in disease. For while automatic acts, as nursing and so forth, may be manifested by brainless infants, and while the spinal cord clearly contains centers of reflex action, yet there is no proof that any conscious thought, of the *higher* kind at least, attends the activity of these reflex centres, any more than in the familiar automatic movements of plants. The hallucinations of trance—the visions of heaven and other innumerable fancies—must then, like dreams, and all mental operations, whether coherent or incoherent, have their seat in some part of the brain; and, according to this hypothesis, their existence, their coherency, and their extreme activity, are all explained.

Illusions and delusions may arise in brains that are in a normal state, through the necessary deceptions of the senses, but many of them are in part, at least, of a transient character, and are in various ways corrected; but in trance both the false perceptions (illusions), and the false conclusions from what is perceived (delusions) are permanent, and in many cases are immensely more absurd than ever arise in the normal state.

NINTHLY. This hypothesis accounts for the relation of trance to its admitted predisposing and exciting causes.

By this hypothesis any influence that tends to overthrow the cerebral equilibrium, to disturb the balance of innervation, would be likely to be a cause of trance; experience shows that this is actually the case.

The predisposing, like the exciting causes of trance, are both physical and psychical.

One is physically predisposed to trance, so far forth, who inherits, or has acquired a nervous system generally sensitive and impressible. One is psychically predisposed to trance who is mentally unbalanced through excessive and disproportionate endowment of imagination and emotion. One who is powerfully developed in reasoning and thinking qualities,

and is badly deficient in observing, practical faculties is so far
forth predisposed to the intellectual form of trance.   The best
subjects are those who are predisposed, both physically and
psychically, who have sensitive organizations, and unbalanced,
ill-trained minds.

A typical subject for emotional trance, especially the mes-
meric form, is the average shop-girl; she is usually delicate in
body, feeble in mind, or rather, all that is in her of mind runs
to emotion, and that little is but half matured; it is almost as
easy for her to become entranced under exciting causes as it is
to laugh or cry. Persons of this mental calibre and im-
maturity, of either sex, are predisposed to trance, even when
physically they are very strong. Trance is not, however, as
many suppose, the peculiar gift of certain temperaments.   It
is the property of the human race. All persons are liable to
become entranced, just as they are liable to become paralyzed
or epileptic, although all do not suffer in this way. All per-
sons are not predisposed to the same form of this disorder;
one can only be entranced through the intellect, another
through the emotions; one person can only be frightened into
this state; one needs the presence of a medium, another of a
mind reader, another of a clairvoyant, and another of a mes-
merizer; another of a magnetized letter, and another still of
one who performs miracles of healing by the laying on of
hands. Mr. Grimes, who has had much experience with the
mesmeric trance, and who is accustomed to direct his subjects
to stand still with closed eyes and folded hands as a means of
exciting the emotion of reverence, says that he failed with
every one out of forty military officers at West Point, while
just across the river, among the operatives, the same process
was very successful.* This is easy to understand from what

* Mr Grimes has recently published a work, entitled "Mysteries of the
Head and Heart," the latter portion of which especially is commended to
the attention of those who are interested in these themes. Mr. Grimes is
almost the only writer on trance who has had sufficient originality and
mental force to see more than one side of it. But, like Dr. Carpenter, and
most other writers on these subjects he fails to see all sides, and he makes
the mistake which is fatal to the scientific study of trance, of conceding
the possibility of thought reading. This serious mistake results from the
studying the subject inductively instead of deductively; it is, indeed, as I
shall show farther on, an inevitable mistake from that false method of
reasoning.

has already been stated in regard to the predisposing causes, but it would be an error to infer that those officers were not capable of being entranced. If they should all sit in a circle around a table for half an hour or more, with the expectation that some strange things would develop, very likely some of them would become carried away, and, by unconscious muscular motion, would move the table, or perhaps they would feel sensations like electrical shocks through their bodies, or they might go into convulsions, or might experience wonderful visions, hearing the voices and seeing the faces of loved ones.

TENTHLY. This hypothesis accounts for the periodicity of trance in certain cases.

It is the nature of all functional nervous diseases—neuralgia, sick headache, hay fever, inebriety, and some forms of insanity, to appear more or less periodically. It may be said, I think, that the majority of cases of spontaneous trance are periodic. Several of the cases reported in this paper to illustrate various points, it will be observed, agree in this element of periodicity, though widely differing in other features. The famous case of the French sergeant, for example, is a striking illustration of the periodicity of this disease. According to Dr. Mesnet's report, the attacks of trance in this case came on after a hemiplegia, which followed a wound in the head received at Sedan. From one to two or three days of every month are passed in this state, into which he enters spontaneously and instantaneously. While in trance the sense of touch is much exalted, the sense of sight appears to be limited to those objects with which he is brought in direct relation, and is only excited into activity automatically by the sense of touch; all the other senses are sealed. The excessive activity of the sense of touch appears to supply the place of the other senses in a measure, so that he eats, drinks, smokes, dresses and undresses, and retires to bed as in his normal state; but throughout mechanically responds to external suggestions.

Here, as it would seem, I might close the case, with all the real phenomena of trance accounted for, unified, and harmonized by this hypothesis of concentration of activity in a limited region of the brain. It may be opposed to all this process of reasoning, that no one has ever seen with his eyes the

brain thus concentrating its force during an attack of trance; but it must be remembered that only exceptionally can scientific hypotheses be verified by actual sight. Even in the material world the seen is but a fraction of the unseen. No man ever saw the waves of light; no man has ever seen gravity; these universal forces are studied only through their phenomena, by means of which we frame hypotheses of the law of gravitation, and the existence of a luminiferous ether. In the realm of physiology and pathology, the chances of verification by actual sense perception are more rare than in astronomy or physics. Only by deduction can we arrive at any idea of any functional disease whatsoever, or of the relation of mind to brain.

It is essential to the validity of a scientific hypothesis that it account for all the phenomena, and that it be the only hypothesis that will account for them. The supernatural hypothesis of trance, which is the one that has been most generally entertained, even if there were no other argument against it, must be dismissed at once, as soon as it is found that the phenomena can be fully explained by natural causes.

Although we have the authority of the best writers on the principles of science, Whewell and Jevons, for accepting an hypothesis if it be singly and solely competent to account for all the phenomena; yet the best hypotheses, as it seems to me, will bear even severer tests; they will shed light on other and allied phenomena, and will point to new and previously unknown phenomena, for which they will also account. This hypothesis of the nature of trance will bear, in a manner, this severe and supplementary test. It puts us in a position to predict, from our knowledge of the mental and physical characteristics of an individual, whether he will or will not be likely to become the victim of any form of trance. This hypothesis is also of material assistance in studying the automatic functions of the brain and the relation of automatism to responsibility, which just now is the battle ground of science.

This hypothesis also assists us in obtaining a general idea of the nature of sleep, explaining in a general way the philosophy of dreams, the cause of their incoherency, their relation to trance, and the cause of loss of will in that state.

Thousands, if not millions, of pages have been written on the will; all metaphysics is complicated by discussions on, and obscured by ignorance of the nature of this element of the mind. In the light of this theory of trance, the will is not any single, or special, or separate faculty, but the co-ordinated action of all the faculties; and the reason why, in trance, the will is displaced is that the activity of a considerable portion of the faculties is suspended. There is scarcely a problem in psychology that is not simplified by this explanation of the nature of the will, as all who are familiar with the literature of the subject will perceive.

In regard to the other popularly alleged phenomena of trance,—clairvoyant, or second-sight power, or the existence of a sixth sense, by means of which the subject is able to see around and through the world, and into other worlds, or to tell time through the back of the head, or to read with closed eyes, or to see through opaque objects, or to discover lost persons and property, or to reveal the past, or to prophesy with precision, to communicate with spirits of the departed, or to raise the dead—in regard to all these claims, and every claim allied to them, I may say that there are not, and never have been, and never can be any such phenomena. How do we know that phenomena of this kind never have existed and never can exist in the trance? It is not a matter of opinion, or of the *ipse dixit* of any man, or of any number of men; it is a fact capable of absolute proof that no phenomena of this kind have ever appeared in the world in any human creature, in trance or out of trance.

How do we know this? Surely not through inductive reasoning, which is the method usually resorted to in studying this subject. No amount of inductive research will ever advance this branch of science one inch. Suppose that there are a thousand clairvoyants, and mediums and mind-readers who profess to have these powers, and that by the investigation of some competent expert (and none but an expert can conduct investigations of this kind), it is shown that none of them are justified in their claims, that all of them are intentional or unintentional deceivers, what have we found? Simply nothing. We leave off where we started. How do we know that the

next clairvoyant does not have the power that she claims to
have? How do we know that even these one thousand may
not have had the powers they claim just before our investiga-
tion, or just after? You say that exposures of this kind di-
minish the probability that any one has these gifts. Very
true, but in science we are not to be content with a low de-
gree of probability when we can get certainty; and on this
subject we can get certainty. I have no sympathy, therefore,
with the habit, honored though it may be by the endorsement
of some of the greatest names, from the committee of the
French Academy down—of offering large sums of money to
those who will display these marvelous gifts. No plan con-
ceivable could be more unscientific than this. Two years ago,
a friend of mine much interested in these themes, and uncom-
monly successful in the study of them, remarked to me, " I
have laid by a few thousand dollars, the results of labor and
saving; I will give it all to any one who will read the mind
of another." He gave me authority to publicly use his offer,
which I did not do for the reason I have given. I observe that
offers of this kind find no takers, for no one ever was endowed
with any of these divine powers; but the fact of their non-
acceptance is negative evidence merely, and leaves the subject
where it found it.

The true way, the only way to settle this question, is
through deductive reasoning, by the application of this law of
nature, devised from the experience of all authorities in physi-
ology—namely, that no human being ever has any faculty dif-
ferent in *kind* from that conferred on the human race in
general. The law is of universal application, and may embrace
all species of living things; but for our purpose here it is
sufficient to limit it to the human race. The difference between
Socrates, Newton, Shakespeare, and Milton, and the lowest type
of society is a difference of degree only; all the mental facul-
ties are common to all. None of the real phenomena of trance,
as above detailed, differ from those that are common to the
human family otherwise than in degree. When, therefore, we
hear these wondrous stories of second-sight and thought-read-
ing, though endorsed, as they oftentimes are, by the ablest and
most honest scientific men of the world, we need spend no

time or force in investigating them; deduction proves their
falsity without any examination.

We can, if we choose, and are competent to do so, verify and
illustrate our deductions by exposure of those making these
claims, but let us beware of making these exposures with the
idea of proving anything. With the masses of the people,
who are moved by their instincts, these exposures count for
much, I admit; but science has no need of them, and save in
exceptional cases, when incidentally they lead to positive and
original contributions to science, as in the case of the Fox girls,
the table lifters, and Brown, the mind-reader, derives no aid
from them in settling this great question.*

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* Lest I may be accused of inconsistency, I may say that whatever I
have done during the past few years in the way of detecting and exposing
mediums, clairvoyants and mind-readers has been, not for the purpose of
ascertaining the truth or falsity of the claims made by these performers
and their advocates, since that question is, as I have stated above, settled
definitely and forever by deductive reasoning, but partly in order to
solve some questions relating to the psychology of jugglery—a most in-
structive and much neglected subject—and partly, also, out of regard to
the weaker brethren who are unable to employ deductive reasoning, and
can only be taught through what, in some way, appeals to the senses.
These exposures are, in strict logic, no absolute disproval in the abstract
of the claims made by those who are exposed; but their influence with
the people, even with physicians and scientific men, is, as I have found
by experience, enormously greater than any scientific method of treating
the subject possibly can be. Thus, in the case of the Eddy Brothers, who
contracted the habit of raising the dead in immense numbers, in an out
of the way hamlet of Vermont, deductive reasoning was, to one who knew
how to employ it, amply sufficient, without any attempt at special verifi-
cation, to disprove the claim; and yet there were in this country thou-
sands of persons, many of them of the highest intelligence, and some of
them of scientific distinction, who, up to the time of my published expo-
sure, were not a little uneasy in mind in regard to what was alleged, on
excellent average testimony, to be performed there.

Likewise in the case of Brown's mind-reading—in some respects the
most original and remarkable performance connected with delusions—
comparatively few, even of physicians and physiologists were able, at once,
to grasp the true, though somewhat complex explanation which I gave—
unconscious muscular tension and relaxation on the part of the subject—
although the theory was confirmed by careful and repeated experiments;
but the incidental comparison of the mind-reading seances to the "learned
pig" exhibitions, in my letters to the *Tribune*, was more effective in
bringing the trickery into contempt, as I have various reasons for believ-
ing, than any published reasonings or explanations.

Likewise the common objections that these delusions do no good and much harm, and add nothing to the treasury of human knowledge, have no place in science, however useful they may be in practically dealing with the people.

In regard to all these claims, it may be said that in all cases whenever the hand of an expert touches them they vanish into air. Non-expert human testimony of vast proportions, in favor of all these claims, crowds our libraries and our newspapers; but the testimony in favor of witchcraft, of alchemy, of magic, and of astrology is incomparably more imposing; but non-expert human testimony in matters of science is, as you are aware, of no value, and no force of numbers can give it any value. No process of addition can make knowledge out of ignorance; a million ciphers are worth no more than one cipher. A strong boy standing on the shore tries to throw a stone across the Atlantic, and fails; a million boys come to his aid, and each one tries to throw a stone; all combined shall not help that first boy, nor come much nearer to reaching the other shore, though they may toil forever.

The cures of disease wrought on trance subjects are oftentimes genuine cures—the result of mind acting on body, as I have proved by a systematic series of experiments wherein even without the aid of trance, I accomplished, and have, in my paper on the subject before the American Neurological Association, pointed out in detail the way for others to accomplish most remarkable and oftentimes permanent results in serious cases of disease,—results which formerly were regarded as miraculous.

An excellent parallel to the superstitions connected with trance, is found in the perpetual motion delusion. For centuries the belief that perpetual motion was attainable, dominated in the civilized world, and, like the delusions of second sight, clairvoyance, and so forth, was most active in the intelligent classes; thousands upon thousands of machines have been devised, which, by their inventors, and by their friends, were supposed to have solved the problem, and no amount of inductive investigation could ever have availed to settle the question, since but a minority of claims could be exposed by expert skill, and in every direction new claimants

were appearing, and likely to appear. Not until physicists
were able to demonstrate deductively through the law of the
correlation and conservation of force, that perpetual motion
was impossible, was the delusion dispelled. At the present
time all classes of perpetual motion are disallowed by physici-
ists without a moment's examination. It would be as much a
violation of the established and unvarying laws of nature, for
one person to have any faculty different in kind from those
belonging to the race in general—such as second sight, or
clairvoyance, or mind reading, or prevision, or retrovision—as
for the sun to rise in the west instead of the east; by legiti-
mate deductive reasoning the one claim is as conclusively dis-
posed of as the other.

It was only by deductive reasoning that the delusion of
alchemy was gradually overcome. The belief prevailed
in Europe that the baser metals could be turned into
gold; the ablest minds were influenced by the delusion; even
Sir Isaac Newton, it is claimed, did not escape the infection.
Throughout Europe, indeed, alchemists were for centuries as
common and as annoying as clairvoyants and mind-readers
are to-day; and induction was equally powerless against them,
and for the same reason. As chemistry passed slowly out of
the territorial into the organized stage, it became possible to
prove deductively that baser metals could not be turned into
gold, and claims to that effect ceased to be examined. In
his one department of astronomy, Sir Isaac Newton was per-
haps the greatest master of deductive reasoning that the
world has ever seen; but in studying alchemy he was but a
layman, a non-expert, and blundered accordingly; and in this
respect, if in no other, his example has been imitated by a
number of eminent judges, physicists, and naturalists of our
day.

Another excellent parallel drawn from physiology is found
in the belief, in this country at least widely prevalent,
even among the educated classes, that oculists can take out the
eye in a living patient, clean it, and put it back again.
Thousands of individuals of general intelligence are positive
that they have seen, with their own eyes, this operation per-
formed, and many of them would be willing to swear in court

to that effect; patients themselves, as an oculist of reputation,
Dr. Matthewson informs me, are positive that this operation
has been performed on themselves. To meet this delusion by
exhaustive investigation, by exposing, in detail, every claim of
this kind that is, or has been, or will be made in the world, is
manifestly beyond human power; and if such exhaustive ex-
posures had been made, the possibility that such an operation
could be performed might still be urged. Only by deductive
reasoning—by the recognition of the absolutely established
physiological law that it is as impossible to detach the eye
from its nervous, vascular, and muscular connections, and re-
store it to those connections, not only unharmed, but bene-
fited by the procedure, as is claimed, as it would be to cut off
the head and put it back again on the living man—can this de-
lusion be successfully met.

Those who have followed my argument thus far, will find
no difficulty in accounting for the fact, that the trance has for
so long a time been a refuge and a hiding-place for the world's
great imposters. The essence of a delusion is ignorance, and
in all ages the delusions of mankind have sheltered themselves
in the darkness of the unexplained phenomena, and have re-
treated as fast as light is let in upon them. As astronomy
rose astrology sank; as chemistry was developed, alchemy
died away; as medicine became something of a science,
witchcraft lost its vitality; the trance and the automatic side
of human nature have been so full of mystery that in them
the delusions of our time have made their last stand, from
which they are already beginning to retreat.

It is the belief of the wisest men of our time, that there is
a scientific substratum to delusions. This belief is well
founded; there is a scientific substratum to delusions, and it
is found in the subject under consideration, and it is for
that sake alone worthy of the study of all physiologists who
are competent to make themselves masters of this branch of
inquiry.

Those who fully understand the nature and symptoms of
trance, as I have here described them, and the involuntary
life of which trance is, so to speak, the triumph and culmination,
may remain assured that they have possessed themselves of all

that is genuine and important in witchcraft, in clairvoyance, in spiritualism, in mind-reading, and in animal magnetism, which latter is the mother of all modern delusions, being, indeed, to our century what witchcraft was to the fourteenth.

In the light of the theory here proposed, it will be seen that trance has a two-fold interest, scientific and practical—scientific in its relations to both physiology and pathology, and practical in its bearing on the principles of evidence, as obtained through human testimony.

There are various reasons why all that portion of logic, as taught in our colleges and schools of law, that treats of human testimony, must be radically reconstructed, and when this reconstruction is made, as it surely will be, it will be found that much of what is called history, is subjective rather than objective, originating in the brain rather than in external nature; and it will be seen that not an inconsiderable portion of our libraries can be laid aside and forgotten without detriment to the cause of truth, and with large advantage to those who wish to make the wisest use of time. The necessity for thus reconstructing the principles of evidence is based, in part, though not entirely on the fact that all persons are liable to be entranced, and that when in that stage their own statements of what they experience is of little or no value.

On legal medicine trance has a direct and most important application, and in four ways:

First. Testimony as to crimes committed under circumstances of great excitement. Arson, murder, and even theft, especially burglary, may cause or be attended by so much of fear or terror on the part of those who witness them, either sufferers or by-standers, that their testimony as to what occurred may become of slight value. The possibility that witnesses may be entranced through the emotion of fear, is worthy of fair consideration in all cases of this kind where details are testified to and insisted on, and where there is important conflict of evidence between witnesses.

Second. Testimony relating to sudden accidents, attended with fatal or serious consequences.

The value of testimony often brought before coroners' juries is impaired, and quite frequently neutralized altogether by the

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fact that the excitement of sudden accidents tends to entrance
those who are directly concerned; and thus the attempt to fix
responsibility in such cases so often miscarries. A typical in-
stance is found in the disaster that occurred to the yacht
Mohawk, in New York harbor, last summer. It will be re-
membered that this yacht was suddenly capsized, and thrown
completely over while getting under weigh, one afternoon, off
Staten Island. Commodore Garner and wife, and several
others were drowned. The captain was quite generally blamed,
and there was a strong desire among the people to have him
indicted for criminal negligence. The testimony given on
the trial before the coroner was far from being satisfactory to
impartial minds, and the captain was discharged. In cases of
this kind newspapers are violent, and call for the blood of those
whom they suppose to be unpardonably negligent; but coroners'
juries, following their intuitions or their feelings of compas-
sion are unwilling, as a rule, to convict on the testimony usually
brought before them; and in this respect they are right,
although they may not know why. Their impulses and their
intuitions are wiser than the logic and eloquence of editors and
preachers. Imminent peril and unexpected disaster, threaten-
ing or injuring life are likely to excite trance, and thus to di-
minish the value of the testimony of the parties concerned, to
such a degree that we should pause before inflicting punish-
ment. In the case of the Mohawk, the squall was so sudden, and
the capsizing of the yacht so quick, and the position of those
on board so perilous that any attempt to ascertain just what
transpired may as well be abandoned. We shall never know
what occurred after the squall struck the yacht in sufficient
detail to punish any one.

There is no doubt that persons in responsible positions some-
times become entranced as peril approaches, and thus they are
liable to take precisely the wrong course, and to do that which
they especially wish to avoid, like a mesmerized subject. A
few years since, while returning from Europe, our steamer col-
lided with a sailing vessel, under these circumstances. It was
a starlight night, and the sailing vessel was sighted at least
fifteen minutes before the moment of collision, and was not
lost sight of during that time. The vessel was coming across

our bows. Under right management on the part of the officer
of the deck, a collision would have been impossible. If we
had stopped, if we had backed, if we had kept on our course,
if we had turned to the port, all would have been well. There
was but one way in which it was possible for us to run into
the sailing vessel, and that was by turning to the starboard and
chasing her. That course the officer of the deck took, and
succeeded in running into and knocking the masts out of the
sailing vessel. There was no suspicion of intention; there could
have been no motive. The officer in charge was not over-
experienced, probably became entranced, and did just what he
terribly wished to avoid doing.*

In cases of this kind, the responsibility, legal or moral, be-
longs to those who allow men of insufficient capacity or expe-
rience to take positions for which they are not adapted. For
what the man does or does not do at the time he is not crimi-
nally responsible. In those who have the right capacity for a
responsible station, and whose experience in that station has
been large, the liability to become entranced through fear is
reduced to a minimum.

Third. Testimony relating to alleged crime or wrong
deeds committed by entranced persons.

It is quite rarely that subjects in any form of trance are ac-
cused of committing crime while in that state, for this reason.

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* On this occasion was also illustrated, in a most interesting way, the
power of fear to entrance large numbers of individuals simultaneously,
causing them all to see precisely what they feared to see. After the col-
lision, the passengers from the cabin and steerage rushed on the quarter-
deck, many of them overcome with terror, and nearly all of them sup-
posed that our steamer was seriously injured. The sound of the falling
spars, unspeakably dismal in the darkness, the roar of the wind, which
seemed, as usual, to rise at the critical moment, the difficulty and awk-
wardness that attended the slow lowering of the boats on the dashing
waves, the shrieks and prayers and cries of the passengers, all combined
to make the emotion of fear the master of the occasion. In the height of
the excitement, the cry went forth that our steamer was stove in, and that
the bow was sinking. Straightway all eyes were turned toward the bow,
and to every eye it seemed to be sinking. I shall never forget how that
bow gradually lowered in the darkness, as I anxiously gazed upon it; and
yet, so far as I was able to judge, I was more angry at myself for not
having taken some other steamer than fearful of shipwreck. Probably
all, or nearly all the passengers would, if necessary, have testified in court,

mainly, that the commission of crime requires, usually, the
exercise of the will; although, as in some forms of insanity,
the will may be irresponsible. The automatic movements of
entranced subjects, whether called forth by external suggestion
of any kind, or of a subjective origin purely, can rarely lead
to positive crime; although they may accomplish some form
of injury. It is conceivable that a mesmerized subject might,
under the influence of external suggestion, strike a blow that
would injure some one; but there could be no deliberate, sus-
tained attack, with a view to injure. Again, the very sugges-
tion of crime, or serious evil-doing, of any kind, to a mesmer-
ized subject, seems to have the effect to restore the equilibrium
of the brain, like a physical jar, or knock, or push; and he
comes out of the trance, at least sufficiently to be able to resist
the suggestion. Of the large numbers of cases of trance I
have seen, I do not recollect any instance of evil doing of any
kind. Mr. Grimes, who has devoted his life to the practical
study of the subject, declares that subjects, even when fully
under the influence of the operator, and ready to act according
to the suggestions he gives, will not do an indelicate thing.
Say to a young man who has gone into a mesmeric trance with

under oath, that the bow of our steamer gradually sank before them on
that night; and yet, the bow did not sink, the steamer was uninjured.
For the circumstances of that accident, substitute the pretense of mystic
or supernatural power, through the agency of an oracle, a seeress, a sor-
cerer, a necromancer, a witch, a conjurer, a juggler, a magnetizer, a medi-
cine man, a medium, a clairvoyant, a mind-reader, or a repository of
animal magnetism, or odic, or psychic force; for the excitation of the
emotion of fear, substitute the excitation of the emotions of wonder,
reverence and expectancy, combined with fear, and we are in a position to
estimate the value of the literature of delusions.

The day before the burning of the Brooklyn theatre, I lectured on this
subject in that city, and remarked that an alarm of fire was one of the
best exciting causes of trance. The confirmation of this view the follow-
ing day was of a most remarkable and unusual character. Probably the
majority of those in the theatre were more or less entranced; some being
unable to move, others unconscious of what they did, and unable to tell
what happened or what they saw or heard. How animals are entranced
through fear is shown by the conduct of horses when stables take fire;
by the experiments of Czermak with crawfish, hens, frogs, and so forth;
and by the ease with which snakes charm birds, and draw them helpless
within easy reach; but in perilous circumstances masses of men are little
or no better than animals.

the belief that the so-called operator will have an influence
over him, that he is a lawyer, and the jury is before him, and
he will proceed with an eloquent, if not logical plea; tell him
that he is in an orchard, and that the ground is covered with
fruit, which he may pick up and distribute to the audience,
and he will do as suggested; hand him a broomstick, saying
that it is a lady, and he will put his arms around it; give him
a brick, and tell him that it is a baby, and he will hug it; and
yet, if that same young man, while in that state, at the mercy
of the operator apparently, be requested to expose his person,
he would at once refuse, the suggestion acting perhaps as a
means of bringing him out of the trance.

In the case of Tilton *vs.* Beecher, it was claimed privately
by the counsel for Mr. Beecher (if newspaper reports are cor-
rect), and publicly by the committee appointed by Mr. Beecher,
that Mrs. Tilton was entranced at the time she signed some of
her contradictory documents. Dr. Corey, of Brooklyn, who
has had much experience with diseases of the brain, testified
before the committee, according to their report, that the con-
duct of Mrs. Tilton could be explained on that theory. It is
conceivable that so simple a movement as the signing of one's
name to a false document of terrible import, might be automat-
ically made by a mesmerized subject, at the suggestion of the
person whom she supposed put her into the trance. There is,
however, no published evidence that Mrs. Tilton was in a
trance at the time referred to, and no certainty, if she were
in that state, that she was in a condition to respond in detail
automatically to the suggestion of any one. Granting that
she was entranced through a combination of physical weakness
and mental excitement which is possible, the suggestion that
she commit the the great crime of charging unjustly a great
crime on another, would have been likely to have brought her
out of the trance.*

Fourth. Testimony in the trial of those who, under pre-
tense of going into trance, defraud the people.

Nothing is easier to counterfeit, after slight practice, than
the early physical symptoms of trance; the closing of the eyes,

* Dr. Mesnet, of France, states in his report on the case of the French
Sergeant, that he has known of two cases of trance that ended in suicide.

the jerking of the head, the general twitching of the muscles,
and the sighing respiration, can all be simulated by even a poor
actor so perfectly that an expert cannot, without careful in-
vestigation, be sure of the deception.   The exhaustion, on
coming out of trance, is also readily counterfeited.   The great
majority of public clairvoyants, seeresses, and thought readers,
keep out of trance, and while they pretend to go into it, en-
trance their visitors through the emotions of wonder and ex-
pectation, causing them to see what they are told to see, hear
what they expect to hear, and to tell them the secrets they
wish to know, and thus they accomplish their fraudulent pur-
poses.   I have had interviews at various times with many of
the most famous clairvoyants in the country, and under cir-
cumstances where I had all possible opportunity of judging,
and I have yet to see a sure case of genuine trance among them.
·   An expert is justified in testifying in court, first, that these
impostors do not go into the trance at all, as a rule, very few
of them having the power of self-inducing that state, and that
those who do have that power have no use for its exercise when
any fraud is to be brought about; and, secondly, that even those
who can induce trance in themselves, do not have, while in
that state, any of the powers which they claim, as is proved de-
ductively by the known law of physiology already mentioned.
All of these persons, therefore, whenever they profess to have
powers different in kind from other persons, *i. e.,* to find lost
persons, or property, or to tell the past or the future, or diag-
nose disease, or to raise the dead, are guilty of fraud before the
law, and when they are tried, if expert testimony is received
in court, must be without exception, convicted.

On the other hand, if average human testimony—which in
this, as in all scientific matters, is vastly worse than no testi-
mony at all—be received, these deceivers must in every case
be acquitted; for in this country ten non-experts can be found
to testify in their favor to one who will testify against them.
It is for this reason that trials of these cases in this country
have been, without exception, so far as I know, disgraceful
both to science and to law.   In the famous Ward case, in De-
troit, for instance, where an attempt was made to break a will
on the ground that it was made under the influence of me-

diums, no single witness competent to testify on the subject, was called by either side, although the trial was a long one; while public mediums, whose lives are devoted to crime, (among them, I believe, Slade, just convicted in London) were called to give their opinion as experts. What would be thought of summoning some local Lucretia Borgia as an expert in a poisoning case, or some member of the Molly Maguire gang, in a case of murder.

A few years since, according to a newspaper account, a clairvoyant pretender was arrested in Bridgeport, and went through the form of a trial for deceiving a shop girl. A dozen, or more shop girls were allowed to testify that the accused had divine powers, and consequently she was acquitted, and borne off amid the plaudits of the mob.

It may be objected both to the theory and the practical conclusions from the theory here advocated, that even granting their entire soundness in the abstract, they would, if generally understood and held, have a paralyzing influence by encouraging the sceptical spirit, both in science and in law. To this I would reply, that scepticism is a relative term merely, and, rightly viewed, is but another form of belief. Strictly speaking we are all believers. We can no more avoid believing than we can avoid breathing. Superstition and science are both credulous, only that the one believes the false, the other the true. Science is indeed but a higher form of faith; it knows what to receive and what to reject. If the distinction between belief and scepticism be retained, it may be said that superstition is the real sceptic, ignorance is the doubter, and science the only believer. Facts, such as are here urged, are therefore the strongest of all bulwarks against scientific and popular scepticism, against that most baneful of all forms of infidelity so prevalent everywhere, but especially in this country, that rejects everything except average human testimony, that makes the emotions the masters rather the servants of reason, and in seeking for objects of its faith, insists only on one condition, namely, that they be untrue.

As regards trance, there is required no better proof of the need there is of studying it, and of reducing it to a science, as in the present essay I have attempted to do, than the fact that

up to the present moment the tendency has been, even among physicians and physiologists, to reject the real and demonstrable symptoms of this state—such as loss of will, automatism, double consciousness, and exaltation of the faculties, and to accept as genuine the impossible claims of clairvoyance or second sight.

If it be claimed that the application of this theory of trance to law, though ideally right, will be practically impossible, I find my reply in the fact that our courts of justice are already acting upon these principles, and with good results. For a long time now it has been the custom to call experts in cases of suspected poisoning and insanity, and yet every time a lawyer summons an expert, he abandons Greenleaf, who, in his very ably written chapter on the principles of Evidence, as introductory to the Harmony of the Gospels, places honesty as the first qualification for a witness.

One reason, though not the only reason why experts are called in cases of insanity, is that the evidence of the senses, as such, is beginning to be regarded as of little value. Everybody can see the insane, but only a few can see them with expert eyes.

It will now not be a very long step for the law to take to embrace in full the suggestions I have offered, and to accept the principle that in matters of science average human testimony as such, is absolutely worthless. To do so they must throw overboard the principles of evidence as taught not only in books and schools of law, but in all our courses on logic and metaphysics, and in our colleges and schools everywhere, and, as assumed by all who attempt to reason on any subject whatsoever; they must throw overboard the metaphysician, Thomas Reid, who, in his text-book on the intellectual powers of man —long time an authority in Europe and America—uses these words: "An upright judge will give a fair hearing to every objection that can be made to the integrity of a witness, and allow it to be possible that he may be corrupted; but no judge will even suppose that witnesses may be imposed upon by trusting to their eyes and ears; and if a sceptical counsel should plead against the testimony of the witnesses that they had no other evidence from that declared than the testimony

of their eyes and ears, and that we ought not to put so much
faith in our senses as to deprive men of life and fortune upon
their testimony, surely no upright judge would admit a plea
of this kind. I believe no counsel, however sceptical, ever
dared to offer such an argument; and if it was offered it
would be rejected with disdain"; they must throw overboard
Phillips and Greenleaf, Edward W. Cox, the eminent English
lawyer, and legal author, who even declares that if the senses
of honest and intelligent observers are not to be trusted, we
must close our courts of justice.

These authorities which I have quoted—not as uttering
peculiar views on this subject, but simply as representing the
current notions of the world—will be, are, indeed already, be-
ing thrown overboard, for in this direction, as in so many
others, human instinct is wiser than human reason; even as
I write the cable brings the tidings that the justice before
whom the trial of Slade is conducted, has decided that on the
accused rests the burden of proving in court that his trickery
is wrought through the aid of supernatural beings. This view
of the justice is not the correct one, according to the ideal
presented in this paper, but it is probably the longest step
ever made in the direction of that ideal, for it implies an
abandonment of the custom of settling these questions by the
testimony of the dupes of the deceivers. If the testimony of
scores of honest persons, including some of the ablest scien-
tific minds in the world—who believe that Slade is aided by
supernatural powers, in other words, is 'a doer of miracles—
should be allowed to have great weight in his favor, as in
this country it certainly would, he must surely be acquitted at
once: for it is not yet fully recognized that only experts are
competent to deal with these subjects, and it is not generally
known that seances of this kind are the best of all exciting
causes of trance: so powerful indeed that only rare and peculi-
arly organized minds can at first resist them, and that only
after long and special study of the phenomena of the nervous
system in health and disease, can any one even begin the in-
vestigation of this department of science. It is one symptom
of trance that one does not usually know that he is entranced,
and will swear that he is in his right mind, as the intoxicated
man will swear that he is sober.

Just now, in the present infancy of this subject, it would be difficult to try these cases in court by expert testimony, for competent experts are very rarely found. In England, Dr. Carpenter, who has labored so hard, and, in some respects, so successfully in this department of science, is yet so far out of the way as to concede the possibility of thought-reading; and admits that he could not see through the very cheap and coarse and transparent trickery of Slade, which any man can do who has two hands, two feet, a limited audience of scientific non-experts, and a conscience sufficiently seared.

All the sciences of the world have passed through the stage in which the subject under discussion is now passing. We smile when a committee of editors, lawyers and clergymen are extemporaneously appointed to decide in public whether entranced mediums have or have not divine powers granted them in their trance state; but the time has been when the great questions of astronomy, of chemistry, of physics, and of medicine were settled, or supposed to be settled, in very much the same way. Sciences in their infancy are like our distant Western territories, where every one who chooses can squat down and stake out a claim. *

Observe that no new principles of evidence are introduced for application to this special subject. The first step in the development of all the sciences has always been the rejection of average human testimony. If we accept what people say, there can be no scientific knowledge of any kind. Non-expert human testimony is worth nearly as much on this subject as it is on any other branch of science; it is of little value anywhere, and intuitively and unconsciously all authorities on science reject it without examination, although they have not heretofore formulated this custom into a principle; for here, as in many other things, the intuition and unconscious reasoning has outstripped and been far in advance of our conscious reasoning. We have professed in theory, in books of logic and of law, that the senses are trustworthy, and that the concurrent testimony of large numbers of honest witnesses must be accepted as truth. In practice we have oftentimes cut

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* The early history of Electro-physics in the last century excellently illustrates this fact.

away from these false dogmas. Already we are slowly recognizing through our intuitions, and must in time recognize through the reason, that all science is and must be the product of expert skill, and that the first qualification of a witness in in court, or out of court is not honesty, but *competence to judge of the special matter in hand.*

In proportion as a science becomes developed into, and recognized as a specialty, through labors of experts in that branch, it is resigned to those experts, and outsiders do not presume to interfere. Illustrations by contrast are right at our very doors. The recent search for the suspected planet Vulcan was, by universal consent, resigned to astronomers, and no one thought of referring the question to committees of eminent and upright judges, or poets, or physicians; for it was assumed that in an investigation of that kind honesty, as such, counted for nothing; and that ability, even the highest scientific genius in other departments than astronomy, counted for nothing. But when Prof. Huxley, who is one of the few authorities of the world on biology, gave a series of lectures on evolution, which only a trained anatomist and geologist could begin to appreciate, not to say criticise, forthwith, the air is darkened with replies, and criticisms, and suggestions that almost any editor, or teacher, or professor, or clergyman feels competent to offer, with no suspicion of any special absurdity in such procedures. Why this contrast in the treatment which these two departments, astronomy and biology, receive? Partly because astronomy is an older science, has survived the attacks made upon it by theology, while biology is as yet hardly out of the territorial stage, where everybody has claims upon it, experts not having yet made it their exclusive property.*

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* It may be alleged that Prof. Huxley took pains to formally attack an account of creation that belonged to a religion, and that in defense of their own belief, and from their own point of view, theologians, at least, were compelled to reply. Even allowing this privilege to theologians, it cannot be allowed to editors or critics in general; but the majority of theologians, it was observed, did not restrict themselves to the defense of the Mosaic account of creation, from the theological or philological stand-point, but brought positive criticism to bear either for or against Prof. Huxley on his own ground in paleontology, and thus by their rela-

The subject we are discussing is now in this state. Every eminent naturalist, or lawyer, or chemist feels that he can occupy it if he wishes, and that the world must respect his claims. They forget that the greatest blunders in history have been made by men eminent in special departments stepping outside of those departments, and that in all directions the truth of the aphorism of the famous magician Houdin,: "It is easier to dupe a clever man than a fool," is confirmed by all who successfully study these themes. The maxim for modern research should be, out of the mouth of two or three experts every word shall be established: and if they err, as they may, their conclusions are to be revised, not by the laity, but by other and better experts.

The very stupendousness of the claim here made, I may say in closing, may be a bar against its ready accep-

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tions to this special subject, laid themselves open to the charge of being squatters in science. It is a wise rule for scientific men, in public discussions, to religiously avoid religion; and for the simple reason that the theological explanations of natural phenomena, all, or almost all, belong to the pre-exploratory stages of science, and only by coincidence can be expected to agree with the deductions of scientific experts, and therefore do not require either rejection or confirmation. The religious idea of creation has never been adopted as an hypothesis by scientific men, since biology and geology have been in what I here call the territorial stage; it belongs, rather, to the pre-exploratory stage, and being, not a matter of science, but rather of faith, it is not for biologists and geologists to argue for or against it.

The supernatural theory of trance, which has been more generally adopted than any other, I do not here attempt to elaborately disprove, for the same reason, that it belongs to the pre-exploratory stage of this branch of cerebro-physiology; and, unlike the animal magnetism theory, has never been adopted or seriously considered by the scientific men who have given their attention to this subject since it has been in the territorial stage.

If scientific men are persuaded that the beliefs of men are delusions, the better way to destroy them is by independently building up positive truths, which, if they are needed, and the world is ready for them, will be gradually accepted, and correspondingly the delusions must disappear. The antidote for darkness is light; as the day dawns night retreats. Those who understand and accept the theory of trance here unfolded, will dismiss forever the supernatural explanation of the phenomena, just as those who understand and accept the theories of astronomy, dismiss forever from their minds all supernatural associations connected with comets and meteors.

tance: but this objection is counter-balanced, in part at least, by the fact that by acting on these suggestions, it is possible for those who have adequate physiological knowledge and mental discipline, to study this whole subject for themselves, so that it shall not be necessary for them to depend on the dogmatic statements of any one.

Among a certain ancient people, it was the custom for any one proposing a new law to stand with a halter around his neck, so that if his proposition were displeasing to the people, they might hang him on the spot. If this custom were in force to-day, it is probable that the least possible mercy would be shown to the author of this paper; for it is not in harmony with the laws of human nature that views like these, that have only truth on their side, and all the world's past belief against them, shall find instant and secure lodgment. It therefore becomes necessary to appeal from the present to the not very far distant future, when a limited number of competent men shall have made themselves masters of and authorities in this branch of cerebro-physiology and pathology, and shall have learned, as they surely must, that it lies within the grasp of the properly trained and properly furnished intellect, that its problems are no longer unsolvable; that, in short, whatever mysteries have gathered around it in the past, exist no longer, but can be explained, and are explained in full detail, and that they can explain them.

As this rich and fertile field becomes occupied by those who alone are able to cultivate it, amateurs and laymen, and what I here call the squatters of science, will gradually retire, and the grotesque spectacle, but a few weeks ago presented at the British Association for the advancement of science, of a man reading, or attempting to read a paper on the phenomena of trance, not only without any knowledge of the subject, but without any conception of the means by which knowledge of this subject is to be acquired, or of the principles of evidence that apply to the acquisition of physiological science in general, will be remembered and cited only as a melancholy warning.*

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* The above criticism applies as much to the critics of Prof. Barrett as to that gentleman himself; for in all that was said in the discussion that immediately followed the reading of his paper, and by the English press

The delusions that the darkness connected with this subject has sheltered, will die, are dying already; they would die even if they could not be scientifically explained, for witchcraft has been dead a hundred years, although to this moment its phenomena, though fully understood, have never yet been publicly and in detail elucidated. But as the delusions disappear, the facts of trance and of the involuntary life, which together constitute the real scientific substratum of delusions, will proportionately rise in dignity and in interest.

When the Egyptian architect, as you remember, erected by the royal command, a light-house on the shore of the Mediterranean, he placed, as he was directed to do, the name of the reigning king conspicuously on the top, but in perishable plaster, which soon crumbled and fell; and beneath, on the enduring marble, the future saw the name of the architect himself. So the delusions of which we have been speaking, which have attracted so much of the world's wonder, will disappear; but in their place, and out of the phenomena that gave them support will be recognized a positive addition to human knowledge, which, in time, must take its place side by side with other sciences, and become the common and permanent possession of mankind.

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subsequently, the real fallacy in the procedure was, so far as I can learn, never suggested. The study of trance, which was practically the subject of Prof. Barrett's paper, is well worthy of the attention of the British Association; but only acknowledged authorities in cerebral physiology and pathology are competent to prepare, or should be suffered to read essays on that subject before any body that calls itself scientific.

Another evidence that this subject is yet far from the organized stage, is that when Mr. Crookes began his investigations in so-called spiritualism, the press urged him on, as though an eminent chemist and physicist were competent to deal with the mooted questions in physiology; and to complete the absurdity, his reports of what he thought he saw were formally replied to by other chemists and physicists, on the doubly erroneous assumption that his experiments were rightly reported, and that any one outside of experts in that branch of physiology was competent to rightly estimate and reply to them. What Mr. Crookes and his associates did, or tried to do, with the medium, Home, will, in this world, never be known. Astronomy is the oldest of the sciences, but the number of those in the world who are capable of reporting correctly and authoritatively in regard to any alleged new discoveries in that branch, is very limited; and in the department under consideration, to which we are but just

beginning to give serious and successful attention, it may be doubted whether there can be found in Europe and in America combined, a dozen individuals sufficiently expert to report with any approximation to accuracy, what transpires in experiments of this kind.

Mr. Crookes is certainly one of the most ingenious and careful of experimentalists in chemistry and physics, and fully deserves his great and increasing fame; but his experiments in chemistry and physics, even those that are original and refined, are simple matters in comparison with physiological experiments with living human beings. Assuming for the moment the substantial correctness of Mr. Crookes' report of some of the superficial facts relating to what was done in the presence of himself and Mr. Huggins, it is clear that of the six sources of error that enter into all experiments of this kind, and all of which must be absolutely guarded against if the experiments are to have any scientific value, not one was even thought of. For a physicist, or even for a physiologist to try to explain such reports of such experiments, is to be guilty of a blunder but one degree below that of the non-experts who originally made the experiments; is, indeed, to beg the whole question at issue. The position of Mr. Crookes and of Mr. Wallace, and Sergeant Cox, in relation to this matter, is not at all peculiar or unprecedented; the stream of superstition through all the civilized ages, is lined, on either side, with the battered reputations of great and good men.

A scientific gentleman of distinction once remarked to me, that he could only explain the statements of Mr. Crookes on the hypothesis of positive dishonesty. This hypothesis is at once unjust, unnecessary, and unscientific; for in science everywhere, simple non-expertness makes more blunders than the most atrocious dishonesty. A friend of mine, a physician of unusual ability and acquisition, whose conversation is always of great value to me—an observer of far more than average acuteness and originality—is unable to attend any performances where strange or supernatural events are claimed or expected, although a skeptic in such matters, without at once becoming entranced; and the accounts he gives of what he thinks he sees and hears in such places, are as amusing to himself as to others. It cannot be too often repeated, line upon line, and precept upon precept, that in science the prime requisite is not honesty, nor general ability, nor skepticism, nor genius even in other departments, but expert skill; that being absent, all else is as sounding brass and a tinkling cymbal.

Art. II.—POISONOUS MUSHROOMS.

By Isaac Ott, M. D.,

Demonstrator of Physiology, University of Penn.

The eating of mushrooms is a very common practice in France, Austria and Russia, and the deaths therefrom are correspondingly frequent. In these countries they are used by the poor like potatoes are in other countries. In the United States mushrooms are used in the preparation of sauces, and poisoning cases are not so very frequent; but lately, near Newark, N. J., there have occurred a few deaths from this cause. The agaricus comprises nearly one thousand varieties, the majority of which are poisonous. The Agaricus campestris is the edible variety most frequently used. The following are stated to be poisonous: A. muscarius, A. phalloides, a very common and very poisonous variety; A. balbanus, A. rubescens, A. pantherinus, A. virosus, A. solitarius, A. excelsus, A. torminosus, A. fuliginosus, A. crysorrhoens, A. theogogallus, A. necator, and A. rufus. In some encyclopedias, the advice is given that if they are sliced, soaked in vinegar for an hour, and then washed with boiling water they will be innocuous; but, as I shall show, these re-agents will not destroy the active principle in some species, and such guidance is dangerous and should be corrected. It must also not be forgotten that the edible variety under certain unknown conditions may become poisonous. Considering the slight differences between poisonous and edible mushrooms, it is not surprising that men well versed in this lore have made mistakes, which are usually dangerous or fatal. In fact the best advice for the majority of people is to let them alone, as none are very nutritious and all are hard to digest. In some species of agaricus, Letellier found a peculiar substance which he called amanitine. It is a nitrogenous body, soluble in water, but not in alcohol or ether; unites with acids

to form crystallizable salts. Apoiger discovered in them a vola-
tile base, soluble in ether and precipitated by gallic acid, which is
crystallizable, has a smell like that of coniium, but did not kill
rabbits. He also found a crystallizable acid which is volatile
and poisonous to rabbits, and an ethereal oil with a mushroom
smell. When poisonous mushrooms are eaten, the symptoms
are slow to develop, very seldom in half an hour, but usually
in six to twenty hours. They are, colic-like pains in abdomen,
choking, vomiting, great thirst without any considerable fever,
diarrhœa, general coldness, great anxiety, great weakness, pale
countenance, pulse unable to be felt, sometimes tetanus with
frothing at the mouth; finally cramps, convulsions, general
stupor, unconsciousness with great distension of the stomach, and
death. In some cases there is dizziness, colored vision, narrow-
ing of the pupil, flow of saliva, inflammation of the tongue,
pain in the throat, dyspnœa, strangury and so on. The diag-
nosis of deaths from this cause is made by an examination of
the intestinal contents, which under the microscope would pre-
sent the sporules, aided by the fluid character of the blood, and
the great abdominal distension. In the fall of 1868, in the vicin-
ity of the University of Dorpat, Prof. Schmiedeberg[*] collected
some Agaricus muscarius, and by chemical treatment obtained
an alkaloid which he called muscarine; this body is only compar-
able to nicotine, is odorless, colorless and tasteless, strongly alka-
line syrup-like, consistence, soluble in water and absolute alcohol
and insoluble in ether. By standing over sulphuric acid it
gradually becomes crystalline. By heating, the dry crystalline
mass becomes fluid and about 80° Cent. begins to brown, and
when heated over 100 C. (212° F.) it is still solid, but by a higher
heat it melts with the generation of a weak tobacco-like odor,
and is consumed without any disposition to sublime.

Schmiedeberg[+] has since shown that muscarine is isomeric
with betaine. In the A. muscarius, he has also found a second
base not poisonous, isomeric, and corresponding with choline or
hydroxaethylidentrimethylammoniumhydrate in all important
peculiarities, and which by heating gives trymethylamine.

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* S. und Koppe. *Das Muscarine.* Leipzig, 1869.

† S. und Harnack. *Centralblatt,* 1875, No. 36.

This second base which he calls amanitine, by oxidation with nitric acid gives, not as choline, betaine, but muscarine.

Amanitine must be looked upon as a hydroxaethylidentrimethylammoniumhydrate, that is

$$(N \begin{cases} (CH)3 \\ CHOH-CH_3 \\ CH \end{cases})$$

and muscarine as
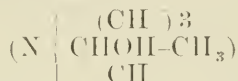
$$N \begin{cases} (CH_3)3 \\ CHOH-CHO \\ CH \end{cases}$$
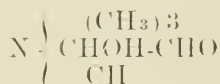
He also obtained from egg-albumen by chemical treatment, muscarine which did not differ from that obtained from the mushroom.

On cats, Schmiedeberg found muscarine in subcutaneous doses of a milligramme of the sulphate to cause, in a few minutes, chewing or licking movements, profuse flow of saliva, increased secretion of tears, colic, vomiting, increased intestinal peristalsis, purging which is often bloody, with tenesmus-like pain, increased flow of saliva, which depends on peripheral stimulation of the chorda tympani; the narrowing of the pupil is one of the most constant symptoms. All these phenomena can take place before the movements are interfered with; the pulse sinks from the beginning; the breathing is very frequent and dyspnœic, the animal is sensitive to touch, and has a tottering gait. Later the respiratory frequency sinks; the animal lies extended, respiratory movements become weaker, and death ensues, preceded often by slight convulsions. The same series of symptoms take place in the dog. In rabbits the pulse frequency and secretion of saliva occur as in cats, but the respirations are more interfered with. On frogs the symptoms are mainly confined to the heart causing its arrest. The cause of death is in the respiratory apparatus and the heart; although the former stops first, it is not definitely decided whether it is by a direct action on the centres of respiration or through the slowness of the heart. Disturbances in the nervous system are probably not direct but a result of want of cardiac activity. Schmiedeberg also found that atropine was an antidote even in the agony of impending death.

Having through the kindness of Prof. Schmiedeberg received some chloride of muscarine. I made some experiments with it on frogs to test the antidotal property of atropine; I was further pressed thereto by the recent cases of poisoning, and the remarks of a medical journal* thereon.

Experiment 1. Frog-sternum removed.

3.55 P. M. Heart-beat, 36 per minute.

3.58   .0005 gr. muscarine subcutaneously.

4   Heart stopped in diastole, on pricking makes a contraction; the ventricle is distended with blood, bulbous.

4.3.   .001 gr. atropine subcutaneously.

4.5   It spontaneously began to beat 28 per minute.

4.9   Heart-beat, thirty-six per minute. It continued beating till next morning and animal had completely recovered from the paralysis induced by muscarine.

Experiment 2. Toad, sternum removed.

12.57 P. M. Heart-beat, 44 per minute.

12.58   A little muscarine subcutaneously.

1   Heart-beat, six per minute.

1.12   Heart stopped in diastole.

1.40   A little atropine introduced.

1.47   Ventricle commences to beat of its own accord twenty-four per minute.

1.54   Muscarine subcutaneously, but it only slightly slows the heart.

As is seen, muscarine is able to stop the heart, and atropine is able in the most beautiful manner to start it. As is known, there is situated in the heart certain nerve masses, called ganglia, which are divided into two kinds, inhibitory and excito-motor. Comparing the heart to a steam-engine, the excito-motor represent the steam, and the cardiac muscle the driving-wheel, and the inhibitory ganglia the brakes. Now, muscarine puts on the brakes, the driving-wheels stop because the steam or excito-motor ganglia are not able to overcome the resistence of the brakes, or inhibitory ganglia. But in

*Med. and Surg. Reporter, Oct. 14, 1876.

atropine there is a force which in doses of a milligramme, is
able to remove the brakes, when the driving-wheels, that is the
auricles and ventricle revolve under the force of the excito-
motor ganglia. By this poison he was also able to ascertain
that nicotine was unable to start a muscarinized heart because
it acted on the handle of the brakes, and not on themselves.
Both nicotine and atropine paralyze the pneumogastrics; hence
he inferred that nicotine acted not like atropine on the inhib-
itory ganglia but on ganglia connecting them with the ends of
the pneumogastric. The antidotal effects of atropine to mus-
carine have been confirmed by Prevost[*] of Geneva. Böhm[†]
of Dorpat and Schiff of Florence.

My experiments show that atropine is a complete antidote
to muscarine.

Atropine paralyzes the chorda tympani, muscarine excites it;
atropine paralyzes the cardio-inhibitory apparatus, muscarine
excites it; atropine dilates the pupil, muscarine contracts it;
atropine causes decreased intestinal peristalsis, muscarine in-
creases it; muscarine decreases or stops the urinary secretion,
atropine restores it.

Krenchel[‡] has also shown that it causes a cramp of the ac-
commodation in the eye which reaches its maximum in 15-20
minutes and passes away in one to one and a half hours.

Muscarine acts like calabar bean in some respects. Like the
latter it excites tetanus of the intestinal tract, it contracts the
pupil and produces a cramp of the power of accomodation, it
excites the cardio-inhibitory apparatus which excitation is re-
moved by atropine, restored by calabar, but not by muscarine.
The question arises, how is atropine an antidote? As is well
known atropine affords great relief in opium-poisoning. Now
in opium-poisoning not only does the patient forget to breathe,
but the want of respiratory action is the cause of death. Von
Bezold and Bloebaum have proved that atropine is a great
stimulant to the centres of respiration, and allows the heart
to run much faster. Here the excitant action on the respira-
tory centres and the sending of more blood to them with cor-

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[*] *Centralblatt*, 16, 1875.

[†] Die Herzgifte.

[‡] *Centralblatt*, 16, 1875.

respondingly more oxygen, relieves them from the narcosis. But in muscarine-poisoning, it is probably restoration of the heart to activity, that mainly is the factor, although the excitant action of atropine on the respiratory centres is not an unimportant element. It is highly probable that atropine is of much value in other cases of poisoning, where the death takes place by arrest of the respiratory apparatus.

Now the query arises: Is muscarine the poisonous principle in all poisonous mushrooms? On man after the subcutaneous use of three to five milligrammes of muscarine, Koppe states that there ensues profuse salivation and rush of blood to the head; redness of the face; the brow is moist and vertigo ensues; with this is griping and colic, and large drops of perspiration stand out on the face. The disturbed vision in connection with the vertigo and the weariness of the head have a remote similarity to the action of alcohol. These poisonous effects on man have a great similarity to the symptoms of mushroom-poisoning.

Schiff thinks that there is another alkaloid, but the most that can be said is, that the probabilities are strong that there is only one alkaloid which causes these deaths. It is quite true that there may be some narcotic principle associated with it. The following *résumé* expresses our conclusions.

1. That at least, in one species, Agaricus muscarius, there is an alkaloid called muscarine; that A. muscarius also contains a base called amanitine, a non-poisonous body.

2. That muscarine is a highly poisonous agent and that it is probably the poisonous body in all mushrooms of a noxious nature, associated with another alkaloid.

3. That in mushroom-poisoning with the usual employment of emetics, stomach-pump, purgatives and gallic acid, atropine should be given subcutaneously, say $\frac{1}{100}$ of a grain, the dose to be repeated according to indications.

PHYSIOLOGICAL LABORATORY, UNIVERSITY OF PENNA.

Art. III.—HYSTERICAL MUSCULAR CONTRACTIONS.

By Wm. A. Hammond, M. D.

Professor of Diseases of the Mind and Nervous System, in the University of the City of New York, etc.

[*Read before the New York Neurological Society, Nov. 6th,* 1876.]

THERE is scarcely a muscle, or a group of muscles in the body which may not be the seat of hysterical contraction; and though this fact has long been known, the resemblance to organic disease is such that physicians and surgeons are still deceived by the counterfeits. As examples of the condition and of the errors to which it may give rise, I beg to submit the following clinical histories, which I have arranged into classes, according to the anatomical seat of the disease:

1. *Hysterical contraction of the muscles of the face and neck.*

Case 1. I. B. A., a gentleman aged 32, and a writer for the press from a neighboring State, was referred to me by a physician, to be treated for Bell's paralysis of the right side of the face, of eight or ten days' standing.

On inspection, it was very evident that the symmetry of the face was destroyed: but very slight further examination was sufficient to show that this was not due to any paralysis of the muscles on the right side, but to strong tonic contractions of some of those on the left side. In fact, there was not a single symptom of that affection present. The forehead could be wrinkled, the brow corrugated, the eye closed, the angle of the mouth extended, the upper lip elevated, the buccinator puffed out, and the orbicularis oris pursed up so far as the strongly contracted left levator anguli oris and zygomatic muscles would permit. Indeed, careful search failed to reveal the existence of any other derangement than persistent con-

traction of these muscles. The aspect of the patient was very striking. The angle of the mouth was retracted and elevated to its fullest extent, exposing the teeth on the left side, impeding mastication, and preventing the patient drinking any fluid except by taking it into the mouth with a spoon.

Of course, there was no valid cause why this case should, even for a moment, have been regarded as one of facial paralysis; but what has happened once may occur again, and I have reason to believe that instances of this kind are by no means very rare.

Upon questioning the patient, I ascertained that the contraction had ensued during a period of great emotional disturbance, and that he was of a very impressionable and nervous organization. There was no marked derangement of the general health.

I did nothing for him but prescribe the bromide of zinc in gradually increasing doses, beginning with two grains three times a day, and in the course of a week the muscles relaxed and the patient was well.

CASE II. Mrs. B., a lady aged 36, had been repeatedly subject to hysterical attacks of various kinds, and on one occasion was the subject of hemiplegia, clearly hysterical in character. One day, about a month before she came under my observation, she was startled by a sudden alarm of fire in an adjoining house, and was on the instant seized with contraction of the left side of the face. The eye was tightly closed, and the angle of the mouth retracted and elevated to its utmost extent. The family physician, a homœopathist, regarding the case as one of facial paralysis, applied the faradaic current to the right side, and gave two or three hypodermic injections of strychnia. It is, perhaps, scarcely necessary to say that, as in the preceding case, there was no sign of paralysis. The treatment with electricity was continued for a month without effect, and then the patient came under my care. At that time the left eye was tightly shut, and the skin permanently wrinkled by the force of the contraction of the orbicularis palpebrarum muscle; the other muscles of the left side were all in a state of rigid contraction except the masseter, pterygoid, and temporal. There was, therefore no trismus, the muscles supplied by the facial or eighth nerve being alone involved.

I determined to bring this patient as rapidly as possible under the full influence of the bromide of sodium, and therefore prescribed a solution in water containing thirty grains to the teaspoonful, three times a day. In the course of three or four days, the effects of the medicine began to be manifested, and as the bromism became more profound, the contractions began to relax, till, by the tenth day they had entirely disappeared, and the symmetry of the face was restored.

Case III. Miss C., a young lady eighteen years of age, and of a strongly hysterical temperament, was brought to me to be treated for rigidity of the jaws. As she expressed it, she could not open her mouth wide enough to allow of her eating a strawberry. Upon examination, I found both masseter and temporal muscles rigid, and she could barely open the mouth to the extent of separating the teeth a half an inch. Even here the true nature of the disorder was not recognized, as she had been treated for paralysis of the antagonizing muscles, with electricity and strychnia, by an " electrician," who ought to have known better. I subjected her to treatment with the bromide of zinc ⟨R ʒinci bromidi ʒij, aqua ʒij tr. p. m.; dose 10 drops three times a day, increasing a drop for the doses of each day, and gradually after a few days the process of relaxation began, and in twelve days from the time of beginning treatment, she was entirely well. I continued the solution as above till a month had elapsed, at which time she was taking forty drops three times a day. There has now, after a lapse of four months nearly, been no return of the trouble. Two other cases similar to this last are now under similar treatment with thus far good results. In one of these cases for the first time, on the 2d inst., I recommended in addition to the bromide the use of the faradaic current rapidly interrupted to the contracted temporal and masseter muscles.

No very severe case of hysterical trismus has come under my observation, and I am not therefore able to speak of my own knowledge relative to the efficacy of very large doses of atropia. Thus far the use of any one of the bromides has fulfilled every indication.

A case very much worse than any I have seen, and belonging to the category of hysterical contractions of the muscles of

the face, is reported by Bourneville.[1] The patient, a woman 39 years old, had suffered from previous hysterical manifestations, and was seized with contraction of the muscles of the right side of the face, in consequence of the sensation induced by the unexpected visit of the attending physician, M. Hugueier. Both this gentleman and M. Sandras regarded the case as one of paralysis of the left side of the face. This side was leeched and treated with electricity. Suddenly after six weeks the contraction disappeared.

CASE IV. Miss S., aged 34, consulted me in December, 1874, for double vision, with which she had been affected for seven months. She had been treated for cerebral disease—first for meningitis and then for tumor—during the course of which she had been blistered, leeched, and subjected to very thorough medication with mercury and iodide of potassium. On ophthalmoscopic examination I could discover no evidence of brain lesion. Both eyes were strongly convergent. There was either paralysis of both external recti muscles or contraction of both internal recti. I came to the conclusion that the latter was the case—mainly for the reasons that there were no head symptoms, and that the affection had been developed suddenly as the apparent consequence of a powerful emotional cause; that there were also occasional contractions of the muscles of the angles of the mouth, sometimes on one side and again on the other, and that there were other hysterical phenomena present, such as the *globus* and fits of laughing and crying.

As the affection had lasted several months, I despaired of being able to relieve it by medicines, and wrote a note to a distinguished eye surgeon of this city referring the case to him for operation. Giving her the note to deliver, I told her its contents, and advised her to have the matter attended to at once. The next day she returned. Her eyes were perfectly straight, and the double vision had disappeared. She stated that the idea of an operation had frightened her; that she had gone to bed thinking of what she would probably have to endure the next day when she presented the letter, and that when she awoke in the morning she discovered at once that

[1] *De la contracture hysterique permanente,* *Paris,* 1872, *p.* 34.

the diplopia had gone, and that her eyes no longer converged. So far as I know she has had no return of the trouble.

CASE V. While engaged in writing the present paper, Mrs. T., aged 34, was referred to me by Dr. Downs of this city for my opinion and advice in regard to double vision, from which she had suffered for almost two years.

On examination I found both eyes convergent, the left rather more so than the right. There was also slight nystagmus in both eyes. The external recti acted quite well, but as soon as the will ceased to be exerted upon them the eyes at once converged through the contraction of the internal recti. At a distance of five feet or less there was but a single image, but at greater distance two were seen, and at fifteen feet were about eighteen inches apart and exactly on the same plane.

Thorough ophthalmoscopic examination showed both retinae and disks to be perfectly healthy, and there were no other symptoms indicating cerebral disease except an occasional vertiginous sensation. The patient had suffered from that classical hysterical condition, tonic contraction of the fingers, and had also from time to time exhibited other evidences of the hysterical temperament, as for instance passing large quantities of pale urine. I therefore expressed the opinion that the diplopia was due to tonic contraction of both internal recti muscles, and recommended the use of bromide of sodium and fluid extract of ergot—with what result remains to be seen, though I think the prognosis is favorable.*

CASE VI. Mrs. T., aged 28, had been under treatment for wry neck, and had submitted to division of the left sterno-cleido-mastoid muscle two months before she came under my observation. The operation was of course followed by no beneficial result, as the contraction was situated on the opposite side. When I first saw her she had been affected for about a year— the contraction coming on soon after a fright from being attacked by a savage dog.

On examination I found the chin drawn over to the left side and closely approximating the corresponding clavicle. The sterno-cleido-mastoid of the right side was apparently the only muscle affected. It was possible by gentle tractile force to entirely overcome the tonic contraction.

*This patient entirely recovered under the treatment.

Concluding that this was a suitable case for the employment of hypodermic injections of atropia, as recommended by Dr. Da Costa* for wry neck of a rheumatic character, I began the treatment by the introduction under the skin of the $\frac{1}{50}$ grain of sulphate of atropia. There was no apparent effect upon the muscle, but the influence of the remedy was very distinctly shown in the redness of the face, dryness of the fauces, and disturbance of vision which ensued. The following day, I gave the $\frac{1}{45}$ gr. and the day after the $\frac{1}{40}$. I continued to increase the quantity gradually daily, till on the 10th there was a slight amelioration perceptible. She was then getting the $\frac{1}{30}$ gr. Amendment from this time was steady under more gradual augmentations of the doses, and on the 22d day the distortion was gone and the muscle was fully relaxed. Treatment was then suspended, and the patient has remained well to this day, though still strongly hysterical.

CASE VII. Mrs. S., aged 38, came under my care some six years since, to be treated for wry neck; apparently hysterical in character, and involving the left sterno-cleido-mastoid muscle. The lady was subject to attacks of "hysterics," but was otherwise in good health. The affection had been developed suddenly, so far as she knew, without obvious cause, and was unaccompanied with pain.

I treated her with hypodermic injections of the sulphate of atropia, reaching gradually to large doses, but without appreciable result, and with zinc, bromide of potassium, conium, opium, and many other substances, to no avail. I also made thorough use of galvanism and faradism to the contracted and normal muscles, with like result. I finally determined to divide the muscle. Accordingly the tendon of the left sterno-cleido-mastoid was cut in the usual manner. There was at once a complete restoration of the head to its natural position and movements. The cut extremities were kept apart, and in the course of a few weeks union was affected.

But while the process was going on, contraction began in the right sterno-cleido-mastoid muscle, and the head was drawn violently in the direction opposite to that which had formerly existed. Deeming all other measures futile, I divided the

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*Pennsylvania Hospital Reports, Vol. I, 1868, p. 391.

muscle. Again relief was instantaneous, but very soon afterwards contraction began in the left trapezius, and gradually becoming more pronounced, caused the head to be drawn backwards, and towards the left shoulder.

I divided the occipital attachment of the muscle, with, as before, prompt relief, but in a few days the left levator anguli scapulæ became more largely affected. This was also cut, and then the contractions disappeared for several weeks, but at last re-appeared apparently in the left complexus.

I now requested my friend, Dr. Markoe, to see the patient, and he agreed with me that it was desirable to divide the muscle as high up as possible. Assisted by him I performed the operation—the patient being placed under the influence of ether. Restoration was complete as in the previous operations, but very soon afterwards the right sterno-cleido-mastoid again began to contract. I divided it, and then the left became similarly involved again.

The patient was now, after these operations of tenotomy and myotomy, exactly as she was when she originally came under my care, more than a year previously. I regarded the case as beyond the reach of medical or surgical aid, and she soon afterwards returned home. I heard from her several times subsequently as unrelieved, but about two years ago I had a message from her to the effect that she had suddenly, without medical treatment, become entirely free from her disorder, and was as well as at any time before its origination.

This is certainly a very remarkable and instructive case. It shows how futile our remedies oftentimes are, and how guarded we should be in attributing the disappearance of a disease to the direct influence of anything we may do. Had this lady recovered immediately after medical treatment, or any one of the operations, the inference would probably have been drawn that the two circumstances stood in the relation of cause and effect, and that, too, with far more reason than can be alleged in favor of some other cases in which cures have been reported.

CASE VIII. Mrs. P., aged 30, received a severe mental shock, and immediately felt her head drawn over to the left side, so that she had the appearance of looking over that

shoulder. She consulted a surgeon, who cut the left sterno-cleido-mastoid muscle, but without beneficial result. It was in fact the right muscle which was contracted. She then went to an electrician, who used electricity without stint, but without success. When she came under my care she had been affected for about two months.

I gave her the bromide of zinc in gradually increasing doses, and applied the actual cautery over the belly of the contracted muscle. But after six applications, and a month's treatment with zinc, she was no better. I then cut the muscle. Relief was instantaneous and permanent, but having in view the case just related, I refrain from claiming too much for the operation.

Hysterical contraction of the muscles of the tongue I have never witnessed, though cases are reported by several authors.

2. *Hysterical contractions of the muscles of the upper extremities.*

There is perhaps scarcely a physician present who has not witnessed examples of contractions in the muscles of the shoulder, arm, fore-arm, or hand, due to hysteria. The muscle most frequently affected is, according to my experience, the biceps, and after that the superficial and deep flexors of the fingers. One case of contraction of the latissimus dorsi, and two of the flexor carpi ulnaris, have been under my care.

The following cases present some interesting features:

Case IX. Miss S., aged 16, was suddenly seized with a spasm of the fingers of the right hand, so that they were tightly closed upon the palm, and could only be opened by strong extraneous force. I saw her the following day, and advised the use of the bromide of zinc. After two weeks, as there was no improvement, her father consulted a physician, who diagnosticated disease of the palmar fascia, and recommended an operation and forcible extension. The fascia was cut, and an apparatus was applied, by which the fingers were kept extended; but as soon as it was removed, the contraction again ensued, and, owing to the pain it caused, it could only be worn for a small portion of each day.

Again she came under my charge, after she had discarded

the apparatus, and with her fingers, if anything, more contracted than when I first saw her.

I now examined her very carefully. It was possible, with minute and gradually applied force, to straighten the hand; but as soon as the fingers were left to themselves, they slowly but steadily contracted, till the ends touched the palm. The thumb was not involved. Faradization of the extensors also overcame the contraction for the time being. I was satisfied that the muscles affected were the two flexors of the fingers; and I resolved to see what could be done with electricity. I therefore daily applied the continuous current to the contracted muscles, and the faradaic to the opposing extensors; but though I persevered for two weeks, there was no apparent curative result. Finally, I determined to try the bromides in large doses, and accordingly prepared a solution containing two grains of the bromide of zinc and thirty of the bromide of sodium to the dose, taken thrice daily. In a few days the bromism became well marked, and then the spasm began to relax. I continued the treatment till she could scarcely stand, keep awake, or articulate a sentence distinctly, and by that time the contraction no longer existed, and she could voluntarily extend the fingers. The medicines were then suspended, and there was no return of the trouble. In all, nearly four months had elapsed since she had first come under my observation.

CASE X. Miss S. I., aged 22, had a severe hysterical paroxysm, from chagrin at not passing an examination for the position of teacher in one of the public schools. During the attack, the flexors of the fore-arm, wrist and fingers contracted to their fullest extent. When I saw her, a month subsequently, she had been treated for spinal meningitis, by blisters and the actual cautery to the spine, and internal administration of iodide of potassium, without the least beneficial result. The elbow and wrist joints were strongly flexed, and the fingers were so closely in contact with the palm of the hand as to cause soreness, so that cotton wadding had to be kept between the two surfaces, in order to obviate, as far as possible, the effects of the continued pressure. The appearance of the limb was very much like that produced in some cases of cerebral hemorrhage, in which secondary degeneration of the spinal cord has taken place.

In this case, I recommended the administration of the bromides of zinc and sodium, to the extent of producing extreme bromism. Four weeks afterwards I saw her again. She was still under the influence of the medicines, and her muscles were so far relaxed that she could move the limb in all its parts, with almost as much ease as ever. I advised the continuance of the treatment for ten days longer, and then heard that she was perfectly well.

The other cases of hysterical contraction of the muscles of the upper extremities, that have come under my notice, present no remarkable features, so that I do not enter into detailed description of them. Suffice it to say that they all—nine in number—recovered, four spontaneously, and five after treatment, as described, with the bromides of zinc and sodium.

3. *Hysterical contraction of the muscles of the lower extremities.*

The cases belonging to this category are probably more numerous than those of any other. The muscles most frequently affected are those which flex the leg, or the thigh, and the foot on the leg, the latter giving rise to talipes equinus, and both sometimes leading to serious errors of diagnosis. Brodie, Barwell, Skey, Meyer, and others, have cited many cases in illustration of this latter fact; and, doubtless, some present here this evening have met with like instances.

Of the cases of hysterical contractions affecting the parts in question, which have come under my notice, the two following possess special points of interest:

CASE XI. Miss X., a young lady aged 15, was brought to me for examination, in order that I might refer her to a competent surgeon for such special treatment as she might require. For two years she had been under the care of a surgeon who had diagnosticated her case as one of white swelling of the knee joint, and had treated her by such means as are usually employed in that disease. For a long time past she had worn a steel splint, placed on the posterior face of the limb, and bandaged around so as to prevent all motion of the knee. As a consequence of the disuse and almost continuous pressure, there had been considerable atrophy of the muscles of the thigh and leg. There was slight contraction of the

flexors of the leg, and the atrophy gave the appearance of enlargement of the knee joint, which, however, I was satisfied, did not in reality exist. Although the patient declared that she could not straighten the limb, and that every effort to do so gave her great pain, I seized the leg with one hand and the thigh with the other, and forcibly extended the knee joint. There was no great degree of pain produced, and though the flexed position was again assumed, I prohibited the further use of the splint, confident that proper treatment would restore the limb to its normal condition. Faradization was used for the atrophied muscles, the joint was repeatedly flexed during the day by passive motion, the muscles were kneaded for half an hour, morning and night, and she was encouraged to use the member, by walking on it and flexing it by voluntary power. In addition, the bromide of sodium was administered in full doses. These means were entirely successful, and the patient now—a year subsequently—not only walks well, but can waltz with entire ease. She still has a little weakness in ascending or descending a staircase, but this is disappearing.

This young lady is one of those whose cases I have mentioned, as now under treatment for hysterical contraction of the muscles of the lower jaw.

CASE XII. I was requested, in September of the present year, to go to New Jersey to visit Miss R., aged 14, in consultation with Dr. Norton. I found her lying in bed, to which she had been continually confined for several weeks— both heels elevated to the fullest possible extent by the contraction of the gastrocnemii and solei muscles. For a considerable period before she was confined to bed she had walked about the house with both feet in the extreme position of talipes equinus. A surgeon who had previously seen the patient, had insisted on the necessity of dividing the tendo Achillis of each leg in order to get the heel down.

Strong efforts at flexion failed entirely to relax the contracted muscles, but before resorting to tenotomy I wished to try the efficacy of the bromide of sodium in relaxing the spasm of the muscles, and as there were involuntary twitchings of the non-contracted muscles, I thought it advisable to combine with it the fluid extract of ergot in full doses. A month af-

terwards Dr. Norton wrote me that under the use of these remedies the contracted muscles had begun to relax, and that the heels could readily be brought to the ground. There is, therefore, strong reason for expecting a cure in this case.

4. *Hysterical contractions of the organic muscles and of the sphincters.*

Under this head are embraced cases of spasmodic stricture of the œsophagus, and the urethra; gastralgia and colic from contraction of the stomach and intestines, and tonic spasm of the sphincters of the vagina, rectum and bladder.

While it is certainly true that all these forms of muscular spasm may exist independently of the strictly defined hysterical diathesis, it is equally the fact that they are rarely found unassociated with great nervous impressibility, and the preponderance of what may be called the emotional temperament.

Of spasmodic stricture of the œsophagus a number of cases, both in males and females, have come under my notice, and in all recovery has taken place, sometimes apparently as the result of medication with the bromides, at others spontaneously on the occurrence of some strong degree of emotional excitement. In all of the cases—so far as I can recollect, ten in number—the affection came on suddenly as the result of emotional disturbance—fright, anxiety, etc., and in all, there were other evidences of hysteria present. In some cases it was necessary for a time to feed the patients through a stomach tube, as the caliber of the œsophagus was so reduced as not readily to admit of the passage of even very small masses of food.

Spasmodic stricture of the urethra, the result of emotional excitement, and accompanied by other hysterical phenomena, is so common a condition as scarcely to have escaped the attention of any physician or surgeon in ordinary practice.

Tonic contraction of the stomach, as a manifestation of hysteria and giving rise to severe cramping pains, is a condition not often met with, but of which two examples have come under my notice. In neither of these was there vomiting or nausea. Both occured in women, and both were cured by the free administration of the bromide of sodium—or at

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least the contraction ceased while the patients were taking this medicine. The diagnosis was not a matter of difficulty as the hard contracted organ could be distinctly felt through the abdominal parietes, especially when it was distended by the small quantity of food it was capable of containing.

Hysterical contraction of the muscular coat of the intestines, producing great pain and nervous disturbance, is so common, that it is scarcely necessary to do more than refer to the condition. There are very few cases of hysteria in which it does not, at some time or other, form a notable feature.

I have never had under my own immediate observation a case of hysterical contraction of the vagina, but such cases are, as I understand from several of my gynecological friends, occasionally met with, and Briquet[*] states that he has repeatedly seen cases in which the contraction of the sphincter vaginæ was so great through hysteria as to be distinctly felt when the attempt was made to introduce the finger into the canal.

Hysterical retention of the urine from spasms of the sphincter vesicæ, and constipation from tonic contraction of the sphincter ani are cases of common occurrence.

5.   Another form of hysterical muscular contraction is that which makes its appearance under the form of tumors, several instances of which have come under my notice, in which operations have been recommended. These are generally met with in the flat muscles of the abdomen and are not, as a rule, accompanied with pain. I have however seen them in the glutei muscles, in the latssiimus dorsi, the pectoralis major and in the platysma myoides.

The persistence with which these contractions sometimes remain, when not subjected to treatment, is remarkable. They never, however, in my experience, withstand the action of the bromides carried to their fullest extent, as is sometimes necessary.

The difficulties of making a correct diagnosis are not very great, if attention be paid to the clinical history of the case. It will generally, if not invariably, be found that there are other indications of the hysterical condition present, and in

[*]Traité clinique et thérapeutique de l'hysterie, Paris, 1859, p. 326.

some instances, similar " phantom tumors" have appeared and disappeared, from time to time. The suddenness with which they develop to a large size is also a diagnostic mark of value.

That cases such as I have described are hysterical in character, will not, I suppose, be doubted. At the same time, it must be admitted that applying to them this designation does little or nothing towards explaining their real nature. That there is a central lesion, slight though it may be, in all of these, is a matter of strong probability; for in some cases, in which the contraction has become permanent, the lateral columns of the cord have been found to be seriously involved. But there is just room for the hypothesis that the spinal disease may have been the consequence instead of the cause of the muscular contraction, and that this latter is therefore the primary trouble. Though I cannot think this view tenable, it is thrown out by no less an authority than Duchenne. There is, no more reason for accepting it, than there is for regarding every other affection of nerves and muscles involving sensibility or motility in which the spinal cord is involved, as of eccentric origin.

It would be arguing in a circle to assume that in these cases in which recovery takes place, there is no central lesion, while in those which persist, the lateral columns of the cord are affected.

While I think the bromides are shown to be efficacious in relaxing hysterically contracted muscles, I am not disposed, in view of the fact that spontaneous recovery sometimes so unexpectedly takes place, to insist strongly in their curative virtues. I am willing to leave this point to the individual experience of those who may have to treat similar cases. My main object has been to call renewed attention to a class of disorders liable to be mistaken for others of more serious import, and incidentally to the fact that recovery is frequently the apparent result of emotional disturbance. To this latter circumstance we are justified in attributing the cures produced by so-called saintly influence by Our Lady of Lourdes, the zouave Jacob and the army of mesmerizers, magnetizers, magicians, spiritual mediums, quacks, etc., who infest to a greater or less extent every inhabited country of the globe.

Art. IV.—ACTION OF LOBELINA.

By Isaac Ott, M. D.

SOME time ago I published some experiments on the action of this alkaloid of Lobelia inflata on lower animals. At that time I did not show that it was the active principle on which lobelia depends for its properties.

Baratlier,* in experimenting with ten to forty drops of a tincture of lobelia, obtained the following results: soon after taking it there was burning in the throat, eructation, nausea with widening of the pupil, disposition to sleep, difficulty in breathing, oppression over the chest, and in large doses tumultuous action of the heart, later sinking of the pulse, and after a few hours colic and fluid stools.

With the assistance of Dr. J. B. Heller, I made on myself the following experiment:

Time.	Pulse.	Respiration.	Temperalnre by the month.
2.23 p.m.	74	17	99
2.25 "	acetate of lobelina, 1-192 M. by the stomach.		
2.26 "	73	19	99 Tickling and dryness in the throat.
2.31 "	70	17	99
2.37 "	69	17	99½
2.41 "	68	18	99½; pupils dilated; uncomfortable feeling in the stomach.
2.46 "	68	17	99½
2.54 "	72	18	99¼
2.56 "	1-192 M. of lobelina.		
3.04 "	74	19	99¼
3.05 "	lobelina. 1-192 M. retching ensues.		
3.08 "	76	18	99¼
3.10 "	lobelina. 1-192 M. vomiting follows.		

* Baudlin, Gifte und Gegengifte.

Time.	Pulse.	Respiration.	Temperature by the month.
3.13 P.M.	77	18	$99\frac{1}{4}$
3.20 "	72	20	$99\frac{1}{4}$
3.27 "	70	18	99
3.44 "	65	19	$99\frac{1}{4}$
3.58 "	68	18	$99\frac{1}{4}$
4.12 "		retching, vomiting, prostration, headache, and slight disposition to sleep.	

As in the lower animals, it at first increases the respiratory action and temperature. It also reduces the heart beat, which subsequently exceeds the normal, and then recedes it nine beats below the normal.

Like lobelia, it gives rise to nausea, emesis, prostration, dilatation of pupils, with disposition to sleep. Of course large doses would have a different effect.

This experiment demonstrates that the active principle of Lobelia inflata is lobelina, and that probably it is the only active principle.

Art. V.—MITRAL STENOSIS; CEREBRAL EMBOLISM; LEFT HEMIPLEGIA, CEREBRAL SOFTENING, DEATH.

By M. A. Wilson, M. D., New York.

[*Read October 27th,* 1876, *Before the Neurological Society, N. Y.*]

MEDICAL literature is rapidly becoming replete with cases which may be included under the above heading, and the subjects of "Embolism" and "Thrombosis" are ones with which the profession are much more familar at the present time, than even but a very few years ago.

It is within the recollection of the majority of physicians, when all cases of sudden or unexpected death, were considered as due to "heart disease" or "apoplexy" alone; cerebral or

pulmonary emboli, thrombosis, heart-clot, etc., never being mentioned nor probably suspected.

With the object of increasing the number of recorded similar cases, and assisting what little it may in the symptomatology and treatment, I report the following case, written in my note-book in 1874–'5. It is also mentioned in the last edition of Dr. Hammond's work on nervous diseases, page 130.

The patient being a near relative, I felt (as most physicians do) some little embarrassment in treating her exclusively myself, and therefore asked the counsel and assistance of several most eminent practitioners in the city.

Miss V. B. W., aged 20, naturally strong and healthy, inheriting no affections whatever. Eleven years ago (1863), she had a very severe attack of acute articular rheumatism, and five years ago (1869), one of acute pleurisy, right side, with slight effusion, since which time *there has been no return* of either affection.

No symptoms referable to cardiac disease appeared until early in 1872, nearly three years after, nine years after the rheumatism, during all of which time her health was exceedingly good.

Since that date (1872), she suffered occasionally from dyspnœa, cough, difficulty in walking at all fast, especially in wintry weather, or in ascending stairways; also within the past year, she has frequently, after retiring, been awakened from a sound sleep, owing to severe dyspnœa, pain, etc., and for an hour or two following, she would not be able to again assume the recumbent position.

Previous to December, 1873, she was not a resident of this city, and was only treated from that date until February 22d, 1875, the date of her death.

The heart's impulse was strong, well-marked, the organ somewhat hypertrophied.

A loud, pre-systolic murmur could be distinctly heard at the apex, indicating mitral obstruction—mitral stenosis—as, also, an aortic murmur, considered as being due to anæmia.

While in the country during summer of 1874, these attacks of dyspnœa were frequent, but became *very* much less so when

she returned to the city in September; her general health at this time was excellent; pulse 80, appetite good, felt and slept well, menstruation and bowels regular.

The treatment, previous to time of the occurrence of cerebral embolism, consisted of the various preparations of iron, "chemical food" (earthy phosphates) quinine, strychnia, sherry, porter; in brief, tonics and nourishing food in general, outdoor exercise, etc., and with the best of results as just above mentioned; the cardiac difficulty was also much improved, as evidenced by the entire absence of palpitation, dyspnœa, pain and cough.

On Wednesday, Sept. 29th, 1874, she complained of headache, seemed much depressed, and remarked that she felt badly. The weather being fine, she called about 3 p. m. upon a lady acquaintance in the upper portion of the city, reaching her residence by street cars. This friend living on the fourth floor of a "French flat," our patient (her first visit there) ascended in an elevator on the wrong side of the house, so had to walk through to the other side and down one flight of stairs to her friend's apartments, when, just as she had reached the door, she suddenly, without further premonition, whatever, fell to the floor, not however losing consciousness.

This was followed almost immediately by a *very* severe, dull, heavy pain in the *right* temporal region, accompanied with vertigo but no nausea or vomiting; this pain, I will state *en passant*, did not subside for some eighteen hours afterwards. I was at once sent for and discovered that complete hemiplegia of the left side had occurred, including the face and tongue. She was entirely conscious, and yet seemed slightly "dazed" or stupid; endeavored to talk but could not; great difficulty in articulation, but no aphasia; the effort to do so seemed apparently so ridiculous when she found she could not, that she tried to laugh, and this being unsuccessful it caused her to cry.

I will again, in passing state that her cheerfulness and liveliness of disposition was a marked feature during her entire illness; there was a slight dilatation of the left pupil, but the iris was perfectly sensitive to light; a numbness and coldness of both extremities—left side; temperature not taken; pulse 100; such were the only symptoms observed at the time.

Within a very few hours, the slight dullness or cloudiness of intellect had passed away, and neither at the time of this attack, nor at *any* time during the whole period of her illness (excepting the few hours before her death) was there any impairment of the mental faculties. The facial paralysis and difficulty in articulation completely disappeared during the first twenty-four hours. On the evening of the fourth day, she was removed to her own home, and for the next five weeks the treatment consisted in the administration of Vallets mass, grs. xx., a bottle of good porter, and at least a ¼lb. of animal food daily; the result was, her general health became exceedingly good in all respects.

The necessity for artificial warmth subsided within the first ten days after the attack; in fact, it *never was* required, except in the left limb, below the knee. After the third week, involuntary contractions or movements took place frequently in the left foot, not above the ankle, pain being felt in the joint at such times. Twice after the fourth week, the *left upper extremity* was *involuntarily* raised from her side at night during sleep, and *placed over her head*.

Friction of left shoulder caused pain, and pain of severe character was experienced occasionally at different parts of the left leg and arm. No friction was employed until about the middle of October.

On the evening of October 28th, she was suddenly seized with *very* severe pain in the *right* arm, a little above the wrist-joint (radial artery); this was soon followed by a sensation of numbness and coldness of the right hand, the pulse lost at that point, but no loss of motion.

The next morning all 'pain had ceased, the pulse at both wrists was the same, and within forty-eight hours all abnormal sensations had vanished. For the first two weeks the pulse ranged between 56 and 70; after this period, between 70 and 80; the temperature at all times, normal.

Nov. 9th. Began the use, hypodermically, of strychnia sulph. gr. 1-48 once daily.

Nov. 10th. The faradic current employed once a day, the muscles of both left extremities responded more or less promptly.

Nov. 20th. Substituted the elixir of Calisaya, containing quinine and pyro-phosphate of iron for the pills of Vallets mass; all the organic functions seem perfectly normal, no improvement whatever in the paralysis.

Nov. 24th. Strychnia increased to gr. 1-32, once daily.

Nov. 25th. Commenced the "movement cure," as conducted by Dr. C. F. Taylor, once a day. Within the past ten days the patient has suffered from severe pains in the region of the spleen, heart and lower right chest, but neither in the head nor extremities, although occasionally head-ache; full doses of morphia and chloral were found to be absolutely necessary.

Just at this time there was loss of appetite, constipation, sleeplessness; the elixir was changed to bitter wine of iron; no improvement in the paralysis; carbonate of ammonia, 10 grains daily, was now suggested, in order to dissolve or prevent further formation of blood or fibrinous clots, according to the views of Dr. Richardson, of London; but as it was feared it might create gastric disturbance without being beneficial, it was never administered.

Up to Dec. 1st, the case had been seen by Drs. Loomis, Polk, Hammond, S. W. Roof, S. B. Ward, and R. Taylor.

Dec. 2d. Dr. A. D. Rockwell was invited in consultation, and advised "central faradization," in conjunction with the above-mentioned current, to be employed very mildly, not specially selecting the "motor points" for the purpose of producing contractions, but merely for the beneficial effect of a gentle current through the muscles and other tissues.

Dec. 4th. The patient was sitting upon the edge of the bed, when I requested her to stand up if possible, unsupported; before attempting to do so, the left leg *below the knee*, was *involuntarily* extended.

Being, both surprised and pleased at this, she discovered, upon *trying* to do so immediately, that she could *voluntarily* extend the leg, she did so several times, until it caused pain. As just requested, she now stood upon both feet, bore her weight upon that side, and by being partially supported, walked a few steps, the *first* and *only* successful attempt since the occurrence of the hemiplegia, two months previously. She had noticed during the day that the whole lower extremity

felt much more natural, especially when the foot was placed upon the floor.

This took place immediately following the use of the battery.

Dec. 18*th*. The loss of appetite still continuing, "Horsford's Acid Phosphate," a teaspoonful before meals, has been administered for the past few days, with but slight benefit; constipation more or less troublesome.

Strychnia has not been given in any form for the past week, as it produced severe local irritation hypodermically, and an interval of non-employment entirely was considered advisable.

Dec. 19*th*. Since the 4th instant, the condition of left lower extremity has gradually improved; the patient bearing more weight upon it, walking more steadily, but not without assistance; complains every few days of sharp pain in the splenic region, as also lately in the *right* ankle-joint, these pains always subsiding in an hour or so; use of electricity being continued as indicated Dec. 2d.

December 20*th*. Strychnia resumed, gr. 1-32, internally.

January 10*th*, 1875. An obscure symptom or phenomenon of this most interesting case, occurred this evening. The patient suddenly (while endeavoring to do so), *voluntarily* moved, raised overhead, flexed, and extended, the left (paralyzed) fingers, hand, lower and upper arm. This she did when requested, and sufficiently often, to convince all present of its being *voluntarily* done; but the muscles becoming wearied, she soon ceased, and was *never afterwards* able to do so again.

January 23*d*. Began employment of the galvanic current; the top of the head being first moistened with salt water, the positive pole was first applied there for five minutes, then over the 6th cervical vertebra for the same length of time; the negative pole in the meantime being placed upon the epigastrium only. The faradic current administered but every other day now.

January 31*st*. With the exception of accidental intervals of a day or so, the strychnia has been continuously given internally since December 20th, and gradually increased during this month from gr. 1-32 once daily, to gr. 1-16 twice daily, without perceptible benefit.

It was now decided to administer it in rapidly increasing doses, until constitutional effects were manifested, the following being a very convenient prescription for this purpose: R. strychniae sulph., grs. iii, aquae dest. ʒi. M., S., 10 drops three times a day, increasing one drop at each dose daily, untill twitching or stiffness of the muscles of the calf, and nape of the neck were produced. Has been taking the bitter wine of iron constantly ever since commencing it.

Her appetite and general condition were now very good, varying slightly every few days; the mental faculties sound, undisturbed, spirits bright, hemiplegia just the same.

During this month (January), she suffered from two *very* severe attacks of dyspnœa, palpitation, cardiac pain and distress, and during the same, loud, moist rales could be distinctly heard upon auscultation of the chest.

Nothing was prescribed at these times, as the semi-recumbent position and absolute quiet occasioned their subsidence.

A prescription, which afforded much relief during previous similar attacks, is the following: R. tinct. valerian ʒss., tinct. digitalis ʒi, spts. lavand. co., spts. etheris co. aa. ʒss: M. S. A teaspoonful when necessary.

February 3d. This a. m. I found the patient standing by the wash-basin, holding on to it with her right hand (she never could use her left), gazing upon the floor with a vacant fixed look, and swaying to and fro as a person intoxicated. Asking her "what was the matter," she made no reply, did not notice me at all.

I hastily lifted her into bed, when she began to cry and sob bitterly; in doing so, the face was drawn entirely to the left side, this being *the only facial paralysis observed since the first day of her illness.*

She stated afterwards that she was unconscious of everything when standing by the wash-basin. She remained in bed all that day, perfectly conscious, but never speaking unless spoken to; had more or less headache, and sharp pain about the middle lobe of right lung.

These symptoms passed off toward that evening, and did not again return.

From the time of this peculiar state of unconsciousness un-

til her death, nineteen days afterwards, it was ever my impres-
sion that she was not the same person mentally, intellectually
"not herself at all;" others could not observe that this was
the case.

She seemed much more reserved, quiet, reticent, yet not
irritable; when talking (although perfectly sensible), she hesi-
tated—considered apparently what she was about to say.

February 5th. A spasmodic, tonic contraction of the ex-
tensor muscles of left leg and foot took place, the heel being
elevated, only the toe touching the floor, accompanied by a
stiffening sensation of the muscles at nape.

These symptoms subsided in a few minutes, and there was no
recurrence of the same until February 13th.

They were naturally enough attributed to the strychnia, of
which she was then taking fourteen drops three times daily, of
the prescription above-mentioned, although at the same time I
felt convinced that there was some brain lesion present (soft-
ening, for instance) other than the embolus alone.

The administration of the strychnia was temporarily sus-
pended, as was also the galvanic current; the faradic was still
continuously employed, one-half hour every other day. The
electro-muscular sensibility of both lower extremities about
equal, the left arm less sensitive than the right.

February 9th. Strychnia resumed, ten drops three times
daily, as also the galvanic current for but a few minutes once
daily.

The "Swedish-movement-cure" manipulations were again
begun at this date, having been discontinued for the previous
three weeks; operator to call twice weekly; the porter and
wine of iron still continued.

February 13th. Since last note, (February 9th) tonic con-
tractions of extensor muscles, left leg and foot became quite
frequent, usually when attempting to walk.

As the strychnia had not been given in excessive doses, it was
discontinued entirely for four days, the contractions occurring
notwithstanding. (Could they not have been dependent upon
cerebral irritation at the point of softening?)

The patient becoming exceedingly weary of the house, to
which she had been so long confined, with a great desire "to

have a ride," and the weather being very pleasant, she was very carefully driven to Brooklyn, to visit an intimate friend for a few days, all treatment being suspended except the iron and porter.

February 15*th.* She returned to her home cheerful and bright, apparently much benefited by the trip, but suffered severely while there from vesical irritation; incontinence, with great frequency of micturition and ardor urinae; pulse and temperature normal; had taken a mixture of belladonna, hyoscyamus, and sweet spirits of nitre, together with hot fomentations to hypogastrium, with some benefit. Spasms of extensors above referred to, had occurred once while absent.

February 18*th.* Return of vesical irritability more severely than before; no pain, uneasiness, nor retention; ardor urinae always *after* urination; no spasm apparently of the sphincter vesicae; a pill of belladonna and conium given every four hours, with citrate of potash half-a-drachm every two hours; flax seed tea *ad libitum.*

At various times during her illness, the urine was examined chemically and microscopically, with negative results, excepting the presence occasionally of crystals of the stellar phosphate of lime.

The strychnia was discontinued at the time of her visit to Brooklyn, and was never again resumed.

After her return the galvanic current was again employed daily, the faradic every other day; iron and porter, as usual. This patient died on February 22d, at 3 o'clock, A. M., the peculiar, sad, painful termination of her illness being as follows:

Feb. 20*th.* Saturday; she had risen and breakfasted as usual; was in "good spirits," the vesical irritability having almost entirely passed away.

About 11 o'clock A. M. she was standing by the mantel-piece, no one in the room except her little niece, who noticed that she was leaning over and forward in a very strange manner, as if about to fall. Assisting her with great difficulty to the bedside, the patient began to cry in a loud and distressing manner, but shortly becoming more composed, she complained of a *very* acute, sharp pain in the *left* temporal region, as also of

pains in the upper portion of the body at different points.
The crying was soon *again* renewed as before, and vomiting
of the ordinary contents of the stomach occurred once. While
now lying down, the crying continuing, the mouth and cheek
were drawn sharply to the *left* side, the lower jaw held firmly
and widely open, she endeavoring all the time to close it, as if
she feared its disarticulation. Asking her if she felt easier
when trying to elevate it, she with great effort answered "yes."
Assisting her to do so, I found a condition of fixed, tonic con-
traction. She could not protrude the tongue beyond the lower
teeth; it seemed "tongue-tied." The *consciousness* of her
condition agitated her exceedingly, and her cries and sobs were
agonizing to hear. Three times she raised herself from the
pillow, was quiet for a few moments, then again screaming fell
back with facial, as also with clonic contractions and tremors
of all the extremities. Pulse 200, respiration 30. Dr. Polk
arriving, at once employed hypodermically ten drops of tinct.
digitalis, which quieted the heart very much within five min-
utes. Again lying back quietly, we left her for a moment,
with her older sister, to consult in an adjoining room, but
were immediately recalled, and found her in a semi-unconscious
state. The jaws were tightly closed upon the tip of her tongue;
the eyes fixed and staring, without strabismus; the face and
forehead becoming rapidly swollen, assuming a reddish, livid hue,
as seen in apoplexy; all the extremities were extended, with
their muscles firm and rigid; the fingers of the left hand
tightly *flexed*, with the palm turned *outwards;* the left foot
strongly *extended*, with the sole turned *inwards.* Another
injection of digitalis, same quantity, was given, and an enema
of turpentine and hot water ordered, which, however, she
never received, as, during its preparation, she became worse
so rapidly it was deemed useless. Cold applications were
made to the head; mustard poultices and bottles of hot water
to the lower extremities, which were now cold; pulse 180,
respiration 30, stertorous and labored; slight frothing at the
mouth; jaws still closed; coma deepening.

She was *somewhat* conscious until about 4 P. M. next day,
Sunday, Feb. 21st, *but absolutely speechless and motionless,*
with the exception of the eyes; the lids were opened and

closed; the eye-balls moved and rolled naturally when looking at any one directly in front of her; the expression of the eyes intelligent but painfully anxious. Once upon Saturday afternoon, when her dear lady friend from Brooklyn stood before her, tears were seen to flow, and an effort made to cry, but only momentarily. With her head constantly inclined to the right side there was not the slightest movement, voluntarily, of any portion of the body; slight tremors and contractions taking place when her face was bathed, or hands grasped, especially the left. Voluntary deglutition was totally abolished; involuntarily, however, from purely reflex action occasioned by its presence when much had accumulated in the pharynx, and many attempts had been made, saliva would be swallowed.

The jaws remained firmly closed until 2 A. M. Sunday, 21st, after which time she breathed with the mouth open; the tongue fell far backward, and became very dry. From this time she was seemingly comfortable, the coma having gradually become more profound. The pulse ranged between 130 and 150, the respiration from 24 to 30, until about noon of Sunday, when the character of the respiration changed, consisting of a loud, sudden, jerking inspiration, followed by *two* short expirations, the second much the longest.

Towards 3 p. m. (Sunday) the expression and appearance of the features and face became gradually more ghastly and death-like, the respiration less frequent and labored. After 10 p. m. the eye-balls were rolled upwards, the cheeks much sunken, mouth open, pulse 130—death closing the scene and releasing the sufferer at 3 a. m., Feb. 22d, 1875.

The treatment during the last two days consisted in the occasional hypodermic injection of tinct. digitalis, as also brandy, and Magendies solution of morphia; enemata of beef tea alternating with sweet oil.

On Sunday, Feb. 21st, a blister was applied to the back of the neck, which "drew" well, without other effect; also two drops of croton oil given, which failed to act. In fact any other or further treatment was considered useless and unnecessary.

POST-MORTEM.

The post-mortem was held 36 hours after death, in the presence of Drs. Hammond, Polk, Rockwell, Roof, R. Taylor and myself, Dr. T. M. B. Cross conducting it. Rigor mortis well marked. The brain alone was examined. The right middle cerebral artery was found to be occluded by a firm, hard, whitish or transparent fibrinous clot, about the size of a grain of wheat; the right corpus striatum was completely broken down, softened, disorganized. All other portions of the brain were in a perfectly normal condition as far as could be ascertained at that time. Both the *right* and left optic thalami, left corpus striatum, pons, medulla, ventricles, cerebellum and lobes of the cerebrum were examined. No hemorrhage, serous effusion, nor other emboli were found in any portion of the encephalon.

REMARKS.

Just before the post-mortem was held there were several opinions expressed as to the special lesion causing death. As before mentioned, the symptoms (of final attack) much resembled those of cerebral apoplexy, and I believe it was generally considered that hemorrhage into the softened portion had taken place, as also at the base of the brain; involving probably the pons varolii, owing to the difficult, interrupted respiration, loss of power of deglutition, rapidly fatal termination, the copious sweating of head and face, although the pupils were not abnormally contracted. As evidenced by the postmortem, however, this was not the case. Again, there are cases on record where such hemorrhage *has* occurred, years before death, without such symptoms being present as are laid down by Niemeyer, and concurred in by Reynolds. One physician present much doubted the existence of any extravasation, but felt convinced that death ensued from the further lodgement of one or more emboli into larger arteries, probably a vessel supplying chiefly the pons. As before said, this was not proven to have been the case at the post-mortem.

Among many others the above case presents the following interesting features:

I. No loss of consciousness. This may or may not occur in cerebral embolism, and is considered at the present time as

a symptom scarcely to be entertained in a diagnostic point of view.

II.   Dull, heavy pain in the right temporal region—the side on which the embolus was lodged.

III.   Emotional condition of the patient. This condition is frequently observed in affections of the right hemisphere.

IV.   Involuntary movements of the paralyzed side. These movements *not* being associated with movements of the healthy side, must be considered as the so-called "automatic movements" observed as occurring in cerebral hemorrhage and other cerebral affections.

V.   Embolism of the right radial artery.

VI.   Sharp pain in splenic region, subsiding in an hour or two, and probably due to embolism of the spleen.

VII.   On Feb. 3d, 5th, 9th and 20th, spasmodic movements were of frequent occurrence.

VIII. On Feb. 3d, there was "drawing" of the face, to the left side, for a few moments, and after this attack, marked hesitation of speech, the face resuming its previous appearance.

IX.   On Feb. 5th, spasm of the paralyzed limbs, which subsided in a few moments; but from this time on recurring frequently, when the patient attempted to walk. Condition of the face not noticed.

X.   On Feb. 20th, severe pain in the left temporal region—vomited once—mouth and cheek drawn to left side, inability to protrude the tongue, general clonic convulsions, followed by general tonic spasm and death.

In explanation of the above, we know that spasmodic movements are apt to take place in affections of the cortex, pons varolii, and the white substance of the right hemisphere, just outside the opto-striate body. It is very probable, then, that the spasmodic movements of the face, on Feb. 3d, and of the limbs on Feb. 5th, were due to embolism of certain vessels supplying the cortex; and that the clonic and tonic convulsions, that ushered in death, were the result of multiple embolisms. The absence of points of softening does not militate against this view; since softening could not be established from the time of this attack (Feb. 20th), until her death, two days after-

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wards. Again, the emboli could only be discovered after much time and labor, which was not given.

This same explanation may be offered for the attacks of Feb. 3d and 5th, at which times the spasmodic movements were more circumscribed; and, if softening *did* occur after these attacks, it was probably overlooked, or collateral circulation was established; that is, if we adopt the views of Heubner, who believes the anastomoses of vessels of the pia mater supplying the cortex, to be very free, as opposed to the vessels supplying the basal ganglia.

We may mention, *en passant*, that the researches of Duret controvert this; as he has shown that the arteries of the cortex, like those of the base, do not anastomose, and are terminal.

In explanation of the spasmodic movements in walking, the strychnia, and the site of the softened patch in the right hemisphere are probably sufficient. Finally, we will only add, that for such cases as these, there is no relief, no cure; for, owing to the anatomical distribution of these vessels (at base of the brain), being terminal, without anastomoses, recovery is impossible; there can be no collateral circulation established.

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Art. VI.—EXPERIMENTAL AND CLINICAL INVESTIGATIONS ON CERVICAL PARAPLEGIA.

By Prof. Dr. M. Rosenthal.

While typical paraplegia of the lower extremities has long been a common subject of medical observation, as much cannot be said of that rarer paraplegic affection, having its seat in the upper limbs, and designated by Gull,[*] as cervical para-

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[*] *Guy's Hosp. Rep.*, IV. 185.

plegia. The different anatomical conditions, the varying clinical features, as well as the peculiarities in regard to diagnosis, render this group of paralyses especially interesting, and call for further research and observation.

Inasmuch as the medical literature is very scanty concerning isolated section of the anterior cord, especially in the cervical region, as far as my knowledge extends, it seemed to me not superfluous to communicate the most important results of my own experiments. To these experimental data, the results of clinical observations can be added with advantage.

A. *Experimental production of paralysis of the fore limbs.*

The section, or partial section of the anterior cord in the cervical region, was performed on frogs. The experiments were twenty-five in number. If we take from a winter frog (*Rana esculenta*) a piece of the cord at the upper limit of the cervical enlargement, from the posterior columns forward, and from one side to the other, the animal exhibits a distorted position of the head, and paralysis of both fore limbs, which are directed inwards, are very slightly irritable to mechanical or electrical irritation, and serve only as immovable supports to the animal. In many cases, the hinder limbs also appear paretic. Besides this, the cutaneous sensibility is notably diminished above the wound, on the head.

Within the next 8 to 14 days, a marked improvement in the motility of the anterior limbs appears, especially in the toes. After death, which occurs with in a certain time, microscopic examination of the hardened cord shows deposits of a hyaline exudation around the nerve tubes, and numerous pus corpuscles collected in the gray horns, or grouped star-wise around the central canal.

In larger animals the cervical cord, from the posterior columns to the most anterior portion, can likewise be wounded. In a rabbit thus operated upon, there immediately appeared dyspnœa and a high degree of hyperæsthesia, with paralysis of both anterior limbs and the right posterior one. Slight pinching of the tail or the members, or electric irritation of the latter, caused, together with outcries on the part of the animal, powerful movements of contraction and

extension of the hind limbs, while the anterior ones remained
perfectly immovable, even upon direct mechanical irritation.
The autopsy showed perfect division of the posterior columns,
and gray horns, as far as the anterior columns. Only a portion
of the latter and the anterior cornua were uninjured. The
hyperæsthesia was here dependent on the injury to the pos-
terior columns, according to Woroschiloff, on the extension of
the cut into the middle fibres of the lateral columns.

The anterior portion of the cord can be reached in rabbits by
two kinds of operative procedure: either from the anterior or
the posterior aspect of the cervical vertebræ. The incision, or ex-
cision of the cervical cord, from the front, can be managed in
different ways. The easiest, but, at the same time, the most
uncertain way, is to introduce the knife into an intervertebral
space, laid bare for a little way, and cut forward. The usual
result of this operation is paralysis of the corresponding fore
limb, and paresis of the members, which latter disappears in
a few days. The persistent paralysis of the anterior extremity
is, as the post-mortem shows, due to a predominant lesion of
the cord, on the side of the cut; the other half is only slightly,
if at all, affected.

In order to make the cut of the anterior aspect of the cord
more advantageously, the spinal canal must first be opened an-
teriorly. Still, this operation is not less tedious than danger-
ous, on account of the unavoidably severe hemorrhage, no
matter whether the bone is pared away with a knife, or bored
through and pinched off. If only one-half of the anterior cervical
cord is operated upon, there follows paralysis of the corres-
ponding fore and hinder limbs; if the injury still extends over
the middle line into the two lateral columns, then we have
paralysis of both fore limbs, generally with paresis of one pos-
terior extremity. Later attempts at motion, on the part of the
animal, lead to the conclusion that one fore limb is more par-
alyzed than the other, and that the extensors are specially
affected, the flexors suffering very little. (A similar condition
of things, as will be shown further on, is observed in cervical
paraplegia of the human subject.)

In most cases the rabbits began to suffer from an intense
dyspnœa shortly after the operation, soon followed by tetanic

cramps of the members. In the course of the same or the following day epileptiform convulsions set in. which phenomena (analagous to those sometimes observed in man) confirm during life the diagnosis of spinal apoplexy. The animal dies within 24 to 36 hours.

The autopsy generally reveals extensive meningeal extravasation, which extends downward on the dorsal cord and upwards on the medulla. Sometimes there is also an intra-medullary hemorrhage corresponding to the point of lesion.

A less difficult and dangerous operation is that from the hinder region of the cervical vertebræ. After the removal of the spinous and transverse processes of the upper cervical vertebræ, an aneurysm needle is introduced laterally into the cord. the veins. etc.. being avoided, and then with a small knife. preferably one crooked toward the surface. an incision or excision of both halves on the anterior face of the cord is made.

Together with the above described paralysis of both anterior limbs. or of one hinder extremity also, the reflex power is also notably altered from the lesion of the gray matter. Thus I saw, in one animal thus operated upon, contractions of both posterior limbs follow electric irritation of one sciatic nerve, the fore limbs remaining meanwhile perfectly quiet. Irritation of one median nerve caused only contraction of the corresponding member. without exciting other contractions.

Examination of the hardened cord revealed a hemorrhage extending across both the white and gray substance in the cervical region. Only a narrow strip of the anterior portion of the latter was uninvolved. In lower sections the medullary substance becomes more and more free. both anteriorly and posteriorly. the hemorrhage becomes limited to the gray columns, and soon is only perceptible in the posterior column in the form of a fine point.

B. *Clinical observations on cervical paraplegia.*

In agreement with the experimental results. clinical observation shows that myelitic local lesions, extending downwards from the cervical region. involve first the upper and later generally the lower limbs in a paralysis. Sensibility and reflex action also suffer simultaneously more or less serious involvement. In caries of the cervical vertebræ, first one arm and

then the other suffers, paralysis, which, with its further exten-
sion, involves the legs also. Cases of this kind are met with
in the older authors, such as Brodie, Marshall Hall, Nichet,
Ollivier, Budd, and Schutzenberger. An acute myelitis of the
cervical cord, from wrenching or fracture of the vertebræ, may
also begin with paralysis. In a lately described case of
Michaud* (gunshot wound of the anterior cervical region),
with resulting paralysis of both arms, stiffness of neck, diffi-
culty of swallowing, and final paralysis of the legs, the autopsy
revealed a fracture of the fifth, sixth and seventh cervical
vertebræ, compression of the cervico-brachial enlargement by
a splinter, with corresponding softening of the anterior portion
of the cord and extension of the incipient myelitis to the gray
matter and the posterior columns. The anterior roots as well
as the brachial nerves showed neuritic alterations (fatty gran-
ular degeneration).

Finally, paralysis and contraction of the upper members
may be induced by compression of the cord at the level of the
upper dorsal vertebræ from caries, and, according to Charcot
and Michaud, by sclerosis of the lateral columns extending up
to the cervical region. Louis has reported a case of this kind†
without, as he himself admits, being able to explain the
anomaly.

That the region of origin of the brachial nerves extends from
the fifth cervical to the third dorsal vertebra, is also indicated
by the fact that paralysis of the arms and dorsal regions belong
to the initial symptoms of circumscribed cervical myelitis.
The frequent partial loss of sensibility is also due to this slight
extension of the spinal lesion, as is likewise observed in the
experimental wounding of the cervical cord. In a very re-
cently described case of spinal lesion at the level of the seventh
cervical vertebra, by Hutchinson,‡ there was during life, be-
sides constriction of the pupil and diaphragmatic respiration,
also imperfect paralysis of the upper extremities. The triceps,
the extensors and the pronators were paralyzed, the flexors
merely weakened, the deltoids, biceps and pectoral muscles on

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*Sur la meningite et la myelite dans le mal vertebrale, 1871, p. 58, 59.
†Sur l' etat de la moelle épiniere durs la carie vertébral, 1822.
‡Lancet, May 22 and 29, 1875.

the other hand performed their functions well. The patient died the seventh day with symptoms of dyspnœa and pneumonia.

With the cervical location of the lesion also appears diplegia of the arms and other symptoms of affection of the cervical cord. As such may be mentioned the oculo-pupillary alterations, more frequently paralytic myosis (contraction from compressive inhibition of conduction), than spastic mydriasis [dilatation from medullary irritation of the dilator pupillæ]. Here belongs also the slowing of the pulse by irritation of the endocardial vagus fibres observed in the already mentioned case of Hutchinson. In a case described by myself[*] as conclusive in its way as a vivisection, in a student, aged 25, after he had received a punctured wound in the neighborhood of the sixth cervical vertebra, there was hemiparesis of the right side, dilatation of the pupil, and variation of the pulse between 48 and 56 beats, for a period of four weeks. The early dyspnœic troubles are explained also by the involvement of the cervical and upper dorsal origins of the phrenic, cervical, and intercostal nerves. With perfect maintenance of the abdominal respiration the lesion cannot have reached the origin of the diaphragmatic nerves in the fourth cervical pair. Speech and swallowing are also frequently embarrassed. In conclusion we may mention the initial increase of bodily temperature and its subsequent rapid fall.

According to the latest experiments of Schroff[+] a decided rise of temperature follows section of the spinal cord in dogs. Still a similar phenomenon follows opening of the spinal canal without injury to the cord, also section of the cord at the level of the atlas with artificial respiration, the atmosphere of the room being at 18 ° or 20 °. C.

Together with the paralysis of both arms following pressure lesions, there is a second class of spinal paraplegias of the upper extremities. In these a cell atrophy in the anterior gray cornua which serve as origins for the roots of the nerves of the upper extremities is the anatomical substratum for the brachial diplegia. These cases, for the most part only lately

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[*] *Zeitschr f. prakt. Heilkunde*, No. 46, 1866.
[+] *Sitzungsb. der Weiner k. Akad*, Mar. 22, 1876.

investigated, are sometimes acute, sometimes chronic. Their
corresponding clinical symptoms show also special peculiarities.

Partial acute myelitis, affecting especially the upper extrem-
ities, has been already described by Ollivier (d'Angers),[1] in one
case. Here, also, are to be included those observations, of
more or less perfect cervical paraplegia, connected with tumors
in the gray substance of the cervical cord, where, together with
diffusion of the process toward the posterior columns and
cornua, the anterior cellular columns are especially involved.
Gull[2] reports the case of a child, in whom, with paralysis of
the right and left arm, the mobility of the legs was better pre-
served. The autopsy revealed the seat of a solitary tubercle, at
the level of the origin of the 6th and 7th cervical nerves.

With merely partial lesion of the cervical cord, the paralysis
is also only partial and unilateral, as in the old case of Eager,[3]
and in a more recent observation, by Virchow,[4] in which hy-
peræsthesia, motor paralysis, muscular atrophy and contrac-
tures were present in the left upper thorax and arm. At the
post-mortem was found a round tubercular mass, in the left
side, at the level of the 3d and 4th cervical vertebræ, eight milli-
metres in diameter, crowding the gray substance to the right.

It is one of the achievements of recent times, to have shown
that the anterior gray medullary columns contain groups of
polyclonal nerve cells, the degeneration and pigment atrophy of
which are connected with peculiar motor and trophic disorders
during life. One series of typical disease forms, and their
combination with bulbar or spinal paralysis, are here excluded.
It also follows, from the latest clinical and anatomical re-
searches of Prevost and David, Joffroy,[6] Erb,[7] and E.
Remak,[8] that the motor nuclei are arranged in stages, one over
the other, in the spinal cervical enlargement. So, according to
the height and extension of the lesion, the form and grade of
the cervical paraplegia suffer considerable alterations.

1 *Traite des Maladies de la Moelle epiniere*, 3d. ed., t. II., 1837, p. 319.
2 *Guy's Hosp. Reports*, 1858, p. 206.
3 *Gottschalk's Sammlung.* 1838, II., p. 65.
4 *Onkologie*, I., p. 656.
5 *Arch. d. Physiologie*, 1874, p. 595.
6 *De la Pachymening cervic. hypertr.* Paris, 1873, p. 87.
7 *Arch. f. Psychiatrie*, V. 1875.
8 *Arch. f. Psychiatrie*, VI. 1875.

In the acute form of cerebral myelitis, the inflammatory processes may become quickly diffused in the cornua, and in cervical lesions the upper members suffer. In a very recently reported case of Raymond,[1] of a previously healthy working man, 21 years of age, in whom, after a severe chill, fever, pain, numbness of the hand, extending upwards to the shoulder, were experienced, with rapidly occurring complete paralysis of the left, and imperfect paralysis, also, of the right upper extremity. On the left side the electro-muscular contractility was locally diminished. The patient died asphyxiated, in twelve days. The autopsy revealed nothing to macroscopic examination. Microscopic examination of the hardened cord, on the other hand, showed in the cervical region, inflammatory alterations of the cells of the anterior horns, with rarefaction and local disappearance of the same. In the connective tissue, there was marked muscular proliferation, and in the hinder part of the lateral columns, certain nerve fibres showed notable enlargement and thickening of the axis cylinder.

Infantile spinal paralysis also depends upon acute myelitic alterations of the anterior spinal tract, with consecutive proliferation and degeneration in the cell mosaic of the anterior columns, as well as in the corresponding roots and medullary cords, as is shown by the recent discoveries of Cornil.[2] Prevost and Vulpian,[3] Charcot and Joffroy,[4] Parrot,[5] Roger and Damaschino,[6] Recklinghausen,[7] myself,[8] and M. Roth.[9] Infantile spinal paralysis, which, as is well known, generally affects the upper or lower extremity, sometimes both lower members, or those of one side of the body, can, nevertheless, in exceptional cases, take on the form of cervical paraplegia. The following case, already published elsewhere by me,[10] may serve as an instance of this form of paralysis occurring in childhood.

1 *Gaz. Médicale de Paris*, 18. 1875.
2 *C. R. Soc de Biol.* 1864.
3 *Gaz. Med. de Paris*. 1866.
4 *Arch. de Physiol.*, etc. 1870.
5 *Arch. de Physiol.*, etc. 1870.
6 *Gaz. Med. de Paris*, 1871, in four cases.
7 *Jahrb. d Kinderheilk*, 1871.
8 *Med. Chir. Rundschau*, February, 1872.
9 *Virchow's Archiv.* 58 Bd., 1873.
10 *Klinik der Nervenkr.* II. Aufl. 1885, pp. 413–414.

In a four-year old Hungarian boy, who, it was stated, had suffered two years before from a diffuse paralysis of all four members, following a short spell of fever, which continued in the two arms, I found, on examination, the right upper extremity livid, much wasted, the skin thick and loose, the musculature everywhere thin. The atrophic, angular shoulder was merely movable a little forward, extension of the arms and fingers impossible, flexion accomplished with difficulty, and the wasted hand fixed in adduction. The deltoid was only slightly irritable by the faradic current on its inner bundles, the same was true of the infraspinatus and pectoralis, the extensors of the arm and fingers did not react at all, the biceps and certain bundles in the atrophied ball of the thumb only very weakly. The galvanic irritability of the tissues of the nerves of the shoulder and arm was tolerably well retained; the paralyzed extensors underwent slow contractions with a strong galvanic current.

The better nourished left upper extremity was, with some exertion, slightly movable forward and outward, the extension of the arm very slow, flexion prompt, motility of the hand and fingers good. I advised continued galvanization of the nerves, and faradization of the muscles. After some eight months, the child was again brought before me, the right arm and shoulder were fuller and more movable, the left nearly well.

This indubitable form of infantile paralysis finds mention, neither in the more recent, nor in the older monographs. Cellular atrophy of the anterior gray columns seems very rare in infancy. Still, cases of this kind have also been observed in Paris. One of those reported by Charcot[1] is to be considered as such.

Recent researches and observations do away with the erroneous view, that the spinal paralysis in question is peculiar to infancy. Recent fuller discoveries in regard to infantile paralysis led to the knowledge of the fact that an analogous disease also affects adults. The keen insight of Duchenne[2] had already, in 1861, recognized that with riper years and with

1 *Lecons sur les Maladies du Sgst. Nerv.* 2 Fasc. 1873, p. 131.
2 *Electrization localisee.* 2d. ed.

febrile symptoms, there might occur a rapidly extending paralysis of the limbs, with muscular atrophy, corresponding loss of electric irritability, with intact sensibility, and with subsequent gradual recovery of motility and irritability. Duchenne placed the seat of this hitherto obscure affection in the gray anterior columns of the spinal cord, and called the affection, general anterior spinal paralysis. Soon thereafter, Hallopean[1] found, in a similar case necroscopically observed, besides atrophy and a fatty degeneration of the paralyzed muscles and nervous branches, the anterior roots gray and degenerated, and in the dorsal and lumbar cord the anterior horns were of a remarkably dark color, and soft to liquefaction.

Latterly, careful clinical observations were made by Frey,[2] Bernhardt,[3] Charcot,[4] Cuning,[5] Erb,[6] etc., which go to prove that acute spinal paralysis of adults, *poliomyelitis anterior acuta* of Kussmaul, in relation to suddenness of development, integrity of sensibility, and extension of muscular consumption, and diminution of electrical irritability, and the latter often only partial recovery, corresponds with the form of spinal paralysis of children in its most essential features. There are also autopsical evidence, of histological identity. In a case reported by Gombault,[7] a woman in the brief space of half an hour was paralyzed in all her limbs, together with sudden atrophy of the muscles, and loss of electrical contractility. After a protracted recovery, months in duration, there remained atrophy and paralysis of the extensors, of the interossei, and muscles of bases of fingers, with a slight claw-like fixation of the hand.

After one year and a half of suffering, there was found, upon section: pigmentary atrophy of the great ganglion cells of the anterior horns, particularly in the cervical and lumbar region, sclerosis of the anterior roots and corresponding nerve-trunks; in the muscles, the well-known results of degenerative pro-

1 *Arch. géner.* 1862.
2 *Berl. klin. Wochenschr*, 1874.
3 *Arch. f. Psychiat. IV.* Bd., 1873.
4 *Progrès Médical.* 1874.
5 *Ibidem.*
6 *Arch. f. Psych V.* Bd. 1875.
7 *Arch. de Physiol.* Jan., 1873.

cesses.   In the most recent case of Cornil and Lepine,[1] the
patient being a man 27 years old, there occurred, as a conse-
quence of a cold, paralysis of the legs; after two years of the
arms also, with muscular atrophy, diminution or loss of elec-
tric excitability, and undiminished sensibility.   In patients
who died of asphyxia, microscopic examination of the spinal
cord revealed atrophy in a high degree, and local lesions in the
cells of the anterior horns, the gray substance sclerotic, the
vessels thickened, besides a descending sclerosis of the lateral
columns, atrophy of the anterior root, together with granular
degeneration of the muscles, the transverse stripes being still
recognizable.

Moreover, in *myelitis antica adultorum*, the anatomically
demonstrated cell-atrophy is located especially in the gray an-
terior columns of the cervical portion of the cord.   These
clinical forms of cervical spinal paraplegia exhibit a striking
similarity to the formerly described analogous affection occur-
ring in childhood, as the following case will show: A trades-
man, aged 50, was, in Jan., 1873, after a severe cold, attacked
with fever and diarrhœa, which, upon subsiding in the second
week, left behind extreme weakness of the legs: to which, in
about four weeks, was added paralysis of one, and soon of the
other arm. Six months later, I found him, upon inquiring among
the patients at the water-cure of Dr. Friedman, at Voeslau,
suffering from paresis of the remarkably emaciated lower
extremities, with a weak, tottering gait.   The faradic, as well
as the galvanic re-action of the muscles and nerves showed only
a quantitative diminution.   Both arms and fore-arms were
atrophied and paralyzed; so, also, the down-hanging, flexed
hands, which presented a remarkable wasting of the first
interosseus and of the thenar.   The excitability of the Nv.
radiales to either current was slight, being limited to the supi-
nators and the exten. carp. radiales.   The electro-muscular con-
tractility of both sides (even in the carpus of the uninvolved
left hand) disappears in the fingers, the galvanic re-action is
equally tardy.   The first interosseus and the palm of the
thumbs are not excitable.   Sensibility perfect, as proven by
Sieveking's æsthesiometer.   The function of the sphincters has
not suffered.

1 *Gaz. Medic. de Paris.* 11, 1875.

In the course of the summer months, the patient recovered under the influence of hydriatic and galvanic (central and peripheral) treatment, in a remarkable manner.

Walking and going up and down stairs were now possible without assistance, the movements of the shoulder-joints noticeably improved. In electric reaction, the nerves and muscles showed no improvement. When the patient presented himself at the water-cure, in the summer of 1874, he had power over the extensors; and the nourishment of the upper extremities, still, for the most part paralyzed, had become greater. In the course of the next month, there was a noticeable improvement in the motility of the arm and fore-arm; the patient could even dress himself again, feed himself, etc. At last, movements in hands and fingers became freer and firmer. At the end of summer, patient was able to write several lines. There was, however, only slight improvement of faradic and galvanic excitability.

The last form of cervical paraplegia, caused by cell atrophy of the gray anterior columns, which may be conveniently called the amyotrophic form of spinal paraplegia, is double cervical paralysis of the upper extremities, in progressive muscular atrophy.

These cases of paralysis, with atrophy of both upper extremities and muscles (with corresponding cell degeneration on the gray matter of the cervical region) have been described by Valentiner,[1] Luys,[2] Hayem,[3] as well as Schueppel,[4] in consequence of hydromyelitis of the cervical and pectoral regions. If bulbar paralysis and central neurotic trouble along the course of the gray anterior columns, creeping up to the nerve nuclei of the floor of the fourth ventricle (*Rautengrube*), is conjoined with progressive muscular atrophy, as in the observations of Dumenil,[5] Lockhart-Clarke and Radcliffe,[6] Charcot and Joffroy,[7] it shows that the upper extremities alone are attacked preferably by progressive muscular atrophy.

1 *Prager Vierteljahrschr.* 2 Bd. 1855.
2 *Gaz. Med. de Paris.* 32, 1860.
3 *Arch. d. Physiol.* Nr. 2, 3. 1869.
4 *Arch. d. Heilk.* VI. 1865.
5 *Gaz. Hebdom.* 29, 1867.
6 *Brit. Med. Chir. Review.* Vol. XXX. 1862.
7 *Arch. d. Physiol.* 11. 1869.

According to the above explantion of cervical paraplegia,
caused by lesion of the spinal centre, we are led to the consid-
eration of another form which arises from an affection of the
peripheral nervous system.   Brachial paraplegia may be pro-
duced by extravasation of blood in the superior nerve roots
(cases of Ollivier, Schnetzenberger).   Less frequently the trouble
arises from caries of the cervical vertebræ, whereby there being
cheesy deposits,according to Michaud,[1] of the suppurating poste-
rior ligaments of the spine, the superficies of the dura is excited
to inflammatory proliferation and vegetation.   The resulting
pachymeningitis externa may give evidence of a double paraly-
sis of the upper limbs, by pressure upon the nerves of the axilla,
or, in a case described by myself,[2] there was principally
paralysis and atrophy of the upper extremities, (with loss of
the electro-muscular contractility), to which latter was added
paresis of the legs, with difficulty of swallowing.   Proceeding
from the wall of the pharynx backward, was seen and felt
roughness of the cervical vertebræ, particularly anteriorly;
the posterior cervical region showed deep pitting and sen-
sibility to pain upon pressure.

The peripheral form of paraplegia of the upper extremi-
ties can, further, proceed from a pachymeningitis spinalis
interna.   This variety, which was carefully observed by Char-
cot,[3] and which was called *pachymeningitis cervicalis hyper-
trophica*, takes its origin from the inflammatory growth and
thickening of the inner structure of the dura and the contigu-
ous arachnoidea and pia.   The spherical induration of the
meninges over the spinal axis is, furthermore, to be dis-
tinguished from the form earlier recognized by its cervical
location, as well as by the circular pressure upon the spinal
cord, which, after incipient neuralgic irritation, leads, in two
to three months, to a paralysis and contraction of the upper
extremities, degeneration of the muscles, and diminution of
electro-muscular contractility.   Later, the lower extremities are
also attached, yet generally in a mild degree. Besides this periph-
eral form arising from atrophy of the anterior and posterior

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1 *Sur la Meningite et la Myelite dans le mal vertébral.* 1871.
2 *Oestr. prakt. Heilkunde.* 48. 1866.
3 Soc. de Biologie. 1869.

roots, there is, according to recent experiments by Joffroy,[1] a central form belonging to an earlier category. This observer found on the cervical enlargment myelitic alterations, with, for the most part, formation of cavities. Here, also, the clinical form of disease was characterized by paralysis and atrophy of the arms, and only single muscles of the hand and finger remained intact.

The diagnostic distinction of the clinical character of the occasional form of cervical paraplegia must depend upon the symptoms of the disease. The peripheral cervical pressure-paraplegia begins with a neurotic paralysis, pain, and hyper-algesia, in the course of certain nerves, followed by anæsthesia, paralysis, and atrophy of corresponding muscles. In the beginning, faradic and galvanic irritability is considerably increased, but becomes less and less; the farado-muscular contractility is diminished, or lost; the galvano-muscular, on the contrary, for a long time heightened. Reflex irritability is, as a rule, extinguished. Sometimes there are, also, trophic disturbances of the skin, nails and knuckles.

For the differentiation of the different forms of peripheral cervical paraplegia, it is necessary to keep in mind the pathological signs. In traumatic injuries of the cervical spinal column (shock, or blow upon the neck), there may follow hemorrhage within the cavities of the vertebræ, and about the centrifugal nerve-roots, with marked injection of the brachial region. In the same variety of cases, besides loss of consciousness, there may appear instantaneous or rapidly-developing paralysis of the upper limbs, in which the paralysis is generally found to be partial, and combined with tonic and clonic contractions in single groups of muscles of the arm. If the lesion goes no further, the symptoms gradually disappear. When there is hemorrhage, which, as in fractures of the cervical vertebræ, is attended with myelitic alterations in the cervical portion, there follows, in addition to the antecedent cervical paraplegia, and increase of temperature, paralysis of the lower extremities, involving the bladder. Upon search, topical examination will reveal deformity from fracture, stiffness, and abnormal position of the head, and painfulness upon pressure and motion.

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1 *Sur la Pachymening. cervic. hypertrophique.* Paris. 1873.

Brachial paraplegia, which is sometimes observed with caries of the cervical vertebræ, when it is a consequence of pachymeningeal pressure upon the upper nerve-roots, is among the earlier recognized appearances of a neurotic paralysis. Upon the initial neuralgic symptoms, together with the synchronal hyperalgesia and vaso-motor disturbances, there follow later, paralysis of the arm, atrophy with the reaction anomalies of the wasted muscles depicted above, with loss of reflex excitability.

In the cervical paraplegia, due to pressure-lesion of the cord, on the contrary, the affected muscles are not materially altered, in a long time, in nutrition or electrical re-action and sensibility. The reflex power exhibits very considerable increase, or where the reflex irritability is slight, and later, even entirely lost, there is temporary accession of spontaneous, painful convulsions of the limbs, which may have its origin in heightened excitability of the gray substance in diminished cerebral inhibition.

Those rarer spontaneous muscular contractions, produced for the most part upon passive movements, are to be designated as spinal reflex spasms, and the inappropriate denomination, spinal epilepsy, is to be reserved for those varrieties, in which pathological or experimental lesions of the spinal cord, or its nerves (according to Brown-Sequard) manifest themselves in epileptiform convulsions.

That the double paralysis of the arms attending caries of the cervical segments of the vertebral column, may be occasioned by deep-seated medullary disease, is demonstrated by an old observation by Budd.[1] In the case of a girl who suffered from scrofula with caries of the cervical vertebræ, and retropharyngeal abscess, there was for two years only a paralysis of the upper extremities; and later, also, development of paralysis of the right leg. Under the influence of excitement, likewise upon urination and defecation, there occurred in the paralyzed leg—sometimes also (in mild degree) in the corresponding arm—more or less powerful reflex movements. In a case described by Vogel and Ditmar,[2]

1. *Med. Chir. Transact.*, vol. 22.
2. *Deutsche Klinik*, 1851.

the initial paralysis of the upper, and later, also, of the upper
extremities, as the autopsy showed, was connected with ex-
ostosis reaching from the third to the fifth cervical vertebræ.

Concerning the possibility of the subsidence of such press-
ure-paralyses before they pass into a spinal lesion, we are
enlightened by an observation of Brown-Sequard's, wherein by
a luxation of a spinal dislocation the suddenly-occurring
paraplegia disappeared. In a case observed by E. Rollett[1] at
Oppolzer's Klinik, the double paralysis of the upper limbs,
arising from compression of the cord, disappeared spon-
taneously. In consequence of inflammation of the highest
cervical vertebræ, there resulted, in the beginning, gradual
luxation of the epistropheus backwards and downwards, and
anchylosis with the atlas. The growing pressure upon the
fore part of the spinal cord produced, first, paralysis of the
upper, then also of the lower extremities. Not until after
the direction of the pressing protuberance was altered in fur-
ther consequence of fusion of the bones, so that it was di-
rected forward, did the paralysis of the legs disappear in con-
sequence of removal of compression of the cord; later, that
of the arms also disappeared. The patient lived a number of
years, and finally died of cardiac disease.

Finally the *ensemble* of symptoms deviates from the typi-
cal in the third category of double-paralyses of the arms, in
amyotrophic forms of cervical paraplegia, caused by cell-
degeneration in the gray anterior columns. In circumscribed
progressive atrophy of the upper extremities, continuing a
long time, the diagnosis is not attended with difficulty. In
cervical myelitis, complication with bulbar paralysis, with
participation of the nervous substance, points to ascending
inflammatory processes in the anterior horns.

Paraplegia of the upper extremities in childhood is very
rare, and, as my case described above shows, will be diagnosti-
cated without difficulty, on account of the characteristic forms
of the paralysis, and atrophy of the affected arms. In the cor-
rect interpretation of the symptoms, only intra-medullary
nerve-formations in the cervical portion could cause any dif-
ficulty. Cases in children may be developed under the guise

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1. *Med. Wochenschr.* 24–26, 1864.

of infantile spinal paralysis, as is shown by the following case
reported by Gull:[1]

In an infant eight months old there was at first gradual
paralysis of the right arm, and after fourteen days the left
was involved. The head was drawn down between the
shoulders, the neck stiff. After $2\frac{1}{2}$ months both upper ex-
tremities, especially the right, were much emaciated, and
hung motionless by the side of the trunk.

The legs failed in nourishment and motility considerably,
and showed frequent spasmodic contractions, particularly in
the right, weaker leg. The urine was ammoniacal. The in-
voluntary motility of the lower limbs lasted in a measure
to the time the child died, in the 7th month of the disease.

Upon section there was found on the under side of the
cervical enlargement, opposite the origin of the sixth and
seventh cervical nerves, a solitary tubercle, which had caused
the compression of the cord with perfect absorption of the
tissues. The swelling appeared to have proceeded from the
right back and neighboring parts of the lateral fibres. Due
consideration of the symptoms, especially the drawing down
of the head between the shoulders, the stiffness of the neck,
the periodical spasmodic movements of the legs, together
with the ammoniacal character of the urine, gives us sufficient
data for a conclusion that we are dealing with the usual form
of spinal paralysis of children, and for the elimination of the
symptoms of pressure-myelitis.

In regard to the recently-considered *myelitis antica adult-
orum* (the poliomyelitis anterior antica of Kussmaul), there re-
mains to remark, in conclusion, that, according to my obser-
vations above, as well as a recent like communication by Carl
Weiss,[2] the febrile beginning, the simultaneous paralysis of
the legs, and later, of the arms, the rapid atrophy of the
muscles, and their resistance, the absence of disturbances of
sensibility, or of the sphincters, of potency, with progressive re-
turn of motility, is sufficient for a diagnostic decision. As
signs of distinction from progressive muscular atrophy, the
following are peculiar to poliomyelitis: the initial form, some-

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1. *Guy's Hospit. Rep.*, 1858.
2 Inaug. Dissert., Breslau, 1875.

times with brain symptoms, the rapidly developing muscular
atrophy, the paralysis ascending, as a rule, from the lower to
the upper extremities; the speedy improvement of the legs,
the paraplegia of the arms persisting, the gradual equalization
of the intensive and extensive motor disturbances, and very
tardy return of electric reaction.

The prognosis of cervical paraplegia is dependent upon the
character of the primary lesion. Worst obviously in the
paraplegic forms in pressure-myelitis, for cases like the above-
mentioned, where the pressure-paralysis is corrected by
nature or art, are very rare. The arm-paraplegias of acute
myelitis share the fate of the quickly fatal intercurrent af-
fection. Among the chronic myelitic affections, the so-called
amyotrophic forms of cervical paraplegia, arising from pro-
gressive muscular atrophy, have the most unfavorable
character, if they are associated with bulbar paralysis. In
infantile double paralysis of the arms, only a partial improve-
ment of the unequally-affected extremities is to be expected.
In anterior poliomyelitis, judging from my own experience,
and that of C. Weiss, we may expect, when the individuals
are previously sound and in good circumstances, and when
there is a rapid disappearance of paresis of the legs, some-
times a complete subsidence of the disease. On the contrary,
the course of that variety of anterior poliomyelitis, in which,
conjoined with double paralysis of the arms, there is later
paralysis of the other limb, is unfavorable, as also (as I wit-
nessed in two cases) where there are indications of atrophy of
the optic nerve, and paralysis of the nervous nuclei, compli-
cating the case. Finally the cervical paraplegias of the periph-
eral variety, those caused by hemorrhage about the upper
nerve-roots, and those resulting from pachymeningitis cervicalis
hypertrophica, are susceptible of treatment. In the last-named
variety spontaneous recovery was repeatedly observed by Char-
cot, and this fact, in particular, is to be kept in mind the
pretensions of therapeutics to the contrary notwithstanding.

The treatment of the form of cervical paraplegia in question
must be directed toward furthering absorption, as well as to-
ward the enlivening the circulation and innervation of the
parts involved.

In the beginning, moderate doses of iodide of potassium, warm baths and thermal springs, are indicated. Later, mild hydriatic remedies are to be recommended, (which are to be preferred to half baths, cool dorsal douches, as well as their combination with moist rubbings and packs).

In electrical treatment, the constant galvanic spinal current and labile currents directed along the nerve roots, and their course in the paralyzed members.*

In stubborn paralysis and muscular atrophies, the nerves may be galvanized, and the muscles subjected to the induced current.

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* In obstinate cases of paralysis and muscular atrophy, alternate treatment of the nerves with the galvanic, and of the muscles with the induced current, may be of service.

Neurological Correspondence.

MESSRS. EDITORS:—I have to report that during the three months just passed, matters pertaining to Neurology have been exceedingly active. Societies, other than those dedicated especially to the consideration of this specialty, have devoted a considerable time to its study. It is a good sign, for there is no question but that the nervous system should be studied very much more than it is by the general practitioner.

I will endeavor to give you the cream of the discussions which have taken place. I also forward you a number of papers entire, which have been kindly furnished by their several authors for the JOURNAL:

THE NEUROLOGICAL SOCIETY.

Dr. Thomas R. Pooley read a paper, entitled "Hemiopia," in which he considered the arguments *pro* and *con*, as to the total or semi-decussation of the nerve fibres in the optic chiasm.

He reported a case of hemiopia in his own practice, together with post-mortem, and gave a review of cases in literature, so far as accessible, in order to ascertain which of the views relating to the course of the nerve fibres is supported by clinical observations. His own case was one of right-sided binocular hemiopia, dependent on a gummy tumor in the left posterior lobe of the brain; the following is a summary:

Dr. Pooley said: "On September 14, 1875, a gentleman, 55 years of age, consulted me. Thirty years ago he contracted a chancre, which was followed by only a slight pharyngitis. He took mercury and iodide of potassium, for a time, and has had no manifestations of the disease since. He was married, and had two healthy children, one of whom is married. Until the

attack about to be described, he had always been healthy.
Six weeks before his first visit to me, he complained of men-
tal illusions, and epileptiform convulsions, severe and pro-
longed, followed by maniacal excitement, lasting for several
hours; soon after he had another similar attack, followed by
slight frontal headache. After the last attack there was a
disturbance of vision, which he described as a fluttering be-
fore the eyes, but no diplopia. When I saw him the annoy-
ance of vision had passed away; and he was referred to me
for an ophthalmoscopic examination. He felt dizzy, talked
confusedly; memory bad, omitted words, and was exceed-
ingly emotional. He had slight periodical headaches, with
nightly exacerbations, and inordinate desire for sexual inter-
course. There were no external manifestations of syphilis; no
paresis of the ocular muscles, nor mydriasis. Examination
of the eyes showed $M \frac{1}{10}$ $S \frac{20}{20}$, slight insufficiency of the
interni, and a small posterior staphyloma in each eye. The
field of vision was unimpaired. Dr. Keyes and I made the
diagnosis of cerebral syphilis, and ordered iodide of potassium,
bromide of potassium, and bromide of ammonium. Two
days later he returned, saying that he could not see objects in
the right part of the field of vision. On examination, we
detected *right-sided hemiopia of both eyes, which was
sharply defined by a line drawn vertically through the
points of fixation.* He took the prescribed remedies for
about a month, during which time he improved in all particu-
lars, and even lost his hemiopia for a time; it soon returned,
however, and continued thereafter until his death. About the
beginning of November, I first noticed that he had partial
hemiplegia of the right side, and some paralysis of sensation in
the right arm. His memory was more impaired, and, in con-
versation, he was often at a loss for the appropriate words.
He continued much in the same condition until about Christ-
mas, when he became very feeble and considerably emaciated.
There was, however, no accession of his cerebral symptoms,
and his attending physician was inclined to attribute much of
his trouble to an affection of the liver. He recovered again
from this attack, and, to some extent, resumed his duties.
The hemiplegia and aphasia were more pronounced. Early in

March he became much worse, attacks of vertigo almost amounting to loss of consciousness, became more frequent.

" On the 30th of March, he complained of diminution of vision of the left eye. I found S-$\frac{20}{50}$ and choked disc in the left eye, whereas there was no change in the right. I now positively insisted on my previously expressed diagnosis of intracranial tumor, and gave a hopeless prognosis, thereby preventing a journey to the Hot Springs of Arkansas.

" In the night of April 7th, he had very intense pain in the head, followed by tonic convulsions and coma, in which condition he died early in the morning of the 8th of April.

" The post-mortem was made on the 9th, thirty hours after death, with the assistance of Dr. Knapp, and in the presence of Drs. Bowden, Lynch and Jourdan.

" On the inner surface of the calvarium were two small defects of substance, apparently caused by an absorption of the inner table. Meninges normal. The dura, however, in the posterior portion of the posterior lobe was adherent to the bone, and thickened, the surface of adhesion being about the size of a cent. Right hemisphere normal.

" The left posterior lobe was somewhat enlarged, and had a depression in its posterior portion, in which, imbedded in the substance of the brain, lay a yellowish-white, rather hard, and somewhat roundish tumor, about $1\frac{1}{4}''$ in diameter and $\frac{1}{2}'$ thick. It was so closely adherent to the dura and pia that it had to be detached with the knife in removing the brain.

" There was a considerable area of softening of the brain substance, which, in the immediate vicinity of the tumor, was almost liquid, and quite gradually increased in consistency as it passed over into the normal brain substance. The softening extended from the posterior through the entire middle lobes, and ceased at the anterior portion of the anterior lobe and at the posterior portion of the posterior lobe. The left thalamus opticus and the neighboring brain substance were completely softened. The cerebellum was normal. The chiasm and optic nerve trunks showed no abnormality throughout their entire course. Nor was there any appreciable change in the vessels at the base of the brain. The right lateral ventricle was dilated and filled with serum. *Thorax*, apex of right lung ad-

herent to pleura throughout to small extent, and when sepa-
rated showed numerous small punctate, whitish granulations.
Left lung also adherent, but to less extent. The apices of both
lungs showed the usual changes of chronic pneumonia. Heart
normal. *Abdomen*, liver irregularly lobulated, fatty, and has
three stellate scars upon its anterior border. Spleen normal.
Kidneys, with the exception of some hyperæmia of the cortex,
normal. Nothing unusual was found in the other parts of the
body. The tumor had a solid, rather whitish centre, with a
less consistent hyaline periphery.

" For the following report of the microscopical examination
of the specimens, I am indebted to Dr. Alt, resident surgeon of
the New York Ophthalmic and Aural Institute. Over the tumor
of the brain, the meninges were very much thickened, by the
new formation of fibrous tissue. The tumor itself was formed of
round cells of various sizes, between which there was a small
amount of hyaline matrix, not very different from myxoma-
tous tissue, in which some long, striated elements of connec-
tive tissue could be found. Whilst in the tumor itself no ves-
sels were to be found, its surroundings were very vascular.
These vessels showed the following very remarkable changes.
Their walls, especially those of the capillary vessels, were very
much thickened, and transformed into a hyaline mass. This
thickening was frequently so far developed to entirely close up
the lumen of the vessel. In this hyaline mass some small,
fatty globules were scattered around.

" There was also one larger vessel which showed the atheroma-
tous changes to a very marked extent. It was surrounded by the
remnants of an old considerable hemorrhage, which was situated
posterior to the tumor. No more nerve elements any longer ex-
isted in the tumor. The mass of the brain which surrounded the
growth, exhibited in a well marked manner, what is called yel-
low softening. Not only was the marrow of the nerve fibres in
a state of fatty degeneration, but there was also a great amount
of fatty detritus. The indurated parts of the apices of the
lungs showed the changes produced by chronic parenchymatous
and interstitial pneumonia. The induration was caused by the
new formation of connective tissue, which was pigmented in the
ordinary way, and enclosed some small heaps of round cells,

and fatty detritus. The stellate scars of the liver were formed in the same manner of dense connective tissue, which was in connection with the thickened capsule of Glisson. Between the fibres of connective tissue were found remnants of liver cells, in fatty metamorphosis detritus. The scars were very vascular. The parenchyma of the liver, as a whole, was in the beginning stage of fatty degeneration, and, as is always the case, the periphery of the lobuli exhibited this process of metamorphosis to the most marked extent. There was no amyloid reaction."

The conclusions at which Dr. Pooley arrived are embraced in the following propositions:

1. "That in right and left-sided hemiopia the impairments of vision, as well as the accompanying symptoms of paralysis, can only be explained by the theory of semidecussation. Symptoms of paralysis are especially in favor of this view. The existing morbid process may be located in the optic tract, corpora quadrigemina, or optic thalami.

2. "Temporal hemiopia is most easily explained by assuming that in such cases we have a lesion which presses upon both inner sides of the optic nerve fibres (fasciculæ cruciati); and this destroys the inner half of both retinæ. And such have been the lesions actually found. This explanation is quite compatible with the theory of semi-decussation.

3. "Nasal hemiopia is produced by pressure upon the posterior angle of the chiasm, or by pressure of pathologically changed arteries upon the outer border of the optic chiasm—that is to say, the inner crossed fibres. But, strictly speaking, the crossing of the fibres in the chiasm is not brought into the question, as only the inner fasciculi are crossed. It must also be admitted that in this form of hemiopia changes so commonly seen in the optic nerve and retina are, to some extent, responsible for the defect of vision in the visual field.

4. "The observations of superior and inferior hemiopia are too few to warrant us in coming to any positive conclusion as to their signification in deciding this question."

Dr. Edward C. Spitzka said: "I dare venture an opinion on this question only in so far as it touches on the confines of neuro-physiology and comparative anatomy, and from these

points of view I can affirm the conclusion at which Dr.
Pooley had arrived, to its fullest extent. The case observed by
the Dr. himself, and in which a very careful post-mortem analysis
seems to have been made, certainly cannot be explained on any
other ground than that of a partial decussation. The princi-
pal lesion noticed was a large gummatous tumor, compressing
and destroying a great part of the occipital lobe of one hem-
isphere, the opposite hemisphere being found intact in the cor-
responding region. Now, the gentleman who has last spoken
referred to a connection between the retina and the occipital
lobes, as discovered by Dr. Ferrier, a connection which, I may
add, was, long before Ferrier performed his experiments, dis-
covered by Gratiolet and confirmed by the physiological re-
searches of Meynert: witness the former's "radiations op-
tiques.

" Therefore a single well-authenticated case, in which other
lesions can be excluded, and in which bilateral hemiopia exists
in connection with a localized destruction of certain convolu-
tions in one occipital lobe, would appear to be conclusive evi-
dence as to the existence of a partial decussation. That our
visual impressions are registered in the cortical centres of the
hemispheres, admits of no doubt at the present day. Let us,
then, consider the probable manner of such registration, and
we shall see why no other conclusion is admissible: Our reg-
istered visual impressions are stereoscopic in character, and
normal stereoscopic vision implies the functional participation
of both eyes in the visual act—an isolated compound concep-
tion, be it abstract or sensorial, of which latter a stereoscopic
registration is an example, will have for its presumptive seat,
such groups of ganglionic cells as are situated topographically
near each other, and consequently at least in the same hemi-
sphere. It follows that the corresponding centre and seat of a
stereoscopic recollected image must be connected on the one
hand with the eye of the same side, on the other with the op-
posite eye. It is true that we might suppose a stereoscopic
blending to result from the union existing between symmetri-
cal centres on opposite hemispheres, through the corpus cal-
losum, but a connection of this kind, although it may exist in
relation to the double projection of identical fields of vision,

must be of secondary importance to such a contrivance as a partial decussation, on account of the interposition of a second tract, delaying the transmission of impressions from hemisphere to hemisphere, and seriously interfering with the necessary synchronous character of the double impressions composing a stereoscopic picture.

" Direct anatomical examination has led me to the result that in the human chiasm, as in the chiasm of certain of the higher mammalia, the outermost fibers on each tract go to the optic nerve of the same side. That as regards the lower animals, Biesiadecki and others are correct in claiming a total decussation, does not militate against the previous observation which applies to genera whose eyes are directed forwards, not laterally as is the case of the species first examined by the authors who believe in a total decussation in every vertebrate! That such a total crossing does exist, for instance with the teliost fishes, any one can satisfy himself by examining the encephalon of the common haddock, where the right optic nerve, coming from the left optic tubercle, passes *under* the left optic nerve coming from the right optic tubercle without entering into *any* connection with it at all; or the herring tribe (Clupea), in whose case the one nerve passes through a slit in the opposite nerve.

"As to the total atrophy of one optic tract ever resulting from artificial or pathological destruction of the opposite optic nerve, I have every reason to suppose that a naked eye examination is calculated to mislead. I am under the impression that most pathological records will show that destruction of the one optic nerve in its whole diameter results in degeneration of both optic tracts. The opposite tract is, in such cases, more decidedly atrophied than the tract on the same side, for the simple reason that the fibres which do decussate preponderate over those which remain on the same side, and frequently the more marked atrophy has been alone noticed. In these cases the microscope affords the safest test and criterion.

"Certain apparently anomalous aberrations from the normal standard, have been accumulated in our literature, whose existence it is more difficult (if not impossible) for those to explain, who believe in a total decussation, than for their op-

ponents. Among others, Caldane,* and Vesalius, the great
anatomical master, noticed an absence of the optic chiasm in
human subjects, the nerve for the right eye coming from the
right hemisphere, that of the left, from the left hemisphere.
Vesalius had the good fortune to have been acquainted with
the subject he dissected, during lifetime, and was thus en-
abled to give us the assurance, that neither diplopia nor hemi-
opia existed in the man. It is a law. derived from accumu-
lated experience and observation, by morphologists and tera-
tologists, that the more frequent aberrations from any stand-
ard, which is called typical, are foreshadowed by an embryonic
stage, or by some similar condition of lesser degree, existing
in the adult, and whose excessive and even exclusive develop-
ment in one individual constitutes the variation in question;
if, then, we suppose the uncrossed fibres to preponderate in
growth and development, over the crossed fibres, we shall have
at least offered a better theory, in explanation of such excep-
tional cases, than can be advanced by those, who claim a total
decussation.

" Advocates of the opposite side of the question might as-
sert, as I believe has been asserted, by one of the authors
quoted by Dr. Pooley. that cerebral changes being so fre-
quently symmetrical, corresponding areas on opposite hemi-
spheres, were coincidently affected. and applying it to the case
in question, suggest that some unobserved lesion had existed
on the opposite side from where the gumma was found. But
against this I have to say, that while meningeal exudations,
calcareous plates of the pia mater, and the foci of encepha-
litic processes generally are often strikingly symmetrical,
adventitious growths, such as existed in Dr. Pooley's case,

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* 'De anno 1520 Paduae fecimus auatomiam. quam legit D. Nicolaus
de Janua, ubi vidimus omnes, qui ibi aderant, et praecipue doctores
sacratissimi Collegii Patavini, inter quos ego Ludovicus Pasinus, vidi-
mus, inquam, nervos opticos, notabiliter separatos, ut dexter tendebat ad
oculm dextrum, sinister vero ad sinistrum, unde quod vidimus testamur,
nec veritas habet angulos.'

' Numerous other cases are noticed by mediæval anatomists—the only
instance noticed within recent years, with which I am acquainted, is a
specimen without a history, in the museum of the Westminster Hospital,
London."

follow no rule whatsoever in this respect. I would have been much interested to know whether the same hemisphere or the ambitus cerebelli, exhibited other gummata, as explaining other sharply defined symptoms of cerebral disturbance in this case, and do not know whether it was examined in this direction or not. With the increasing perfection of cerebral autopsies, cases of single gummata are becoming more and more rare, although the possibility of their solitary existence is not yet to be denied. It is unfortunate that the thalamus was disorganized on that side, where the cortical centre was destroyed, it renders the anatomical diagnosis more complicated; however, the changes of this ganglion were evidently secondary, to the invasion of the cortex by the progressing tumor, and as the hemiopia was of long standing, I think that we may safely say that it was produced by the annihilation of the perceptive centres on one side of the brain, and that Dr. Pooley's case forms a valuable addition to the pathological evidences, which are accumulating in favor of a partial decussation of the optic nerves."

Prof. Hammond mentioned a case under his care probably having some bearing on several points in the paper of Dr. Pooley:

A married man subject to syphilis, was seized one night with hemiplegia, involving the left side, aphasia, and nasal hemiopia of left eye only. He was treated with iodide of potassium, which produced an apparent recovery. A short time after, probably a month, he was attacked again with hemiplegia, this time upon the right side, with right hemiopia, but no aphasia. He recovered from that, and was attacked a third time with hemiopia of left eye, right hemiplegia and aphasia.

Dr. Eugene Dupuy believed in entire decussation of the fibres. Experiments were performed more than fifty years ago, and so far as they went, they showed complete decussation. He himself has been performing a series of experiments, confirming those referred to above, and has found that decussation is especially marked in some fishes, and lower animals. He also referred at length to the experiments of Dr. Ferrier, of London, regarding the connection of the retina and occipital lobes.

At a meeting of the Neurological Society, held Nov. 6th, 1876, Prof. Hammond read a paper on "Hysterical Contractions." The paper will be found in another portion of this JOURNAL.

After the reading of the paper, the following remarks were made:

Prof. M. A. Pallen said:

There were some points in the paper of Dr. Hammond on which he (Prof. Pallen) would desire a more definite history, particularly with regard to some of those cases of young girls (cases 11 and 12) who suffer from contracted knee joints. The largest number of these cases he believed occurred in females, and the majority of these in young girls, in whom the generative principle is most active. With many of these cases the history is so meagre that we are unable to make out a positive diagnosis of hysteria. The majority of these cases, he thought, depended upon disease of the cord, but at the same time there may be neuralgia. He was one of those few persons who believe that hysteria always originates in the ovario-uterine region, and that secondary changes take place in the cord, as a result of that condition. He does not think that they are primarily produced in the cord. He had seen several cases of hysterical contractions of the joints. One was a young woman, 27 years of age, of excessively nervous organization, whose emotional power was intense. The flexor muscles were contracted to such an extent that the heel rested upon the buttocks. She had been put under the influence of morphia, and her limb was immovable for three weeks; this treatment was abandoned as of no avail. Her menses were regular (every 28 days), but there was at this time a great deal of irritability of the spine. In consultation with the physician in charge, he suggested the propriety of removing the ovary, which was not agreed to, and the patient was lost sight of, until a year after, when she died of general marasmus and with various hysterical manifestations. He would like to ask Prof. Hammond why he uses ergot in these cases, unless there is some hyperæmia of the cord?

Prof. Hammond said he only used it in one case, and then he gave the reason for it.

Prof. Pallen had seen many cases of hysterical contraction. He had seen spasms resulting from the irritation of the clitoris by ascarides.

Prof. Hammond desired to know if Dr. P. called such cases hysterical?

Prof. Pallen thought they were more properly involuntary contractions from a special cause. He thought there was no doubt but that many of these cases of hysteria depended upon uterine or ovarian causes. He had, however, seen more cases of hysteria which did not depend upon such cause.

Prof. L. A. Sayre referred at some length to the subject of reflex irritability from an abnormal condition of the genital organs, and mentioned several cases where the patients were cured by the simple removal of the source of irritation, and in such cases the cure is almost immediate.

Prof. Pallen still thought that over 90 per cent. of the cases of hysteria resulted from uterine or ovarian irritation. He further remarked that he had seen cases of hysteria in boys; whereupon Prof. Hammond remarked that he thought his friend Pallen a little inconsistent. He says hysteria is due to irritation of the ovaries, and then says hysteria occurs in boys! He had seen several cases where there was no material irritation whatever. Many suffer from an excess of imagination, and much of the trouble is brought on voluntarily. He had had a patient under his care having hysteria, to whom he administered 12 oz. of chloroform in the course of twenty-four hours. She was again attacked, and when she heard the name "chloroform" she got well! He had seen other cases where cold water was thrown on the patient, and they were cured instantly!

Dr. Dougherty, of New Jersey, mentioned a case of a lady, aged 48, who had been abroad, and who, on her return from Europe, found that her husband had not remained at home as steadily as he ought to have done. She, therefore, accused him of infidelity, while she consoled herself by going into a hysterical state, which terminated in melancholia. She would sometimes brighten up, but would relapse into this peculiar condition. She could procure sleep only by the aid of anodynes. Her case presented such phenomena as to cause her friends to desire to have her transferred to a quiet place. Now, said Dr.

D., was this hysteria, or was it insanity? What is the limit between insanity and hysteria?

Prof. Hammond thought that, according to Prof. Pallen, if the lady had passed her climacteric period she could not have hysteria.

Prof. Pallen said he did not state that if a woman had passed her menstrual period she would no longer be subject to hysteria.

In connection with the subject of hysteria we will quote Prof. Pallen's views as enunciated in a debate at a meeting of this society in February, 1875.

Clinically speaking, he remarked, we can get some insight into the trouble by studying its phenomena synthetically rather than analytically, for its effects point very decidedly to its cause.

He has been in the habit of dividing hysteria into centric, when there is a want of co-ordinative action between the cerebro-spinal and sympathetic systems of nerves, and eccentric, or peripheral, the result of reflex irritability—the totality of manifestations being a disturbance of motility and sensibility.

What is very strange in the manifestation of hysteria is a very apparent paradox, a complete reversion of the law of intellectual development which recognizes sensuality to be in the inverse ratio of mental culture—that the greater the capacity for sexuality the less the development of mind, marked examples of which are seen in the donkey and the hog! Yet by far the greater number of hysterical patients are women of refinement, culture and purity; women who are æsthetical and sensitive—painters, musicians, poetesses and the like, and who are libidinous, erotic and sensual in the highest degree when laboring under the hysterical attack.

We are often shocked to find young and innocent women, virgins even, exhibiting glaring and marked indications of erotic excitement found only in the voluptuary and the nymphomaniac.

Here is an effect of genital irritation somewhat explaining the pathological status of the individual.

As an analogue, another curious fact is that women very frequently develop in puerperal mania the very opposite quali-

-ies of heart and mind which are peculiar to them in health. Thus a pure and religious woman may become obscene and profane. From these facts we observe that, unless we reason synthetically, we cannot obtain any knowledge of the pathology of the trouble, because morbid anatomy has not yet yielded indications of its causation in the nervous system.

There are certain facts, however, which we do know, and which bear very strongly upon the pathology of hysteria, the principal of which is that *pathological changes alone, in the uterus, the ovaries, or both, are rarely productive of the phenomena of hysteria,* but that in a vast number of cases the causes may be traced to disordered functions of the ovaries, either from hypertrophy, early and frequent ovulation, or from conicident uterine and ovarian congestion. Of course he referred only to eccentric or peripheral hysteria, and from data on the subject it is not at all improbable that centric hysteria is but the sequence of the other form; and it, too, might likewise be thus classified as the generative circle, either partially or totally, in most women, is in excess of development.

We often find in frail, delicate women, whose adipose tissue is but very slightly deposited, disproportioned mammæ, an enlarged clitoris, elongated nymphæ, vulvar and vaginal hyperæsthesia, and when we examine the ovaries we discover one or perhaps both sensitive, enlarged, and when rolled between the finger of our hand in the vagina or rectum, and the other hand on the supra pubic region, the hysterical manifestations are induced in an intense and marked degree, and then the reflex irritability which is manifested by and through hysteria finds its causation in the plexuses of nerves distributed in and around the ovario-genital regions. For these reasons only can we account for the libidinous and erotic manifestations of young virgins when laboring under hysteria. Therefore it may be stated that the pathology of hysteria is to be found in causes arising in the pelvis, which are reflected to the cerebro-spinal centres, and manifested by the phenomena of disordered motility and sensibility.

The name "hysteria" is an unfortunate one, as it leads us to locate the trouble in the uterus. The term *ovaria* will not do, as it is the plural of *ovarium.* Ovaritis likewise is objection-

able, because it implies inflammation of the ovaries. Nor will "oopharia" answer, as Barnes calls it, as that locates the trouble in the ovaries alone. Metroopharia would in all probability cover the entire ground as indicative of derangement of the utero-ovarian regions.

The treatment of the hysterical phenomena, although purely empirical, points to its causation. The so-called anti-spasmodics and nervines rarely accomplish any decided results in the more decided forms of the trouble, but if we can succeed in relaxing our patients by emetics, we very often cut short the attack, and frequently prevent its recurrence, until the same pelvic cause again arises. This form of treatment indicates that there is an irritable condition, a tension of certain sets of nerves, which determines the phenomena in question, and these sets of nerves are located in and around the genital circle. The bromides, and the preparations of valerian, musk, asafœdita, camphor, etc., have not been very successful in his hands, and save in the most severe forms of cataleptiform manifestations, chloroform has likewise been inefficient. But if we direct our attention toward the pelvis; if we succeed in allaying ovarian congestion; in modifying ovulation; in relaxing genital nerve-tension and hyperæsthesia, in cases where no surgical interference is demanded, then we can cure our patients of the trouble, instead of alleviating symptoms. In patients who are nymphomaniac, who have enlarged clitoris or hypertrophied nymphæ, nothing short of surgical procedure will accomplish any permanent beneficial result.

Hysteria rarely, if ever, is developed in women with uterine disease simply, but when it does take place, it is coincident with ovulation, except in cases of confirmed hysterical habit when any exciting or emotional cause will develop it, and even in these cases, the conjunction of causes is very apparent. In those cases of hystero-epilepsy and dysmenorrhœic hysteria, we invariably find that the uterus is not the only diseased organ, but that the ovary likewise partakes of the disorder, and the hysterical as well as the epileptiform phenomena are aroused by and are coincident with ovulation. Dysmenorrhœa without hysteria, is a most frequent occurrence, even when the patient labors under that form of so-called " ovarian dysmenor-

rhœa''; yet hysteria never takes place unless there is excess of genesic power as manifested by a marked erotic nature, which may be unrecognized by the woman, and whose life is pure in thought as well as deed, but which develops itself in a more or less intense degree as soon as the hysterical train of deranged motility and sensibility is aroused.

In order to illustrate these points, he would relate a case wherein they were most marked in every particular.

A very well developed, muscular woman, who had menstruated at the age of 12, became impregnated at the age of 16. Up to that time she had been perfect as to menstruation and general health. After impregnation she resorted to abortion, which resulted in chronic endometritis. Each menstrual period thereafter increased the uterine hyperæmia, the hysteralgia becoming greater, until finally the endometrium became so much enlarged and turgid that it prevented a free exit of blood, and there was obstructive dysmenorrhœa co-added to hysteralgia. Hysteria had been a prominent feature of menstruation, and this ultimately became hystero-epilepsy. These symptoms occurred at no other period than at menstruation. She was treated surgically by a division of cervix uteri and the internal os, which did away with the obstruction, and the endocarditis was cured by local applications. Yet she had hysteria, and occasionally hystero-epilepsy, although the hystero-epilepsy was cured. Ovulation still went on. Fortunately, she became impregnated after a few months; ovulation, of course, was suspended, and the hysteria and hystero-epilepsy likewise were suspended. She went to full term, and was delivered of a living child. She never had any more hysteria or hystero-epilepsy, but bore two more children, after which she passed from his observation. This case was one of marked peripheral or eccentric hysteria, dependent upon reflex irritability of the utero-ovarian system of nerves, and is a typical example of the pelvic causation of hysteria.

THE MEDICAL JOURNAL ASSOCIATION.

Oct. 20th, 1876. Geo. M. Schweig, M.D., read a paper on cerebral exhaustion, with special reference to its galvano-balneological treatment.

After a few general remarks, he considered briefly its etiology, pathology, and symptomatology, and then considered its treatment.

· The one great object of a therapeutic course is to seek, by appropriate nutrition and physiological stimulation, in conjunction with the much needed rest, to restore to the brain its lost vigor, to correct its impaired nutrition; in short, to bring it back to a normal state. To effect this, it is of the utmost importance that no time should be wasted with tonic measures, that exercise but little, if any, immediate influence on the brain.

The very first condition for treatment, the *sine qua non*, is " rest for the exhausted brain," not a brief rest of days, nor even weeks, but absolute abstinence from all mental tasks throughout the entire treatment, and until health is perfectly restored. * * * Of remedial agents, he has found galvanism to surpass in efficiency all others. At first glance, it would seem that the objects to be attained by this agent could be best effected through *direct* galvanization of the brain. This, however, is impracticable. A direct current, of sufficient intensity to influence the brain, would prove injurious, on the principle that enfeebled organs will not bear strong stimulation. Moreover, we cannot, with the utmost care, prevent the occurrence, at times, during galvanization of the brain, of giddiness, ocular flashes, faintness, etc., all undesirable phenomena, to be avoided in the electrical treatment of functional enfeeblement of the brain. If, on the other hand, we employ a current so feeble, that occurrences such as those enumerated become impossible, or, at least, very remote, and that strong stimulation is out of the question, it becomes very questionable, to say the least, whether a current reaches the brain at all, and if so, it will be too insignificant to accomplish any therapeutic results. His own experience with galvanization of the brain, has long since led him to abandon it in the initial treatment of cases of enfeeblement of that organ.

The manner in which he seeks to effect the *brain* is by *general* galvanization, administered in its only perfect form—the galvanic bath. By its means, the brain is made to sustain a reflex, or indirect influence, from all points of the periphery,

and, at the same time, through derived currents, a mild direct impression, which can be regulated at will; not only by modifying the intensity of the current, but also by establishing more or less perfect communication between the occiput and the water of the bath. This direct influence differs from ordinary cerebral galvanization, whether bi-polar or uni-polar, in that here no electrode is applied directly to the cranium, which, during the entire process, is never, for a moment, in a direct line between the two poles of the battery. It is never attended with any of the undesirable phenomena enumerated above as not always unavoidable in local galvanization. There are, indeed, no subjective sensations during the bath, to indicate that the brain is being acted on at all. Nevertheless, an interruption, or reversal of the current, will promptly produce the galvanic taste, and, if the current has sufficient tension, ocular flashes, etc.; thus establishing, beyond doubt, the fact that the cranial nerves participate fully in the galvanic influence. The galvanic bath, then, of appropriate intensity for each individual case and stage, he looks upon as one of the most valuable remedies for the treatment of cerebral exhaustion. Being at one and the same time, a suitably modified unipolar direct current and a reflex stimulus from every point of the periphery, to bear on the brain, it stands unique among electro-therapeutic procedures. The electric bath meets all the indications of the disease but one, namely, special nourishment for the brain. In addition to an appropriate diet, this may be best supplied by the exhibition of either phosphorus or cod-liver oil, or both, according to indications in individual cases. The phosphorus may be given pure (in pill or solution), or as phosphide of zinc. When electricity is employed, he considers medical stimulants superfluous, nor, with one exception, does he know of any other remedial agents, in addition to those enumerated, from which special benefit might be expected. The exception alluded to is "the bromides," which may be advantageously given in cases that are characterized by inability so extreme, and hyperæmia so decided as to indicate the employment of some additional remedy to combat these special conditions.

As to the mode of administration of the bath in the treatment of cerebral neurasthenia:

It is impossible in this respect to lay down any routine formula either in regard to the duration of the bath, the temperature of the water, or the intensity or duration of the current. Each case has its own laws.

Very mild currents should be employed in the beginning; as recuperation advances stronger currents may be gradually introduced. The intensity of the currents should be carefully regulated to keep pace with the gradually increasing capacity of the various organs to respond to the electric stimulus without detriment. Both currents may be used from the beginning, although the faradic current is strictly necessary only when paretic or sub-paralytic conditions exist. The galvanic should precede the faradic, and should be employed not more than two minutes. When irritability is a feature of the case the current should be descending; otherwise ascending. This may be followed by the faradic, not of sufficient intensity however for the first few baths to cause any but slight muscular contractions. In most of the cases iron may with advantage be added to the bath. The duration of the bath should at first not exceed fifteen minutes; in some cases even this is too long, the patient complaining of being fatigued, perhaps after the lapse of ten minutes. When this is the case, the bath should at once terminate. It is in these cases, not the electric current but the warm water bath, that gives rise to the sense of fatigue. Later in the treatment the duration of the baths may be from twenty to twenty-five minutes, according to indications.

At a meeting held Nov. 3d, 1876, Dr. A. D. Rockwell made a few observations on the "Differential Indications for the Use of the Faradic and Galvanic Currents," of which the following is a synopsis:

The differential indications for the use of faradism and galvanism demands the closest scrutiny, for on the accuracy with which we estimate these indications will largely depend the success of our efforts.

An intelligent estimate of the point demands both a knowledge of the physical and physiological distinctions of the currents, and an experience that has not only been sufficiently extensive and varied, but that has been carefully and systematically formulated.

The greater mechanical effects of the faradic current render it powerfully tonic in its action, and the method of general faradization is indicated in many cases of nervous exhaustion and localized faradization in the mal-nutrition and atrophy of muscles. The galvanic current, by virtue of its greater power of overcoming resistance, and through its reflex tendencies, is indicated when we wish to act upon the central nervous system, and from its superiority in exciting nerve irritability we use it to produce contractions in paralyzed muscles that fail to respond to the faradic.

We must, however, to a very considerable extent rely upon the aid afforded by repeated clinical observation. In a practical review of the subject we naturally consider successively those diseases, or symptoms of disease, which seem to demand the faradic current; those that call for the galvanic; and, lastly, those in which both currents are frequently and interchangably indicated.

I. Concerning those diseases that seem to demand the faradic current alone there is but little to be said.

There are in various generic diseases specific symptoms that invariably demand one or the other of the two currents, and even special qualities of current, but there are few distinct organic or functional conditions that in every phase of their manifestation demand alone and always any special form of electricity. Asthenopia, a condition depending on an absolute or relative deficiency of energy in the muscles of accommodation, and accompanied by hyperæsthesia of the retina and ciliary nerves, is about the only distinct disease that demands the faradic current alone.

II. There is also little to be said concerning the exclusive use of galvanism, although it certainly has a somewhat wider range. I would designate spinal irritation, certain sequelæ of cerebro-spinal meningitis, and most of those skin affections in which electricity has been shown to be of service, as the distinct diseases in which the galvanic is invariably superior to the faradic current.

III. Those diseases in which either current may prove equally of service, or where at one stage of the symptoms the galvanic, and later the faradic current is in-

dicated. In hemiplegia, where there exists, as is so often
the case, an exalted electro-muscular contractility, elec-
tricity if used at all, should be used in the form of faradiz-
ation, and with an exceedingly mild and rapidly in-
terrupted current; even when muscular contractions are
somewhat less readily called out than in the normal condition,
the same current is as a rule preferable. When on the con-
trary there is a very great diminution, and even relatively to
the faradic current, a complete loss of electro-muscular con-
tractility — the galvanic current is indicated — the faradic
coming into play only when the muscles give evidence of some
reaction to its influence. In paraplegia, whether depending
upon structural changes in the cord, or upon causes that result
in simple anæmia or hyperæmia, we generally find after a
short time, complete or approximate loss of farado-muscular
contractility. The galvanic current is, of course, alone appli-
cable in these cases, for the specific purpose of restoring nerve
excitability, although the faradic is useful in attempts to im-
prove the impaired nutrition of the paralyzed member. The
difference in the reaction of the two currents is typically illus-
trated in some forms of facial paralysis, and especially when it
results from the action of cold, (*rheumatismal*) or compres-
sion. In these cases the faradic current does not cause con-
tractions, while not only do the muscles respond to the
galvanic, but a much weaker current will answer, than when
the parts are normal.

As the patient improves, it takes an increased tension of
galvanism to cause the same effects, until finally farado-muscu-
lar contractility becomes manifest. The experiments of Erb,
and after him of Ziemssen and Weiss seemed to show that
after the laceration or division of the sciatic nerve in a rabbit,
the excitability of the muscles through the first week became
diminished for both currents, but subsequently while farado-
muscular contractility became more and more feeble, gal-
vano-muscular contractility rapidly increased, until two
cells caused contractions. These results are interesting as
illustrations of how clinical facts may be reinforced by electro-
physiological experiment. In the essential paralysis of child-
hood, the farado-muscular contractility is generally diminished

and often abolished, while occasionally the galvanic current, as in facial paralysis from cold, produces contractions more readily than in health. If the muscles respond in any marked degree to faradization, it should be used, if not, galvanism is indicated.

The relief of pain whether of a pseudo-neuralgic or hysterical character, or whether dependent on true neuralgia or other causes, is a very important function of electrization ; but in no condition has it been more difficult to discriminate correctly in the selection of the proper method of electrical treatment. True neuralgia as defined by Anstie, is without doubt most successfully treated by galvanism, whilst hysterical neuralgia, and the so-called pseudo-neuralgias, which are simply forms of pain, occupying certain areas and running seemingly in the direction of certain nerves, yield most readily to faradism. More specifically, the effects of pressure in the various forms of neuralgia are exceedingly useful, as guiding symptoms, indicating the proper treatment. I do not by any means lay it down as a universal law, but it will certainly be found, that in the great majority of cases of neuralgia, where *firm* pressure over the affected nerves aggravates the pain, the galvanic current is indicated, while the faradic current has the greater power to relieve, when such pressure does not cause an increase of pain. In the class of cases called sometimes hysterical hyperæsthesia, it is well-known that firm and prolonged pressure affords marked relief, while pressure superficially applied, increases the distress. The faradic current is here infinitely superior to the galvanic.

In the treatment of the pain of herpes zoster galvanism is invaluable. In many cases that have fallen under my observation, I have never known it to fail to afford either complete or approximate relief. The effects of galvanism on the extreme suffering that so often accompanies mammary cancer are often little short of magical. I have in many instances seen the acutest agony relieved instantly, and while the relief is necessarily, seldom if ever permanent, it is possible in many cases, by repeated applications, to keep the pain in abeyance for months, and thus the necessity of constantly administering opium, is in a measure obviated. In the relief of neither of

the last named diseases have I found faradization to be of essential service.   As we advance to the consideration of those other forms of disease which experience has shown to be more or less amenable to electrical treatment, it will be found to be more difficult without submitting the patient to preliminary and tentative applications, to discriminate between the currents best adapted to the case in hand, but I venture to assert, that in cases of chorea, of amenorrhœa associated with anæmia and debility, and in cases of nervous exhaustion in general, we cannot often err, if we resort to the faradic current by the method of general faradization.

<div align="center">THE MEDICO-LEGAL SOCIETY.</div>

Testamentary Capacity of Monomaniacs.—Considering the interest which has been awakened by the remarks of Dr. R. L. Parsons, contained in my last communication on the testamentary capacity of monomaniacs, and of the importance of the subject, I am constrained to forward you the views of several eminent lawyers of this city—members of the Medico-Legal Society—upon the legal side of the question.

Hon. Geo. H. Yeaman said he had intended, at that meeting, to ask from the medical side of the house, a discussion of the question: " How far does pronounced partial insanity raise a suspicion, or a presumption, of a general mental impairment?" In other words, can a man be wholly mad or deluded upon some one important subject, and be wholly sane upon all others? But Dr. Parsons had anticipated him.   And he was bound to admit, for some time past he had been inclined to modify the established and strictly legal view, and to adopt, as a scientific conclusion, the view indicated by Dr. Parsons.

How can the man who goes about proclaiming that he is the Christ Jesus, be either known or believed to be sane upon all other subjects?

Take the case of a man thinking he was daily and hourly pursued by a mad dog, barking at his heels, and expressing the most painful fear of hydrophobia.   He is taken sick, is attended by an only daughter, makes his will, dies, and cuts her off, leaving all his estate to his robust sons.   This man was a monomaniac upon the subjects of mad dogs and hydro-

phobia, but he is found to be sound on all other subjects, and therefore his monstrous will must stand. He may have thought his daughter put poison in his water, or his medicine. But there is no proof of *this* delusion; and the will not being, on the face of it, affected by the delusion of the barking mad dog, the will must stand—outrageous and unnatural as it is. The speaker admitted that there was another side to this question. The doctrine indicated might prove a two-edged sword. It might interrupt, almost prevent, the administration of criminal justice.

Must the man who says he is the Christ Jesus, or who thinks the mad dog is pursuing him, but who seems sound on other questions, be excused for murder, arson, or theft, upon the ground of insanity? Here is the danger and the difficulty.

He mentioned these things to call attention merely to the extreme intricacy and difficulty of the question; and he did not regret that we spend so much of our time upon this and kindred subjects. It is, to-day, the most difficult and delicate question that engages the attention of science and law-makers, and he thought that we might hope that the discussions conducted here would at least help to direct science and legislation to correct and humane conclusions.

Mr. F. R. Courdert confessed to being somewhat startled and alarmed at the declaration that has fallen from the lips of the learned medical gentleman (Dr. Parsons). If he understood that gentleman correctly, he declares it as his opinion that where partial insanity, or monomania is proved, it must be assumed that the whole mind is diseased.

In other words: that there is really no such disease as monomania, or partial insanity. This is a very startling doctrine at this day, after the great study and attention that has been paid to this subject, and the results that have been reached by scientific men, and it seemed to him that a most dangerous revolution would be brought about, if courts of law were to accept it as their guide. If such a doctrine is sound, the will of a man afflicted with partial insanity is to be set aside upon the proof of that fact, even where such insanity has no connection, real or apparent, with the instrument in controversy. Then, indeed, the practice of setting aside wills will be the

rule, and their admission the exception. It is a very uncomfortable, and still a generally recognized fact, that very few men, if any, possess minds equally well balanced on all subjects; few, indeed, who never betray such weakness upon any particular subject, or group of subjects, political, religious, social, domestic, or personal, as not to make them open, at least to the charge of eccentricity, bordering upon the boundary line of insanity. What a temptation is offered by the theory of the learned doctor, to all who may be interested in magnifying such eccentricities into positive aberration of the intellect.

One of the leading physicians of this city, who has attained great distinction in the investigation of these subjects, recently narrated to the speaker the following instance which came under his personal observation:

A member of the bar called upon him and stated that he heard noises which haunted him night and day, and which at times resembled a human voice, and prompted him to do wicked and criminal things, against which his reason rebelled. The learned doctor commenced by an examination of his ear-drum, and found a deposit of matter, which he removed. With this deposit the noises disappeared. The disease vanished, and this man, who entered his office a monomaniac, departed with his reason restored.

Some months after, this same lawyer, in the course of an argument in court, grossly, and without provocation, insulted and threatened the presiding judge, who very properly committed him to jail. The unfortunate man's relatives called upon the doctor, who, having examined his former patient, came to the conclusion that he was suffering from the same disease as theretofore, and that his condition was attributable to the same cause. He was merely obeying the voices that prompted him to insult the court. The doctor stated the fact to the judge, who at once discharged the unfortunate man from imprisonment.

Now, suppose that this lawyer, on his way to the doctor's office, when he was pursued by those phantom voices, had died. Would his will have been void? It certainly would, according to the theory of the learned physician and of the gentleman

who spoke after him. It certainly would *not*, if the decisions in this State, some of which have been cited by Mr. Patterson, are to cover cases of this character.

Again, the learned physicians who are called as experts in these cases, concur in characterizing as delusion and as constituting a species of insanity, that condition in which a man fancies that he sees persons and hears voices that do not exist. Within a few months one of the wisest and most experienced lawyers of our day passed away. During the last thirty years of his life, he was as fully convinced that he constantly saw and conversed with persons long since dead, as he was of his own existence. Not only was he imbued with the *doctrine* of Spiritualism, but for him the reality of such apparitions was undeniable. This was undoubtedly what the doctors have constantly called " a case of insane delusion," and yet he was at all times a wise, prudent, sagacious, practical adviser. No lawyer, said the speaker, would pretend, and no court, he was very sure, would hold that this gentleman's will must be deemed void, simply because of the existence of such a delusion. He admitted that if, in the will, the influence of that delusion was clearly traceable, and it was shown that he had been influenced thereby to such an extent that his posthumous bounty was diverted from its legitimate and proper channel, then the will must be declared the product of the delusion, and therefore void. But if in regulating his affairs, with a view to a distribution of his estate after death, he showed a clear comprehension of the various claims of all the persons who might properly look to his bounty, and there was no trace whatever of the hallucination under which he labored during his life, then the courts must hold the instrument to be good.

He was aware that many years since Lord Brougham held very much the same opinion as the learned doctor, but that opinion has found no followers and has never been cited, that he was aware of, with approbation in the leading cases of the day.

In conclusion, he would say, without entering into any discussion as to the scientific aspects of this question (with which aspects the learned doctor is infinitely more familiar),

that the rule suggested by the lecturer of the evening is the true rule, and cannot be varied or departed from except by abandoning those landmarks which have guided courts and text writers for many years past, and which rule, upon the whole, with all its imperfections, is the safest, the most practical and the best.

Dr. Parsons said, that judging from the descriptions given, a great majority of the cases to which the legal gentlemen had referred as being monomaniacal, were probably not insane at all.

Mr. Patterson arose and said the course of the discussion had been directed to two matters to which he intended to give prominence, namely: *First*, what rule can the courts with safety apply to the determination of mental capacity in monomaniacs. *Second*, the disposition of some persons to attempt to find in cases of monomania, the evidences of a general insanity. He referred to the cases cited by medical gentlemen who had spoken, and said that in them were clearly exhibited indications of partial insanity merely. Now if the mental processes of those people were astray only as to one topic or class of topics, and it was established that in all other matters the individuals were rational and intelligent, that they apprehended their respective situations, and understood the claims of their kindred upon them, and there could not be descried in the will any relation to or evidence of the influence of the monomaniacal delusion, what gross injustice it would be to say: "Yes, these men have made reasonable wills — they have provided for their families — but they were peculiar in entertaining some delusions which affected their thoughts, but have had no perceptible influence upon their testamentary acts; therefore, they shall *not* be permitted to dispose of their estates. They can have no privilege of expressing preference — the law must make their wills."

Now, in all departments of science, where rules of general application are to be reduced from multitudes of facts, the results of the observations of many persons, the main difficulty is in formulating the rule, which must, of course, represent general facts and not mere exceptions. In these cases of monomania, the courts have been keenly alive to the necessity of

defining the conditions under which they will sustain the wills of monomaniacs. The decisions have varied, but it must always be borne in mind, that the rule as lawyers now understand it, is based exclusively upon the teachings of the accepted medical authorities on the subject, and those teachings, have of course preceded the adoption of the rule.

The necessity for a general rule on this subject is admitted by all to be imperative. Now where can a rule be found which shall protect the family of one partially insane from the extravagances of his delusions, at the same time reserve to him the right to dispose of his property as he chooses? The sentiment of the legal profession seems to be that to avoid the will of a monomaniac, (keep in mind the definition of a monomaniac) it must be a will which discriminates against his wife and children or other relatives, if he has any, and it must be a will made under the influence of a delusion.

But there is, of course, fair ground of discussion as to the reasonableness of the rule indicated. What surprises lawyers, however, is to hear such views as have been urged by the learned doctor (Parsons). If he is correct, then all the antecedent learning, experience and knowledge of both professions must be so much error and waste; then there is really no distinction between general and partial insanity; there is no such thing as monomania, except as a mere word utterly meaningless, and that is the proposition of which lawyers justly complain; viz., the disposition to find evidences of general insanity in case of monomania. If this is persisted in, you must lay down a rule which shall be as rigid in its exclusion of testamentary power, as the old English rule was in its inclusiveness, and that would be merely to introduce chaos into the law again. The reasonable rule lies where truth generally does, in the mean between the two extremes.

Reviews and Bibliographical Notices.

I.—HERBERT SPENCER : PSYCHOLOGY.

The Principles of Psychology. By Herbert Spencer.
    2 Vols.   1876.

It would be hardly possible to profitably study Mr. Spencer's
Psychology, apart from his system as a whole, but more
especially from his *Principles of Biology*, which immediately
precedes psychology in the order of development of his scheme
of the philosophy of evolution.

There is a dominant idea which more or less consistently per-
vades his system from beginning to end, which should be
apprehended at the outset, in a study of any of its parts.   In a
consideration of his Psychology, it is hardly less important to
understand Mr. Spencer's mode of explaining the genesis of
vitality or life, which in his hands, in the course of a progres-
sive evolution ripens finally into mind.

And such in brief will be our course in the present attempt,
to estimate in some respects the scientific and practical value of
his psychological doctrines, for such of our readers as may not·
have made an independent study for themselves.   This is all the
more necessary in a Journal like our own, at a time when a new
edition of the author's writings is issuing from the press, and
when it is remembered that his views have been more or less
largely adopted by British and American medical psychologists.

Then, as for ourselves, we have not been able to accept as
freely as some others have done, much that belongs to his
methods and reasonings, and hence much that is found in his
conclusions.   And this statement is made, we trust, in full
view of the eminent service Mr. Spencer has rendered in various
important fields, more particularly in aiding the systematic or
generalizing side of science, at a time when such a work was
greatly needed.

It would seem to have been a rare occurrence to have met
with minds, capable on the one hand of the fruitful observation
and study of individual facts, and on the other, possessed of the
highest powers of philosophical generalization.   A Descartes,

a Bacon, a Bichat, and a very few others ranking below, but
worthy to be classed with them, exhausts the list in modern
times. It is too early, perhaps, to assign Mr. Spencer's relative
place within this class, even if he should be found to have just
claims to enter it. But this much can be said, that his chief
title to recognition appears to lie in his powers of generalization,
and hence as already said in his service to the systematic side
of science, or rather, we should say—human knowledge.

For his scheme certainly has the merit of comprehensiveness,
since it aims at nothing less than embracing within its confines,
the sum total of human knowledge, actual and possible, ranging
the materials of the same with a remarkable display of capacity
for analysis and synthesis, in subordination to the *law of evolu-
tion.* In the endeavor to execute this ambitious and herculean
task, he has succeeded beyond ordinary expectation, in impress-
ing his views upon and in quickening the thought of his times.
Multitudes of minds of this generation have been excited and
guided by his varied, and upon the whole, attractive writings.
Perhaps no philosophic writer of the past quarter of a century,
has said, or done more, if as much, to stimulate thought, in so
many healthy directions, as Mr. Spencer. But after all, speak-
ing for ourselves, after a pretty careful study of his works, we
believe we have found rather ample evidence, that he has
been unduly fascinated by the splendid generalities of his
system, and that he has been thus betrayed at times, if not
into loose observations, at least into loose assumptions, and
pressed by the exigencies and voids of his scheme, has in the re-
finement of his speculations, habitually trespassed on the
grounds forbidden to legitimate inference. Comprehensively
surveyed, his system appears to our eyes to present a universe of
suggestive conclusions on a very limited basis of facts.

The dominant idea in the philosophy of Mr. Spencer, is as
every one knows, that of *evolution.* After distinguishing the
knowable from the *unknowable,* in which he follows closely Sir
William Hamilton and Dr. Mansell, the chief aim of his work,
entitled *First Principles,* is to unfold the law or laws of evolu-
tion.

The conception embodied in this case, makes it necessary in a
survey of the universe to begin with the most fundamental
phenomena, or factors attainable by analytic processes, which
are for the physical world, two in number *matter* and *force.*
These, neglecting certain refinements, constitute the points of
departure of the evolution. This takes place by a series of
"differentiations," in either and between both, with subsequent
"integrations" and "re-integrations," as between portions of
matter or forms of force that have *somehow* come to differ from
each other. There results sooner or later, several special kinds
of matter and force, with various kinds of motion. But we
have never been able to see how these "differentiations," etc.,
were brought to pass. In every case they have to be assumed.

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While these various kinds of matter, force and motion, agree among themselves, according to their class, in important respects, they have *somehow* come to differ in properties in others. By re-acting on each other according to their differences, they give rise to other and more complex forms of matter and force. Each advance that is made in the "evolution," the results are more and more complex continuously, and so on *ad infinitum.* The procedure is from the "general" to the "special," from the "simple" to the "complex," from the "homogeneous" to the "heterogeneous." From the simple beginning as stated,—matter and force, and the capability of motion being given,—the whole system of the visible universe, organic and inorganic, animate and inanimate, is "evolved." The substance in which changes are wrought, is matter : the cause or condition of all change, is force, or as it would appear, motion.

It is the deliberate aim in the scheme of Mr. Spencer, to analyze all phenomena, whatsoever their kind, into one or other of these factors as their sufficient basis. Proceeding on such an assumption, that part of the natural world is first entered wherein we find matter and force or motion, in their simplest forms and manifestations — the *inorganic.* Guided through it by the idea of "evolution from the simple to the complex," the advance is made upwards through physics to chemistry and beyond, when we begin almost insensibly to meet with the more complex and special phenomena of life or vitality. The latter it is admitted are new, but a sharp analysis reveals the fact that they contain nothing new, or at least nothing worthy of mention, besides matter and force or motion. There has been simply a series of "differentiations," "combinations" "and unfoldings." The phenomena are more complex in their "relations of co-existence and sequence," and this is all.

Ascending now through the domain of living beings, under the guidance of the "law of evolution," from the lowest vegetable to the most exalted animal forms, we begin to meet with a new order of phenomena, usually called *mental.* These seem at first to be radically different from what had been met with before, but the difference is only a seeming. The "law of progress" or of "evolution from the simple to the complex," read inversely, proves equal to the emergency and we are led back on converging lines to the simple primal factors, matter, and force or motion. And so on to the end until all phenomena, physical, vital, intellectual and moral, have been subordinated to the sway of this comprehensive law.

Not only have the *objects* of knowledge fallen into a progressive order in obedience to this law, but it has also determined the relations, and hence the order of our knowledge. The classifications of the same, hitherto made, in which, except in view of subordinate distinctions, the sciences have been ranged in at least two parallel classes, "physical" and "mental", is consistently abolished in the scheme of Mr. Spencer. Be-

ginning according to the terms of the law above mentioned
with the simplest or most fundamental of the sciences, the
advance is made not on two co-ordinate lines simultaneously,
but on one line, from physics to chemistry, from chemistry to
physiology or biology, and from biology to psychology, and
from psychology or a study of the individual to sociology, or
the study of men in a state of aggregation — or in view of
their relations to each other,— finding somewhere within the
confines of *sociology* a place for higher morals and religion.

Let the mind of the student once become permeated and
swayed by a knowledge and the spirit of the law of evolution,
and with no more than ordinary imaginative power, he can rise
above the scheme and course of nature, which can be thus
brought under the eye of the mind in its entirety, and he may
glance in his mental vision, from the " star dust " of the most
primeval times, along the unbroken and ever-widening track of
evolution to its last and highest term, in the process of unfold-
ing; viz., *man.*

One of the notable features in this grand process is the implied
profession, that nothing is imported or admitted from without,
from the time of its beginning to the end. No new elements
are required or slipped in by the way. The entire stock of
materials and forces were taken up at the start, and they are as
we have seen, simply matter and force, physical force, or motion.
These furnish the warp and woof of the process and its total re-
sults or products. The mechanism of evolution is self-contained
and self-acting. It does not need and does not have any out-
ward aid or impulse, whether human or divine.

In the first part of Mr. Spencer's work, entitled *First Princi-
ples*, is laid the philosophical basis of his system. Under the
head of the " unknowable," he dismisses the gods, not only
from the sphere of human knowledge, but like Epicurus of old,
from the care of the universe.

Such is a mere outline of the system of Mr. Spencer, in its
widest generalities.

From this brief characterization, we pass to the special subject
in hand, that is, an examination in certain of its aspects of our
author's work, entitled *Principles of Psychology.* Speaking of
the facts of psychology, Mr. Spencer says, " before beginning
their study from a psychological point of view, we have first
to study them from a *physiological* point of view." (p. 14).
It is to this latter and possibly more practical phase of the sub-
ject, to which we would direct chiefly the attention of our
readers. In our own minds, we have a direct experience or a
consciousness of volition, thought, feeling, etc. This is the
field of psychology in its more restricted sense. But in a study
of mind from a stand-point as purely physiological as possible,
we study the *signs* of mental action and feeling from the out-
side, and hence must deal with the organisms of animals, more
particularly their nervous systems and the parts like the muscu-

lar system, most immediately subservient to it. This makes it necessary to consider the structure and modes of action of the nervous system, even in disease. And this is the point of view of most practical importance to the physician if not to the professed philosopher.

This course will lead us naturally to examine first in order Mr. Spencer's statements, respecting the mechanism and modes of action of the nervous system. The general account of the structure and modes of action of the nervous system with which the first volume may be said to open, though concise, is graphic and may be accepted as correct, as far as it goes.

The nervous functions: as a whole, being reduced in the last analysis to some form of motion, are divided by Mr. Spencer into the *recipio-motor*, or sensory, the *libero-motor*, or motor functions proper of the gray matter, and finally the *dirigo-motor* or the motor nerves which pass out to the organs to be set in action.

The cerebellum is held hypothetically to be "an organ of doubly-compound co-ordination in *space*, while the cerebrum is an organ of doubly-compound co-ordination in *time*." (P. 61, Vol. I.). In a note on the next page, Mr. Spencer adds :

" It should be remarked that the above-proposed definitions, are, to a considerable extent, coincident with current conceptions. The *cerebrum* is generally recognized as the chief organ of mind ; and mind, in its ordinary acceptation, means more especially a comparatively intricate co-ordination in *time* — the consciousness of a creature " looking before and after," and using past experiences to regulate future conduct. In like manner the function ascribed to the *cerebellum* in the foregoing paragraph, partially agrees with that which M. Flourens inferred from his experiments. It differs, however, in two respects. It implies that the *cerebellum* is not an organ for the co-ordinations of motions only, or of synchronous motions only ; but that it is also an organ for the co-ordination of simultaneous impressions, and for. the co-ordination of the synchronous motions in adaptation to the simultaneous impressions. And it further implies that not all simultaneous impressions and adapted synchronous motions are co ordinated by the *cerebellum;* but only the doubly-compound ones, which have for their external correlatives the intricate combinations of attributes that distinguish objects from one another, and the more multiplied and varied localizations of objects in the space that extends beyond the immediate limits and reach of the organism." (P. 62).

But we will refrain from remark on the adequacy of these generalizations of the functions of the cerebellum and cerebrum, until a later part of this notice, where they may receive attention.

" The conditions essential to nervous actions," are next considered. They are declared to be " continuity of nerve substance," the maintenance of a regular temperature at a certain level, differing, however, in certain animals; a regular supply of healthy blood, healthy in quantity and quality, and finally the absence of certain matters, the presence of which would disturb or destroy nervous action.

As regards the mode of action of the nervous system, the following passage, perhaps, in as concise a form as any other, expresses Mr. Spencer's views :

" Briefly reviewed from a somewhat different stand-point, the following are the leading facts which it concerns us to remember:

" Nervous stimulations and discharges consist of waves of molecular change, that chase one another rapidly through nerve-fibres. The stimulus or discharge formed of such waves, arises at some place where unstable nerve-substance has been disturbed ; *and is the same* no matter what agent caused the disturbance. The successive waves severally travel with a velocity which, though considerable, compared with ordinary sensible motions, is extremely slow, compared with other kinds of transmitted molecular motions. And each set of waves, while itself caused by the decomposition of unstable nerve-matter, is a means of decomposing other unstable nerve-matter ; so generating further and often stronger sets of waves, which similarly chase one another into many and distant parts of the nervous system.

" There is a triple rhythm in these nervous stimulations and discharges — each form of rhythm being due to the greater or less incapacity for action which an action produces. We have seen that every wave of isomeric transformation passing along a nerve-fibre, entails on it a momentary unfitness to convey another wave ; and that it recovers its fitness only when its lost molecular motion has been replaced and its unstable state thus restored. We have also seen that any portion of grey matter in a nerve-center, which having been disturbed and partially decomposed has emitted a shock of molecular change, is proportionately incapacitated ; and that it recovers its original ability only as fast as it re-integrates itself from the materials brought by the blood. And then there comes the further rhythm constituted by the alternations of sleep and waking — a rhythm having the same origin as the last, and being supplementary to it.

" The remaining truth which we have contemplated is that each special stimulation and the special discharge produced by it, do not together form the whole of every nervous act ; but that there is always an accompanying general stimulation and general discharge. Every part of the nervous system is every instant traversed by waves of molecular change — here strong and here feeble. There is a universal reverberation of secondary waves induced by the stronger primary waves, now arising in this place and now in that ; and each nervous act thus helps to excite the general vital processes while it achieves some particular vital process. The recognition of this fact discloses a much closer kinship between the functions of the nervous system and the organic functions at large, than appears on the surface. Though unlike the pulses of the blood in many respects, these pulses of molecular motion are like them in being perpetually generated and diffused throughout the body ; and they are also like them in this, that the centripetal waves are comparatively feeble, while the centrifugal waves are comparatively strong. To which analogies must be added the no less striking one, that the performance of its office by every part of the body, down even to the smallest, just as much depends on the local gushes of nervous energy, as it depends on the local gushes of blood." (P. 95, Vol. I.).

In this account as a mere hypothetical statement of a subject, which is open only to the incursions of inference, we see nothing to which to object. One merit it has, depends on attributing the passage of nervous impressions to vibrations, rather than to the *circulation of a nervous force or fluid.* This latter is a clumsy hypothesis in favor of which, not a single fact exists, and none but superficial analogies.

But we will now invite the attention of our readers to Mr.
Spencer's account of the mode of formation of the nervous
system. It occurs under the head of "*the genesis of nerves.*"
Not less than twenty pages in the aggregate are given to this
subject. in the *Biology* and *Psychology.*

The following passage is cited partly as an example of Mr.
Spencer's mode of treating such subjects. and of the extent and
the microscopic minuteness of detail. to which he carries his
physical speculations. even into the provinces of biology and
psychology. Let the reader not be wearied with the tedious ac-
count which follows. It embraces only certain selections from
the chapters devoted to the subject in hand. How then is the
nervous system formed, according to Mr. Spencer, and what is
the scientific and practical value of his speculations?

" *Supposing* the various forces throughout an organism to be previously
in equilibrium, then any part which becomes the seat of a further force,
added or liberated, must be one from which the force, being resisted by
smaller forces around, will initiate motion towards some other part of the
iorganism. *If* elsewhere in the organism there is a point at which force
bs being expended, and which so is becoming minus a force which it
efore had, instead of plus a force which it before had not, and thus is
made a point at which the re-action against surrounding forces is
diminished ; then, manifestly, a motion taking place between the first and
the last of these points is a motion along the line of least resistance.
Now a sensation implies a force added to, or evolved in, that part of the
organism which is its seat ; while a mechanical movement implies an ex-
penditure or loss of force in that part of the organism which is its seat.
      *   *   *   When there is anything in the circumstances of an
animal's life, involving that a sensation in one particular place is habitu-
ally followed by a contraction in another particular place — when there
is thus frequently-repeated motion through the organism between these
places ; what must be the result as respects the line along which the
motions take place ? Restoration of equilibrium between the points at
which the forces have been increased and decreased, must take place
through some channel. If this channel is affected by the discharge — if
the obstructive action of the tissues traversed, involves any reaction upon
them, deducting from their obstructive power ; then a subsequent motion
between these two points will meet with less resistance along this channel
than the previous motion met with ; and will consequently take this chan-
nel still more decidedly."

" To aid our conceptions, we will, as before (§ 19), take the rude analogy
furnished by a row of bricks on end, which overthrow one another in
succession. If such bricks on end have been adjusted so that their faces
are all at right angles to the line of the series, the change will be propa-
gated along them with the least hindrance ; or, under certain conditions,
with the greatest multiplication of the original impulse. For when so
placed, the impact each brick gives to the next, being exactly in the line
of the series, will be wholly effective ; but when they are otherwise placed
it will not. If the bricks stand with their faces variously askew, each in
falling will have a motion more or less diverging from the line of the
series ; and hence only a part of its momentum will impel the next in the
required direction. Now, though in the case of a series of molecules, the
action can be by no means so simple, yet the same principle holds. The
isomeric change of a molecule must diffuse a wave which is greater in
some one direction than in all others. If so, there are certain relative
positions of molecules, such that each will receive the greatest amount of
this wave from its predecessor, and will so receive it as most readily to

produce a like change in itself. A series of molecules thus placed must stand in symmetrical relations to one another — polar relations. And it is not difficult to see that, as in the case of the bricks, any deviation from symmetrical or polar relations will involve a proportionate deduction from the efficiency of the shock. and a diminution in the quantity of molecular motion given out at the far end. (But now, what is the indirect result when a wave of change passes along a line of molecules thus unsymmetrically placed? The indirect result is that the motion which is not passed on by the unsymmetrically-placed molecules, goes towards placing them symmetrically.) Let us again consider what happens with our row of bricks. When one of these in falling comes against the next, standing askew, its impact is given to the nearest angle of this next, and so tends to give this next a motion round its axis. Further, when the next thus moved delivers its motion to its successors, it does this not through the angle on the side that was struck, but through the diagonally opposite angle; and, consequently, the reaction of its impact on its successor adds to the rotatory motion already received. Hence the amount of force which it does not pass on, is the amount of force absorbed in turning it towards parallelism with its neighbors. Similarly with the molecules. Each in falling into its new isomeric attitude, and passing on the shock to its successor, gives to its successor a motion which is all passed on, if the successor stands in polar relation towards it, but which, if the relation is not polar, is only partially passed on — some of it being taken up in moving the successor towards a polar relation. One more consequence is to be observed. Every approach of the molecules towards symmetrical arrangement, increases the amount of molecular motion transferred from one end of the series to the other. Suppose that the row of bricks, which were at first very much out of parallelism, have fallen, and that part of the motion given by each to the next has gone towards bringing their faces nearer to parallelism; and suppose that, without further changing the positions of their bases, the bricks are *severally restored* to their vertical attitudes; then it will happen that if the serial overthrow of them is repeated, the actions, though the same as before in their kinds, will not be the same as before in their degrees. Each brick, falling as it now does more in the line of the series, will deliver more of its momentum to the next; and less momentum will be taken up in moving the next towards parallelism with its neighbors. If then, the analogy holds, it must happen that in the series of isomerically-changing molecules, each transmitted wave of molecular motion is expended partly in so altering the molecular attitudes as to render the series more permeable to future waves, and partly in setting up changes at the end of the series; that in proportion as less of it is absorbed in working this structural change, more of it is delivered at the far end and greater effect produced there; and that the final state is one in which the initial wave of molecular motion is transmitted without deduction — or rather, with the addition of the molecular motion given out by the successive molecules of the series in their isomeric falls.

" From beginning to end, therefore. the development of nerve results from the passage of motion along the line of least resistance, and the reduction of it to a line of less and less resistance continually. The first opening of a route along which equilibrium is restored between a place where molecular motion is in excess and a place where it is in defect, comes within this formula. The production of a more continuous line of that peculiar colloid best fitted to transmit the molecular motion, also comes within this formula; as does likewise the making of this line thicker and more even. And the formula also covers that final process by which the line, having been formed. has its molecules brought into the polar order which least resists, and indeed facilitates, the transmission of the wave.

" Otherwise, we may say that while each passage of a wave is the establishment of an equilibrium between two places in the organism, the

formation of this line of easy transmission is an approach towards equi-
librium between the structural arrangements of the line and the forces to
which it is exposed. While its molecules are so arranged as to offer re
sistance to the passing wave, they are liable to be changed in position by
the wave — they are out of equilibrium with the forces they are subject to.
Each approach towards an attitude of equilibrium is a change towards
diminished resistance. And so on until there are simultaneously reached
the state of structural equilibrium and no resistance.

"Carrying with us these conceptions, we now pass from the genesis of
nerves to the genesis of nervous systems. We will look at these in their
successive stages of evolution." (P, 511-520, Vol. I.)

Now, let the reader give attention to this lengthy extract, in
which Mr. Spencer labors, through a mass of pure conjectures,
fine spun physical hypotheses, and bare assumptions, to show
how nerves are developed or " evolved."

What real necessity was there for entering such a field, which
could only be traversed in such a manner?   Is there the slight-
est real proof that the process of the " genesis of nerves," so
elaborately worked out by Mr. Spencer, is the true one?   Not the
least, so far as we know, and we have had some opportunity to
make the acquaintance of such matters. Mr. Spencer begins
by " supposing" an *organism*, the various forces in which are
assumed for the sake of a case to be " in equilibrium."

He next *supposes* this hypothetical organism to " become the
seat of a further force, added or liberated."

He then supposes this " further force" to " initiate motion
towards some other part of the organism," because, as it is
assumed, it is " resisted by smaller forces around."   It is next
conveniently " supposed," that " elsewhere in the organism
there is a point at which *force is being expended*, and which
so is becoming minus a force which it had before, instead of
plus a force which before it had not, and thus is made a point
at which the reaction against surrounding forces is diminished."

All these gratuitous suppositions and pure assumptions being
granted, the conclusion inevitably follows, that " a motion tak-
ing place between the first and the last of these points, is a
motion along the line of least resistance."

Before we pass on, we must ask the reader to look once again
at the *premises* from which this conclusion has been deduced,
for we will soon see what use is made of it and we must remem-
ber its value.   Says Mr. Spencer, " a sensation implies a force
added to or evolved in that part of the organism which is its
seat," while on the contrary " a mechanical movement implies
an expenditure or loss of force in that part which is its seat."

Here we have an organism *assumed*, capable of feeling and of
motion.   The problem is to establish a nervous track, or in
other words a nerve-fibre to connect these two seats of action.
How is this track or fibre developed?

It is in the attempt to show how this is done, that Mr.
Spencer uses his " rude analogy of a row of bricks."

At first " their faces are all at right angles to the line of the

series." Then they are all knocked down. Next they are set up
again " with their faces variously askew." Then they are thrown
down again. At this point, Mr. Spencer passes over to a line of
imaginary molecules in the organism, connecting the assumed
seats of sensation and of motion in the supposed organism.
Though it is admitted that " the case of the series of molecules
can be by no means so simple," yet it is assumed the *the* " *same
principle* holds " in the one case as in the other. A shock is
" supposed " to pass along this " supposed " line of molecules.

If the molecules are placed by assumption unsymmetrically
or "askew," " it is not difficult," says Mr. Spencer, " to see, that,
as in the case of the bricks, any deviation from symmetrical rela-
tions will involve a proportionate deduction from the efficiency
of the shock and a diminution in the quantity of molecular
motion given out at the far end," or in other words, at the seat
of motion,— muscular motion.

Having granted an imaginary organism in the proper state,
and seats of sensation and motion, and conjectural shocks, pass-
ing in an assumed line of direction between them, and having
granted molecules "askew," or unsymmetrically placed, having,
we say, granted all these things in an entire absence of proof of
their having ever existed, in fact, the following question is
asked : " But now, what is the indirect result when a wave of
change passes along a line of molecules thus unsymmetrically
placed? The indirect result is that the motion which is not
passed on by the unsymmetrically placed molecules, goes
towards placing them symmetrically. To illustrate the case
in its present phase, recurrence is had once again to the " row
of bricks," to show how, though the members of the row may
stand askew in relation to each other, yet by repeatedly falling
against each other, they tend to bring each member squarely
into the series. Says Mr. Spencer,—" *similarly* with the mole-
cules." But what a host of assumptions lie back of this point
in our progress? " But one more consequence is to be observed."
It is, that " every approach of the molecules towards symmet-
rical arrangement increases the amount of molecular motion
transferred from one end of the series to the other."

After once again reverting to the "rude analogy " of the row
of bricks, Mr. Spencer says, " *if, then, the analogy holds*, it must
happen that in a series of isomerically-changing molecules, each
transmitted wave of molecular motion is expended partly, in so
altering the molecular attitudes as to render the series more per-
meable to future waves, and partly in setting up changes at the
end of the series: that in proportion as less of it is absorbed in
working this structural change, more of it is delivered at the
far end and greater effect produced there; and that the final
state is one in which the initial wave of molecular motion is
transmitted without deduction,—or rather, with the addition of
the molecular motion given out by the successive molecules of
the series in their isomeric falls."

Now, once again let the reader pause, and assure himself as to what has been really shown in this labored, conjectural account of the process by which the "genesis of nerves" is accomplished, and see whether he is mentally prepared for the next step taken by Mr. Spencer. Surely this cannot be even an exhaustive imaginative account of the genesis of a nerve. What real light has been thrown on the process, or what fact or facts placed in clearer relations by these imaginings of Mr. Spencer? But what is the next step taken in the course of his exposition? It is as follows : "From beginning to end, *therefore, the development of nerve results from the passage of motion along the line of least resistance, and the reduction of it to a line of less and less resistance continually.*" And *this* is the way nerves are formed! But again, "The first opening of a route along which equilibrium is restored, between a place where molecular motion is in excess and a place where it is in defect, comes within this *formula*. The production of a more continuous line of that peculiar colloid, best fitted to transmit the molecular motion, also comes within *this formula*, as does likewise the making of this line thicker and more even. And the *formula* also covers that final process by which the line having been formed, has its molecules brought into the polar order which least resists, and indeed facilitates the transmission of the wave." "And so on until there are simultaneously reached the state of structural equilibrium and no resistance."

Finally, says Mr. Spencer, "carrying with us these conceptions, we *now* pass from the genesis of nerves to the *genesis of nervous systems!*"

This passage is referred to, partly because it shows what are Mr. Spencer's views as to the mode of development of the nervous system, and partly because it is a fair sample of his *mode of working out* his scheme of evolution. It exhibits also to what extent the speculative and generalizing tendency prevails in his work. The passage we have cited is not an uncommon one. It would be easy to cite dozens from his writings just as striking. It will be needless to tell us that it was never intended as more than a mere speculation, by its author, and should be treated as such, and that after all, speculation is not only justifiable but necessary in the advance of science. It is true that speculation within healthy limits is the life of scientific progress, as out of its place it is one of its greatest banes. One of our chief complaints against the work of Mr. Spencer, is that he has to an unsafe and dangerous degree, substituted fact by conjecture, solid premises by pure assumptions, and has used, perhaps, unconsciously, his splendid capacities and great influence in imparting an appearance of solidity to the structure, to the rearing of which his life has been chiefly devoted, and which *may* prove in the future to have anticipated the truth of discovery, but which reposes at present in large measure on unsubstantial conjectures and fine-spun hypotheses. The tone and even the terms of the

closing sentences of the passage given, if they were read before those which precede them, would leave on the mind of the reader, the impression that Mr. Spencer had actually shown how nerves are generated. But what are the facts of the case? An organism is *assumed* with a virtual nervous system to begin with. For does it not have " tissues " and a place and capacity for sensation, and a place and a capacity for motion — presumably muscular motion—and intervening between these parts or places, a " line of that peculiar colloid," capable of transmitting motion from the one place to the other, along a line of molecules fitted to transmit it? What is this, substantially, but a nervous system? And yet what is done by Mr. Spencer but *assume* all these things? All the functions of a simple, nervous system being assumed in the outset, it would seem to be a task unworthy of the name of either science or philosophy, afterwards to gravely construct a mechanism through which these functions are to be accomplished. And we make these strictures in view of the fact that many of the lowest animal organisms do not have either demonstrable nervous or muscular tissues, and yet are capable of some kind of feeling and of motion. If the evolution hypothesis is true, it must be admitted that the time should come in the course of a progressive development, when the apparently structureless lower animals would begin to show some signs of nerve tissue. And it was doubtless the aim of Mr. Spencer to hypothetically describe this process of development. But it is not to the attempt to explain it to which we object in this particular case, it is to the assumption in the outset of an organism performing all the functions of a nervous system, but which is denied a corresponding mechanism. We object to the explanation as wholly inadequate and even superficial, and also to the degree of confidence placed in its sufficiency by its author, as unjustifiable by the premises or course of reasoning on which it is based. It is a harm to the sober and healthy progress of science to employ under the sanction of so high authority, so freely, mere conjecture or speculation, the truth of the results of which it is plainly impossible ever to reach, or lies in the uncertain future. Such prolific use of hypothesis, no doubt is suggestive, and *may* lead the way to important truth, but is far more likely to lead even the best minds into the mazes of error.

All through the subsequent chapters of the work, Mr. Spencer refers the reader to his exposition of the " genesis of nerves," in terms which show he regards it as having a higher value than that of a mere hypothesis.

We ought not to omit to mention, that at the close of the next chapter, Mr. Spencer endeavors to meet in part the objections we have urged, but as we think not successfully.

In the next two or three chapters, Mr. Spencer devotes his work to simple-compound and doubly-compound nervous systems, and in which he conforms so far as facts are concerned, to

known anatomical and physiological data. The portions of these chapters which are novel or peculiar to Mr. Spencer, are almost wholly speculative, and in great measure, so far as we can see, devoid of scientific or practical value. As a mere connected exposition, its warp and woof is too exclusively conjectural. But any further remarks on these subjects, and on our author's treatment of the themes of Psychology proper, must be deferred until our next issue.

[TO BE CONTINUED.]

II.—THE FUNCTIONS OF THE BRAIN.

THE FUNCTIONS OF THE BRAIN. By David Ferrier, M. D. With Numerous Illustrations. p. 323. New York: G. P. Putnam's Sons, 1876. Chicago: W. B. Keen, Cooke & Co.

For good reasons, the central nervous system, more especially its intra-cranial portion, is attracting at present, a larger share of attention from anatomists, physiologists and pathologists than any other part of the organism. A greater amount of unexplored territory lies within the confines of the spinal cord and brain, under whatever aspect they are considered, than is to be found, it is probable, in any other part of the body. It is, hence, at this time, one of the most tempting domains open to investigation, and the one most likely to yield rewards both scientific and practical, in the immediate future. And this little work of Dr. Ferrier's is one of the best fruits of its study. It is, in a sense, an epoch-making book. It is not only valuable on account of the novel facts it contains, but because of its *suggestiveness*. It is, in large part, made up of physiological facts discovered by the author himself, with sensible and often acute reasonings on them.

We propose to make this notice the occasion for a rather extended review of the present condition of the physiology and pathology of the central nervous system, more particularly of the brain; and hence, while attention is given chiefly to the present work, we shall not hesitate to refer to researches not mentioned in its pages. Most of the facts given in the work, as well as in other recent publications, we have had occasion to notice and discuss in the pages of the JOURNAL, but this will not prevent us from giving a connected statement of those believed to be the most valuable, nor from attempting to estimate their scientific and practical bearings. In the endeavor to carry out this design, it will not be practicable for us to enter on a

history, even of recent progress in regard to the anatomy and
physiology of the central nervous system. To do this satisfac-
torily, would exceed, the limits of the present notice. But with
this declaration, we must pass at once to the subject in hand.

The work contains thirteen chapters. It may be of ser-
vice to some of our readers, to give their titles. They are, re-
spectively: I. *A Sketch of the structure of the Brain and Spinal
Cord;* II. *Reflex Functions of the spinal cord;* III. *Functions
of the medulla oblongata;* IV. *Functions of the mesencephalon
and cerebellum;* V. *Functions of the optic lobes, or corpora
quadrigemina;* VI. *Functions of the Cerebellum;* VII. *Func-
tions of the cerebrum;* VIII. *Phenomena of electrical irrita-
tion of the cerebral hemispheres;* IX. *The Hemispheres con-
sidered physiologically;* X. *Functions of the basal ganglia;*
XI. *The Hemispheres considered psychologically;* XII. *Diagram-
matic summary;* and finally, XIII. *Cerebral and cranial
topography.*

The preliminary "Sketch of the structure of the brain and
spinal cord," is given very properly " as a guide to the details of
experimental investigation." In discussing the "exact localiz-
ation of the sensory and motor tracts of the spinal cord," Dr.
Ferrier seems inclined to adopt the results arrived at by Woros-
chiloff, as regards the motor conducting tracts, that is, in assign-
ing this function chiefly to the lateral columns. This we believe
to be substantially the correct view, and for reasons which we
would be glad to give *in extenso*, but we must defer their state-
ment until another occasion. "The anterior columns are
regarded more as commissural connexions between the motor
nerves and adjacent segments, and not at least the direct paths
of motor impulses proceeding from the brain." (P. 4.) We do
not doubt the substantial correctness of this opinion; but the
statement seems to us to omit reference to one of the most im-
portant probable offices of certain of the fibres of the anterior
columns. We have been led, in our own studies, to look upon
them as part of the spinal apparatus of co-ordination. Our
reasons for this view, it is part of our purpose to give at some
length, at another time, in a review of Stilling's work on the
spinal cord. ("*Neue Untersuchungun Ueber den Bau des Rueck-
enmarks*," etc.)

Dr. Ferrier would seem (p. 4) to attribute to Schiff and Vul-
pian, the discovery that the gray matter is capable of conveying
sensory impressions; but the credit of this discovery belongs to
Brown-Sequard, rather than to the authors named. The opinion
of Schiff, that tactile impressions are conveyed upwards in the
posterior columns, does not seem to us well founded, for both
anatomical and physiological reasons. They are to be regarded
as probably subservient to the co-ordination of muscular mo-
tion, *through the medium of the muscular sense.* These columns,
it seems, are partly composed of longitudinal commissural fibres,
connecting the different spinal ganglia, represented in the gray

matter of the spinal cord, and corresponding to the different pairs of spinal nerves.   There is but little more of probability in the view of Woroschiloff, quoted by Dr. Ferrier, that the lateral columns contain the fibres of the sensory tracts.   This, if true at all, must be so to a very limited degree.   We regard the chief tract for the conduction of sensory impressions upwards, the length of the cord, toward the brain, to be *the gray matter*, and not so much the fibres of either of the columns.

   In the endeavor to trace upward, through the medulla, the motor and sensory tracts of the cord, Dr. Ferrier says " the motor paths undergo decussation at the anterior aspect of the lower extremity of the medulla oblongata, at a point termed the *decussation of the pyramids.*   At this point, therefore, the path of motor or efferent impulses, from the hemisphere, crosses to the opposite side of the cord."   This decussation, Dr. Ferrier recognizes, as others have done, as complete in man, but probably incomplete in certain of the lower animals, as has been shown by Philipeaux and Vulpian, among others.

   There is frequent reference, in this first part of Dr. Ferrier's work, to anatomical researches of Meynert, but those of Luys are scarcely mentioned.   Notwithstanding the fact that so much of the work of M. Luys is schematic, and notwithstanding the undue preponderance of hypothesis in his writings, we regard them as highly suggestive and valuable, though not the most reliable and satisfactory.   As an instance of this neglect of the results of the labors of Luys, we would refer to Dr. Ferrier's remarks on the probable cerebral destination of the superior crus of the cerebellum.   The view of Meynert is cited, that its fibres may contribute to the corona radiata; but at this point, a hint is given by Luys, which we consider of some value.   He calls attention to the fact that there is a rather remarkable agreement histologically, even in the matter of tint, between certain cell groups in the cerebellum and the "red nucleus" in the corpus striatum.   This fact of agreement in histological details may throw some light on the cerebral termination of the superior crus of the cerebellum.   The conjecture warranted by this fact, if it be such, does not, so far as we can see, conflict with any known anatomical and physiological data.

   In speaking of the reflex functions of the nervous system, and especially in the simplest forms of the nervous system, as in the ascidians, we notice, *en passant*, that Dr. Ferrier adopts the views of Kovalewsky, Darwin, and others, that the ascidians are to be regarded as "the ancestral type of the vertebrates."   Dr. Ferrier adopts the views of Schiff, and that of one of his pupils, Herzen, as to a certain mode of arresting reflex action.   If a single sensory nerve should be irritated, it may be followed by reflex action.   If so, the action that would naturally result may be arrested, according to these authors, by irritating a sensory nerve in some other part of the body.   The tendency to reflex action in one part is neutralized, in some way, by the tendency

simultaneously developed by irritation elsewhere in the nervous system. Now, though this is a valuable fact, yet it does not, in our opinion, have the force of a law, in nervous physiology, as Dr. Ferrier would seem to imply, in his mode of stating it.

Dr. Ferrier reviews at some length, the facts relating to the reflex adaptive action of the spinal cord, and which were so prominently brought into notice by Pflueger, some time ago. (*Die Sensorischen Functionen der Rueckenmarks der Wirbelthiere.* Berlin, 1853.) The latter held that the various complicated actions a headless frog can perform are to be looked upon as evidence that the spinal cord is possessed of mind; and hence, is endowed with consciousness, will, etc. But the author sides with Goltz and others who ascribe such actions to *reflex adaptive action* of the cord, and as it seems to us, correctly; but this question is not without its difficulties.

In the remarks on the medulla oblongata, there are no points worthy of special mention. Dr. Ferrier, however, does not share in the rather strange opinion of Vulpian, that the medulla oblongata is the seat of the emotions. Dr. Ferrier, moreover, in his remarks on the seat of a vaso-motor center in the medulla, would seem to imply that it is the sole vaso-motor center in the cerebro spinal axis. He says "the vaso-motor nerves pass (from this center) by the spinal cord, to the blood vessels, etc." But this statement disregards the fact which we have long taught, that a line of such centers extends down the whole length of the cord to its lower end. The chief vaso-motor center located in the medulla, dominates the line of centers mentioned; just as the brain, in some of its parts, dominates the gray motor centers in the cord, which are related to the voluntary muscles by the motor nerves. By means of this view, we can explain the observations of Vulpian, quoted by our author, which go to show that even after it has been severed from the medulla, and has been in this way, cut off from the influence of the supreme vaso-motor center, lesions of the cord modify the circulation of the blood, in certain parts of the body, in the same direction, too, as that of lesions of the medulla.

Next in order, Dr. Ferrier proceeds " to the consideration of those parts of the encephalon which lie between the cerebral hemispheres and the medulla oblongata, comprising the *pons varolii, corpora quadrigemina, and cerebellum.*" The functions of these parts of the central nervous system "may be determined by a study and analysis of those forms of activity which are manifested by animals, when all the centers situated in advance of the corpora quadrigemina, or optic lobes, have been removed." And such is the subject and the method of this chapter.

Dr. Ferrier gives an account of the phenomena exhibited by different species of animals after the removal of the hemispheres to the extent above described. His descriptions refer to the frog, carp, pigeon, rabbit, guinea-pig, and do not materially differ from those of other physiologists since the time of Flourens.

The lower down the animal is in the scale of existence, the more completely does it retain its functions when mutilated in the manner just supposed. The higher animals operated on, such as cats, dogs and monkeys, seldom survive the shock of the operation, and if they do, never recover to the extent that seems possible among the lower animals. Simply for the sake of rendering the subsequent discussion comprehensible, we will quote our author's account of the phenomena, shown by the rabbit after removal of the hemispheres.

"When the hemispheres have been removed from a rabbit or guinea-pig, the animal, at first utterly prostrate, after a varying interval, begins to show signs of the retention of a capacity for the performance of actions of a considerable degree of complexity. It is observable, in the first place, that the muscular power of the limbs has become enfeebled, to a very considerable extent. The muscular weakness is proportionately much more marked in the fore, than in the hind limbs. The animal can maintain its equilibrium on its legs, though of a rather unsteady character; and the fore paws have a tendency to sprawl, or to be planted in irregular positions. If the equilibrium is disturbed, the animal is capable of regaining it. If the hind foot is pinched, the animal will bound forward, in the usual manner, until it strikes its head against some obstacle, or until the excitation has exhausted itself. No one, so far as I am aware, has observed that power to avoid obstacles in the path, manifested by frogs and fishes similarly treated. The rabbit, therefore, continues its flight, once begun, in a headlong and blind manner. The pupils, however still contract when a strong light is thrown into the eye, and the eyelids wink if the conjunctiva is directly menaced. A loud sound will cause the ear to twitch, and provoke a sudden start. Colocynth, or some equally unpleasant rapid stimulus, will cause movements of the tongue and muscles of mastication, in all respects resembling those characteristic of disgust, with efforts to get rid of the nauseous taste. Ammonia held before the nostrils will cause a sudden retraction of the head, or induce the animal to rub its nostrils with its paws. Not merely does the animal respond by certain movements, to a pinch or prick of its toes, or tail, but if the pinch is a little more severe, it will respond with repeated and prolonged cries, of that plaintive character with which all sportsmen are familiar, who have gone hare or rabbit shooting. Vulpian specially calls attention to the plaintive character of these cries, as distinguished from the brief cry which may be elicited when all the parts above the medulla have been destroyed. My own experiments entirely confirm the description which Vulpian has given to them. If the animal be left to itself, undisturbed by any form of external stimulus, it remains fixed and immovable on the same spot, and unless artificially fed, dies of starvation, like the frog, fish and bird, in the midst of plenty. If artificially fed, however, the animal may live an indefinite period.

"With the exception of the greater degree of muscular paralysis, and the diminished power of accommodation of movements, in accordance with sensory impressions in general, and with visual impressions in particular, the phenomena manifested by rodents deprived of their cerebral hemispheres differ little from those already described in frogs, fishes and birds. The power of maintaining the equilibrium is retained; co-ordinated locomotive actions and emotional manifestations are capable of being excited by impressions on sensory nerves, essentially, if not altogether, to the same extent in all.

"In cats, dogs, and higher animals, the prostration is so great, and there is such interference with motor power, that the independent activity of the lower centers, so far as relates to the maintenance of equilibrium and co-ordinated progression, practically ceases to exist; though the fact of emotional response to sensory impressions points to the conclusion that we have to deal, not with the complete absence, but only with the suspension of the other forms of functional activity. This conclusion is capable of substantiation by other facts, to be adduced when we come to consider, in a more detailed manner, the functions of the hemispheres." (p. 39–40.)

So much as to an outline of the facts observed after removal of the hemispheres; but, says Dr. Ferrier very truly, "When we turn from the consideration of the facts themselves to the theory of their explanation, we enter on a *questio vexata* of physiology and pathology." The position of Dr. Ferrier himself, in relation to the question in issue, is that the complicated actions certain animals may perform after removal of the cerebral hemispheres, is not to be attributed to the presence and action of mind, but to nerve reflex adaptive action. One of the chief reasons for this belief, lies in the fact, as it seems to be, "that in the absence of the cerebral hemispheres, the lower centers, of themselves, are incapable of *originating* active manifestations of any kind. An animal with brain intact exhibits a varied spontaneity of action, not, at least immediately, conditioned by present impressions on its organs of sense. When the hemispheres are removed, all the actions of the animal become the immediate and necessary response to the form and intensity of the stimulus communicated to its afferent nerves. Without such excitation from without, the animal remains motionless and inert. It is true, that some of the phenomena which have been described, would seem to be opposed to this view, but they are so in appearance only, and not in reality." (P. 40.)

Dr. Ferrier enters at length into the question, as to the presence of consciousness, as indicated by the plaintive cry of the rabbit, when it is injured after removal of the hemispheres, or the avoidance of obstacles by the frog, under similar conditions, etc. He is inclined to adopt the views of Goltz and of Lotze of Goettingen, that such actions are simply the result of an excitable mechanism, adapted, either hereditarily or by education,

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to perform them.   Longet, Carpenter, and others, have regarded
the mesencephalon as the seat of sensation (*sensorium commune*),
and the actions which take place through it, as their central
seat, are called *sensori-motor*, as is well known.   One of the
chief proofs of this position is drawn from a consideration of
invertebrates.   They perform actions, evidently conscious, it is
said, but, " have no true cerebral hemispheres, but only a series
of ganglia, homologous with the mesencephalic ganglia of ver-
tebrates."

But Dr. Ferrier denies the parallelism of the cases, and hence,
the validity of the conclusion founded on it.   He says quite
truthfully, that the assumed parallelism is "materially weakened
by the fact that invertebrate animals are capable of actions of
an *entirely different kind* from those of vertebrates deprived of
their cerebral hemispheres."   But if the cases *are* parallel, it is
manifest that with essentially the same nervous apparatus, they
ought to perform the same actions.   A higher vertebrate, de-
prived of its cerebral hemispheres, will not move, as a rule,
unless excited by some external impression, and will perish in
the midst of plenty.   But the lower vertebrates "manifest a
varied spontaneiety of action, under, as far as we can see, the
same external conditions; they *seek* their food, are capable of
education, and *learn to adapt* their actions, so as to seek what is
pleasant, and avoid that which is painful, faculties which are en-
tirely abolished by removal of the cerebral hemispheres in ver-
tebrates."   " From this," says he, " I would argue that the
ganglia of the invertebrates are not completely homologous with
the mesencephalic ganglia of vertebrates; for if they were so,
we should expect that not merely sensation, but also the other
psychical faculties should be manifested by vertebrates deprived
of their cerebral hemispheres, even though to a less degree.   But
it is not a difference in degree only which is obsrved, but a mani-
fest difference in kind.   It is probable, therefore, that in the
ganglia of the invertebrates there are nerve cells which perform,
in however lowly a manner, the functions of the cerebral hemi-
spheres in vertebrates."

From such considerations, Dr. Ferrier turns for proof of the
correctness of his position, to the phenomena witnessed in the
case of man, in such forms of disease as do not abolish conscious-
ness, but do abolish sensibility in one half of the body.   Under
such circumstances, *though sensibility is utterly lost*, reflex
actions take place as truly as ever.   In such a case, the actions
take place without consciousness.   In man, and the highest
vertebrates, we doubt not, these arguments represent the truth;
but in the lowest vertebrates, we are inclined to admit that the
mesencephalic ganglia may, as in the case of the invertebrates,
perform offices which belong to the hemispheres in higher ver-
tebrates.

But in the discussion of this question, we are met by practical
difficulties.   If we insist, with Dr. Ferrier, and many others

before him, that the actions performed by the higher vertebrates, which have been deprived of their hemispheres, are simply reflex and adaptive, and do not imply consciousness, the question arises, where can we rationally stop with this way of construing nervous action? If the complicated actions which can be performed by a brainless frog, are not to be regarded as an evidence of the presence and action of mind, what actions are an evidence of it? Why not explain all actions the same way? Is not all brain action reflex and adaptive? The discussion of this question will fall more naturally into a later part of this notice, when examining Dr. Ferrier's views as to the nature of brain action; and hence, until that time, we will dismiss it. But, on the contrary, if we affirm that the actions of the brainless frog are to be looked upon as evidence of the presence and action of mind, when shall we arrest this mode of interpreting nervous phenomena? Why not admit that the actions performed by the spinal cord, or even by the sympathetic ganglia, or ganglion cells, are an evidence of the action of mind? The fact of the case is, that the difficulties in the way of drawing a distinction between the action of the mesencephalon and spinal cord are so great, that some have been led to affirm that wherever there is gray nerve tissue there is mind. While we are by no means of the opinion that we are obliged to carry either reflex action everywhere throughout the nervous system, if anywhere, or to admit all gray nerve tissue as indicating the presence of mind, if any nerve tissue does, we will not, at this place, enter on a statement of our views; but we promise to attempt to discuss this subject at an early day.

We do not doubt but that all gray nervous tissue may be, and probably is, the seat of reflex action, we do doubt whether all mental action is reflex; but we promise to take an early opportunity to discuss this subject in a manner commensurate with its importance.

Some of these difficulties involved in the terms consciousness, sensation, etc., Dr. Ferrier thinks may be dissipated by means of certain distinctions. He says, " If we avoid the term sensation altogether, and arbitrarily use the term *æsthesis* to signify a mere physical impression on the centers of special sense, and the term *noesis* to signify a conscious impression, we may avoid some of the difficulties caused by the ambiguities involved in the common terms. The reaction of the mesencephalic and cerebellar centers might be termed *æsthetiko-kinetic*, and be thus distinguished from the *kentro-kinetic*, or excito-motor actions of the spinal cord, on the one hand, and the *noetiko-kinetic* action of the cerebral hemispheres on the other." (P. 46.)

To these terms, or the ideas they express, we can see no valid objection; but as regards the first term, we would not limit it to mere " physical impressions on the centers of special sense." We would make it include all sensory or excitor nervous impressions whatever.

The chapter on the "maintenance of equilibrium" is a valuable and suggestive one. Says Dr. Ferrier: " The maintenance of the equilibrium is an example of æsthetiko-kinesis, and involves the conjoint operation of three separate factors: 1. A system of afferent nerves; 2. A co-ordinating center; 3. Efferent tracts in connection with the muscular apparatus concerned in the action."

As regards the "afferent apparatus," or system of afferent nerves, it is said to be "of a compound nature, but mainly consists of three great systems, which, in conjunction, form that *synæsthesis* on which the due maintenance of equilibrium and co-ordination depend.   The equilibrium is disturbed by lesions of one, or more, or all of these.   These three systems are: 1. Organs of reception and transmission of tactile impressions; 2. Organs for the reception and transmission of visual impressions; 3. The semicircular canals of the internal ear, and their afferent nerves." (P. 47.)

By these three routes, or sources, are the peripheral impressions received, which excite the central co-ordinating nervous apparatus.   As regards the first route, or that of the tactile impressions, it is seen to be disordered in spinal locomotor ataxia, the affection being largely confined to a particular portion of the posterior columns of the spinal cord.   In this disease, the power of co-ordination is in a measure lost, and is especially manifest if the individual closes the eyes, so as to become deprived of visual impressions, as a means for co-ordination. Tactile sensibility is lost at the same time as a means of co-ordination.   As Dr. Ferrier says: " The fact of chief importance with which we have to deal, is that the loss of the faculty of equilibration and co-ordinated progression proceeds *pari passu*, with the diminution of sensibility to certain forms of cutaneous impressions.   These are especially impressions of contact, and it would seem that the contact of the soles of the feet with the ground is, in great measure, the exciting cause of the co-ordinated combination of muscular actions concerned in the maintenance of the upright posture and steady progression."

Dr. Ferrier seems inclined to reject the view that the disorder of the "muscular sense," so-called, has a share in spinal ataxies; for, after a moderately full discussion of this subject, he says, that "it would appear that it is not any and every form of tactile impression that is the efficient excitant of the co-ordinating centers of equilibrium and locomotion, but a special form of *cutaneous* impression generated by contact." (P. 53.)   If it is really the view of our author, that the muscular sense must be excluded, as a factor, from the spinal ataxies, we could hardly agree with him; but we cannot, in this place, enter on the discussion of so large a question.

Dr. Ferrier refers to, but does not discuss the question, as to whether "the different forms of tactile impressions are conveyed by different nerves," as Dr. Brown-Sequard has supposed, or

whether the same nerve fibres convey all kinds of sensory impressions, the different forms of the same being "conditioned by the nature of the stimulus applied to the peripherical terminations" of one and the same nerve fibres, as has been held by many, as well as by M. Vulpian, who is cited by our author. As to the opinion of Brown-Sequard, we have no hesitation in rejecting it; and the latter view, only represents the truth in part. It is true that the same nerve fibre may convey different kinds of impressions, but one of the most important features in the whole case is not mentioned. It is that when the impressions are conveyed to the center, that they are diffused through it, by means of the fine net work of fibres, with which nerve cells and the axis cylinders of nerve fibres are connected, and in this way the impression reaches those groups of cells in the cord fitted to re-act to them. Other groups of neighboring cells are not affected by a certain impression, because they are fitted to re-act to other kinds of impressions. The real point for the analysis of sense impressions is at the nervous centers, as in the receiving apparatus of the Gray telephone, for the sake of example. We do not offer these views as if they were novel, but as having greater importance than any one seems to have admitted, so far as we are aware.

Though visual impressions are believed by Dr. Ferrier to perform ordinarily a subordinate part in the co-ordination of muscular movements, yet, in locomotor ataxia, when the capacity for the maintenance of equilibrium by means of tactile impressions is impaired or lost, visual impressions become absolutely necessary, as may be easily proved. In locomotor ataxia, the patient is obliged to make conscious efforts to maintain an equilibrium. "But conscious efforts," as Dr. Ferrier says, "except under the guiding influence of present visual impressions, are utterly unable, of themselves, to compensate for the loss of tactile impressions. In this we see the necessity of immediate in contradistinction to mediate or cerebral registrations of visual impressions, for the due excitation of the co-ordinating centers, a fact which shows the mainly esthetiko-kinetic nature of the phenomena." (P. 55.) We would be glad to remark further on this subject, especially in view of its relations to sea-sickness, but our space will not permit. We shall return to it at another time in the future.

Next in order comes a consideration of labyrinthine impressions, in their relations to the maintenance of equilibrium. But this subject has been rather fully discussed, in former numbers of the JOURNAL, in referring to the researches of Mach, Breuer, Crum Brown, and others. Practically, this subject has much importance, on account of its relation to Menière's disease. After describing the results of experiment on the semicircular canals, in producing vertiginous movements, Dr. Ferrier says in regard to the origin of the impressions producing them, that they "are consequent on interference with, or perversion of cer-

tain impressions which act on some central organ of co-ordina-
tion, is shown by the fact that section of the auditory nerve
within the skull, causes marked disturbances of equilibrium," as
shown by Brown-Sequard. The author adopts Dr. Crum
Brown's description of the labyrinthine mechanism of co-ordina-
tion, consisting chiefly of the semicircular canals, the nerves
distributed to the membranous ampullæ of which, take cogniz-
ance of changes in pressure, etc., occurring within them, and
transmitting the impressions to the cerebellum, or at any rate,
to the co-ordinating centers. But we must postpone further re-
marks until the next number of the JOURNAL, in which we will
endeavor, not only to give a full summary of the experiments
and views of Dr. Ferrier, but also a critical judgment as to their
value.

III.—DISEASES OF THE NERVOUS SYSTEM.

HANDBUCH DES KRANKHEITEN DES NERVENSYSTEMS, XI. ERSTE
    HAELFTE: von Prof. H. Nothnagel, in Jena; Prof. F. Ober-
    neier, in Bonn; Prof. O. Heubner, in Leipzig; Prof. G. Hu-
    guenin, in Zurich, and Prof. E. Hitzig, in Zurich. Mit 5
    Holzschnitten. Leipzig, 1876. 819 pages. (*Hand-book of
    Diseases of the Nervous System.*)

In former numbers of the JOURNAL, we have taken occasion to
notice the two parts composing the twelfth volume of Ziems-
sens' Hand-book, and which treat of the more general and peri-
pheral disorders of the nervous system. The present volume
which precedes them in the series, forming as it does the first
portion of the eleventh volume, but which appears later in order
of time, is devoted especially to affections that directly involve
the brain and its membranes, the disorders of its circulation and
nutrition, its inflammation and those of its membranes, with
also the results of cerebral syphilis and tumors. The different
articles or memoirs are by some of the most distinguished au-
thorities on their respective subjects, as will be readily seen by
reference to their names at the head of this article. Like those
in the volumes previously noticed, they are, in the present state
of our knowledge, almost exhaustive monographs of the disor-
ders of which they treat, and by no means mere text-book hand-
lings of their subjects. There are, it is true, many points that,
in our opinion, are insufficiently discussed, and some that are,
perhaps, omitted almost altogether; but these omissions or de

fects do not materially depreciate the value of the work. We shall endeavor to point out these deficiencies as they appear to us. The excellencies of the several memoirs are for the most part sufficiently obvious, and an honest statement of their merits necessitates an exposal of both.

The first one hundred and ninety-two pages of the book are occupied with an account of the various circulatory disorders of the brain, by Prof. Nothnagel, of Jena, formerly of Breslau, whose well known investigations as to the cerebral functions have been frequently noticed in the pages of the JOURNAL. After a short preliminary chapter, in which he discusses the general conditions of the circulation within the cranial cavity, the influences which affect it, and its regulation, the author passes to the subjects of cerebral anæmia and hyperæmia which are treated in two successive chapters. A few points in these chapters are worthy of special notice as relating to questions still somewhat in dispute and giving the author's views in regard to them, or as giving new views, or such as are in other respects notable, considering the source whence they come.

Among the causes of cerebral anæmia. Dr. Nothnagel recognizes local vasal spasm causing a partial circumscribed deprivation of blood, but he does not go at any length into the subject, and merely alludes to it as being the pathological basis of certain forms of hemicrania and of *petit mal*. Vaso-motor paralysis, producing local congestion, is more fully alluded to in the remarks on cerebral hyperæmia, and the author favors the view that it may occur, though he holds that the absolute proof is still lacking to confirm it. Still the question as to the possibility of disorders of the circulation occurring in this or that limited portion of the brain receives comparatively slight attention at his hands, altogether less, it seems to us, than the subject deserves. A large number of clinical facts can hardly be well accounted for on any other supposition; though positive proof is lacking, and may perhaps be impossible to obtain, the inferential evidence is certainly very strong. We have in former numbers of the JOURNAL given our own views at considerable length, and it is not necessary to reiterate them. We notice the point here because there appears to us to be a lack of completeness in the author's account of the conditions of cerebral anæmia and hyperæmia, with so little mention of what must be comparatively frequent pathological conditions. Allowing them to be such—and it is hard to account for very many clinically observed facts without doing so—the practical importance of their recognition is evident, especially as regards diagnosis and prognosis. What might be, and indeed often is, taken for a symptom of serious and perhaps mortal injury of the brain, by its transitory nature and complete disappearance, not unfrequently gives us scarcely any choice but to consider it as due to a vaso-motor disorder of a very limited portion of its substance.

In his analysis of the sensory symptoms observed in cerebral

anæmia, Dr. Nothnagel alludes to the question as to whether the impairment of the senses of hearing and sight, is due to involvement of their special apparatus, or to imperfection of the psychic perceptive ability. He mentions a well known case of Abercrombie, that of the man whose hearing was impaired while in the upright position only, and was recovered on his assuming the recumbent or even stooping posture, as affording some support to the view that the acoustic was involved. It hardly appears to us to have any very decided bearing that way, since the general functions of the brain, psychic as well as sensory, are liable to be deranged more or less in its anæmic condition. In fact we see no reason for supposing the sensory symptoms to have any necessary connection whatever with the peripheral apparatus. The presumption is fully as much the other way. This view is supported, moreover, by the evidence offered by the author in regard to the ocular symptoms observed. He says that while he does not venture to say whether the optic nerve or the central perception is alone implicated, he has had oral evidence from a former colleague, Prof. Manz, that repeated ophthalmoscopic examinations in chlorotic and anæmic cases have revealed only slight pallor of the background of the eye, and no decided alterations of the caliber of the vessels, such as might be expected were the derangement of function dependent upon the implication of the ocular apparatus.

As regards the pathological signification of the symptoms observed, Dr. Nothnagel rather favors to some extent, the theory that they are due in part at least to variations of intracranial pressure. He says, speaking of this view, " If this opinion is perhaps too one sided, still the almost complete neglect of this cause at the present time is also plainly incorrect. That the intracranial pressure may diminish is an established fact, and if this takes place suddenly, so that interference with the functions of the brain may occur, this hypothesis is throughout supported. Unfortunately we have not as yet been enabled to test this question experimentally."

There are many clinical facts that seem to us hard to be accounted for by any of the other theories that have been proposed, such as a lack of a sufficient quantity of nutritive blood, or a change in its quality. Such, for example, are the suddenness with symptoms presumably due to cerebral anæmia, and directly following its rational causes. The peculiarities of the cerebral circulation are such as to readily justify the supposition that a much slighter variation of pressure of the brain substance than is perhaps supposed, may be followed by very marked phenomena, and when this is general, as there is a possibility it is in some forms of syncope, or complete though partial as in embolism, the symptoms are at once striking and sudden.

The researches that have been made in regard to the various centres in the medulla and pons, those for respiration and convulsion, are mentioned as affording us a better explanation of

the action of the other causes of the phenomena of anæmia.
We know to some extent the effects of deficient or depraved
blood supply on the respiratory centres. It is not difficult or
irrational to suppose, as the author says, that, although the re-
action of other parts of the brain may be somewhat different
from that of this centre, yet there can be no doubt that the ac-
tual physical and chemical processes that occur within the
ganglion cells must be the same in all.

Variations of intra-cerebral pressure are also the explanation
for the depression phenomena observed in cerebral hyperæmia,
in the opinion of our author, while the psychic exaltation is to
be attributed to the superior amount of nutritive material fur-
nished the cerebral cortex. The convulsive symptoms that are
observed in this disease, he thinks, are partly properly to be con-
sidered as really epileptic, but he refers the reader for the
discussion of the differential diagnosis of epileptic convulsions,
and those due to congestion of the brain, to the article on
epilepsy, already mentioned in this JOURNAL (Oct. 1876).

We have noticed only a comparatively few points in the two
sections on cerebral anæmia and hyperæmia. There are many
others equally worthy of mention, but they do not, perhaps, in-
dicate so much the author's peculiar views or call for adverse or
favorable comment. The subjects are throughout very judiciously
and ably treated. The remarks on the therapeutic management
of these conditions are brief, perhaps too much so, and are for
the most part confined to general measures, special remedies being
very little alluded to. Thus in the remarks on treatment of
cerebral hyperæmia, no mention whatever is made of the drugs
supposed to regulate the circulation, such as the bromides, nor
in fact of any drugs whatever. The only medicinal substances
specially mentioned for internal administration, are certain
revulsive cathartics. It is probably, however, just as well to
leave the details of treatment to the good judgment of the com-
petent practitioner, after laying down the general priciples that
should guide his action, but we venture to predict that in this
respect the book will somewhat disappoint the average American
reader.

The important subject of cerebral hemorrhage is given a
lengthy chapter of some ninety pages. After a short historical
sketch of the opinions that have prevailed and the literature of
the disorder, the author enters at once upon its ætiology. He
first notices briefly and dismisses one or two erroneous ideas
that have been advanced, that the hemorrhage is caused by a
primary softening or a primary atrophy, and resolves the prin-
cipal causal conditions into two, (1) increased vascular and
particularly arterial pressure; (2) disease and conseqent weaken-
ing of the coats of the vessels themselves. As to the relative
value of the two causes, the latter is, with our present knowledge,
declared to be altogether the most important factor in the pro-
duction of the accident of cerebral hemorrhage. Among the

facts that lead to this conclusion, which the author here discusses in detail, are mentioned the miliary aneurysms of Charcot and Bouchard, that had previously been observed by Cruveilhier and Virchow, and which, according to the first-named authors, depend upon a chronic inflammatory process of the envelopes of the vessel. The atheromatous degenerations of the arteries are a less common cause of aneurysm leading to cerebral hemorrhage, and when they have this effect, it is generally in the larger arteries of the brain, and usually in the form of a meningeal rather than an intra-cerebral rupture. As regards the question whether simple atheromatous processes with their results of destruction of the elasticity of the arterial walls has any direct connection with cerebral hemorrhage, Dr. Nothnagel expresses some doubt. In an indirect manner and in connection with other causes producing weakness of the smaller vessels, like for example the above mentioned miliary aneurysms, the effect of this condition of rigidity of the coats of the larger vessels increasing the pressure in the already impaired terminal branches, its influence is still worthy of consideration.

Simple arterial blood pressure without previous alterations in the vessels, the author holds, is not to be included among the causes of cerebral hemorrhage, or if included, it must be as a very exceptional one. But with these previous alterations it becomes a very important factor in bringing about the rupture of the vessels. Venous congestion, under similar conditions may have a like result, but simple increase of tension in the veins, Dr. Nothnagel, thinks can no more lead to rupture than can that in the arteries. In support of this view, he mentions the cyanosis in the paroxysms of whooping cough and sometimes observed in child-birth, during which conditions apoplectic attacks are among the very rarest of accidents.

The above are the two principal ætiological conditions of cerebral hemorhage; there are others, perhaps, such as altered qualities of the blood, that have an influence in certain rare and exceptional cases, but we need not dwell on them here. The author passes next to the predisposing causes, among which he considers advanced age to be the principal one. The so-called apoplectic habit of body which favors congestion, he thinks, has but little influence predisposing to cerebral hemorrhage. In his remarks on the anatomical relations of cerebral hemorrhage, he accounts for the well-known greater frequency of the lesion in the corpora striata than elsewhere, by the more direct blood pressure exercised on these parts than on others, they being irrigated, according to the statements of Heubner and Duret by the first branches of the anterior and middle cerebral arteries, in which vessels the impulse of the current from the carotid is most direct. Allowing then that the diseased conditions favoring hemorrhage are equally liable to occur in these vessels, it is not difficult to account for the greater frequency of hemorrhages in these parts.

The distinction that has been made of the so-called capillary apoplexy, from ordinary cerebral hemorrhage, the author holds to be unnecessary, inasmuch as it is in his opinion, either a secondary or subordinate phenomenon, or, if independently occurring, of very slight and transient importance. Minute extravasations into the brain substance are probably very quickly absorbed; he has never been able to find the slight hemorrhagic traces caused by a small needle inserted into the brain, two weeks after the operation.

The symptomatology is given with very much fullness, but it is impossible for us to follow the author in all his detail. A few points will, however, command our attention. Dr. Nothnagel in discussing the cause of the loss of consciousness in cerebral apoplexy and reviewing the theories that have been proposed, comes to the conclusion that all the explanations are as yet less satisfactory than perhaps is generally supposed. His disposal of the question is altogether non-committal. It appears to us that an expression of a provisional opinion as to the cause of the coma might, at least, have been justified by the facts and probabilities of the case. Too little account is made of the effect of the suddenness of lesions, which has been pointed out by Hughlings Jackson and Jaccoud and the effects of shock, which it seems to us may be as great from a small lesion in an important part of the brain, like the centres in the basal ganglia, as from a larger one in other portions less directly in relation with the principal bodily functions and routes of conduction, and less directly connected with all other portions. While it is eminently safe therefore to leave the subject as our author leaves it, this caution yet appears almost uncalled for. The fact that sudden hemorrhages may occur without producing loss of consciousness, is not by any means a proof that it is not generally so brought about, nor is the failure of artificial lesion in the lower animals to produce this, at all fatal to the supposition that it thus occurs from injuries of the much more responsive and sensitive human brain.

In regard to the question of direct hemiplegia, or that in which the intra-cerebral lesion and the paralysis are both on the same side, Dr. Nothnagel is equally non-committal. He admits the existence of such cases, and that they are very difficult to account for. He rejects also the hypothesis of a non-decussation of the fibres in the pyramids and that of their re-crossing proposed by Schiff, as untenable, being thus far unsupported by any anatomical evidence. The opinion of Ambrosi, adopted also, we believe, by Rosenthal, that hemiplegia is due to a secondary œdema affecting the opposite side of the brain, is objected to, on the ground that the direct effects of the obvious lesion have to be considered as non-existent, a point to which there is really considerable force. Dr. Nothnagel seems inclined to admit that the cases published of direct hemiplegia were correctly observed and reported, and allowing this much, his course is the safe one.

But the real objection to these cases and the one to which he
does not at all allude, is the doubt as to the completeness and
correctness of the observation.   Hardly a case of brain lesion or
disease that has ever been reported gives evidence of absolutely
exhaustive examination, and is not liable to criticism on this
ground, that would be valid against its acceptance as overthrow-
ing any established views supported by the great mass of
evidence.   Cases of seemingly direct hemiplegia are too in-
frequent to be anything but rare exceptions to the general rule,
and the probabilities of oversights in the observation of such
reported cases are so great as to make it almost needless, it
seems to us, to call in any other hypothesis to account for them.
    Dr. Nothnagel himself alludes to the usual imperfection of
the recorded observations, when discussing a little farther on,
the question of the localization from the clinical phenomena of
the lesion.   He says, " If we examine the literature, it unfortu-
nately reveals the fact that only a relatively few of the recorded
observations meet the requirements, and that in many of these
few, the clinical phenomena are very imperfectly reported, often
only because the observations were made for special purposes."
While this is true as regards the question of the more minute
and special localization, it is none the less so for the more gen-
eral localization of the trouble in one or the other cerebral
hemisphere, and a consideration of this fact ought to have pre-
vented the author from going so far as to favor, even vaguely
and generally, the possibility of a direct hemiplegia, without in-
volvement of the opposite hemisphere or some special arrange-
ment in the decussation of the fibres, which, as stated above he
rejects as improbable and unsupported.   We know from both
clinical and experimental evidence that the sensory fibres decus-
sate in the cord, and that reflex movements occur on the same
side of the body on which the impulse was given, even when
they involve centres, probably above the sensory decussation.
Moreover, the disorders of sensibility in hemiplegia are commonly
co-extensive with the paralysis Hence the necessity of assuming
that there is also a crossing of the motor fibres.   In a case of so-
called direct hemiplegia, therefore, it is necessary for us to sup-
pose the lesion to be indirect for the sensory symptoms and
direct for the paralysis, or else to assume the existence of soem
very complex, and, anatomically and physiologically undemon-
strable system of special commissural connections, which is ab-
surd.   Every physiological oirap bly as well as the great mass
of clinical evidence is against it.
    The symptomatology of the specially localized lesions in this
or that portion of the brain, is pretty fully discussed, but with
the same cautious and conservative spirit that is evidenced
elsewhere in the author's work.   His general conclusions are
summed up in the following words :   " Summing together what
we have in only a briefly sketched manner stated, in regard to
the possibility of a localization of the hemorrhagic infarctions, it

follows that we can diagnosticate with approximate certainty, the coincidence of certain determinate conditions being presupposed, hemorrhages only in the following localities : (1) in the pons, (2) in the cerebral peduncle, (3) in the nucleus lenticularis, (4) in the crura cerebelli, (5) the disease of a certain definite region, more fully described elsewhere, by the presence or predominence of a peculiar anæsthesia. And if the presence of a lesion in these localities even, can only be diagnosed with approximate certainty, in all other parts, after careful weighing of all possibilities, only more or less well-grounded probability can be stated, and even this often is lacking, especially when the lesion is extensive."

The special locality alluded to in the above quotation includes the posterior portion of the inner capsule, and possibly some of the adjoining parts. M. Charcot refers the anæsthesia to lesions of the posterior one-third of the inner capsule, the anterior two-thirds producing motor paralysis.

Dr. Nothnagel recognizes no established difference between the two sides of the brain as regards their functions, except in respect to the function of speech which is referred to elsewhere, nor does he consider the opinion held by some that lesions of the occipital lobes are especially connected with intellectual disturbances as at all supported.

It is possible, perhaps, that some modification of this statement might be made at the present time, considering the advances that have been made within the past two or three years in the department of cerebral physiology. The author himself says in a note at the close of his chapter on embolism and thrombosis following the one under consideration, that he has not made use of these later researches, since he finished his manuscript in November, 1874. We are very far from certain that he is not too conservative in the views he here expresses, and that the approximate diagnosis of lesions in several other locations may not be made during life.

The remarks on treatment are as usual, general in their character, and with the exception of the iodide of potash and a general condemnation of the use of strychnia, drugs are scarcely mentioned. The author's reliance is on other measures, venesection, counter-irritation, electricity and warm baths. As in the case of the remarks on the treatment of cerebral hyperæmia, we venture to predict that his treatment in hemorrhage will not altogether coincide with the ideas of the American practitioner.

The remarks on meningeal hemorrhage present no particular features to call for attention here. The same is the case to a considerable extent with the following chapter on embolism and thrombosis, which, though of considerable length, may be passed by here, one or two points only being noted. The question as to the cause of the coma in cases of embolism is taken up and disposed of in much the same way as in the case of the coma following cerebral hemorrhage. Our remarks when dis-

cussing that point will also apply here. As to the convulsions that sometimes occur, their explanation is possible, when large tracts are deprived of their blood-supply as in the case of the occlusion of the basilar, by the diffuse anæmia of the pons and medulla. In case of embolisms of smaller arteries, he can only account for them by calling into consideration the results of the well-known experimental researches on the cortical substance in which convulsions were excited by electrical or mechanical irritation, the sudden anæmia caused by the embolism, in this case acting as the excitant.

The section on tumors of the brain by Dr. Oberneier appears to be a useful and quite complete and well-arranged memoir upon its special subject, but for various reasons chiefly on account of the relatively slight frequency of these affections and the necessary limits of this notice, we cannot here give it detailed attention. The section next following that of Dr. Heubner on syphilitic disease of the brain, cord and nerves we shall pass, as we have already noticed in the pages of the Journal (April, 1875,) a more extensive work on part of the same subjects, and the article though important is very concise and presents no important points for criticism. We can cordially recommend it to our readers as a brief but thorough handling of this very important subject by a thoroughly competent authority.

[TO BE CONTINUED.]

IV.—BARTHOLOW : THERAPEUTICS.

A Practical Treatise on Materia Medica and Therapeutics. By Roberts Bartholow. M. A., M. D. New York : D. Appleton & Co. 1876; 537 pages. Chicago: Jansen, McClurg & Co.

In the multiplication of works on all the practical and general departments of medicine at the present day, each new-comer is liable to be closely questioned as to its *raison d'etre* and must furnish valid reasons for its appearance. And though it is far from being true in all cases that success depends upon merit, yet any work that is more than a mere epitome or labor-saving condensation, must to meet the favor of the book-buying and reading public present some well-grounded claims for consideration. This is especially the case with a work like the one whose title heads this notice, and which appears so soon after another work of unusual merit, that of Dr. H. C. Wood, occupying the same department of medicine.

Dr. Bartholow recognizes this fact in his preface, in which he states that this book embodies in effect the ideas as to what such a work should be, derived from many years experience as a teacher of materia medica and therapeutics, and alludes to certain features in which it differs from its predecessors and which therefore entitle it in some measure at least, to the consideration of the medical profession. We can only give attention to these general features of the work, its details are too numerous to be even approximately, much less adequately reviewed, in a notice like the present one.

In the first place this work differs from nearly all others in its method of treatment of its subject. Like the very valuable and scientific treatise of H. C. Wood, it gives scarcely any attention to the pharmaceutical details that formed no small part of the text in most of the former works, on therapeutics, and which have been the bug-bear of many a medical student. In a work on therapeutics of the compass of an ordinary text book such details really have no proper place, there is enough valuable matter that can better fill the space they occupy, and when it is needed to refer to them there are special works that can amply meet all requirements. The omission of these needless details, and their substitution by fuller and more elaborate accounts of the physiological action of the various medicinal agents, places this work and that of Dr. Wood on a much higher plane than their predecessors, relieving them very decidedly from the old stigma of "cook-book pharmacy," and we have nothing but approval for the change.

A feature in which this work differs from most of its predecessors is in the classification adopted. As Dr. Bartholow says, in the present state of our knowledge a perfect classification is impossible. The only question, therefore, is, not whether the one here adopted is free from faults or inconsistencies, but whether these are sufficiently prominent and glaring to vitiate the system A plan of classification may be so simple and general as in a measure, to place it apparently without the pale of criticism, the difficulties appear in the elaboration and detailed application of such a plan, and the more detailed a system is, the more obvious are its defects at first sight. Dr. Bartholow's method has certainly the merit of simplicity, he divides remedies into five classes: viz., those used to promote constructive metamorphosis; those used to promote destructive metamorphosis; those used to modify the functions of the nervous system; those used to cause some evacuation from the body; and lastly, topical remedies. The only general criticism that this arrangement suggests, is partly anticipated by the author,—it is that, inasmuch as the nutrition and all the processes of the body are under the control of the nervous system, at least in a certain sense, the third class here given would be, by a closer analysis, largely resolved into the others already named, or that as it stands at present it comprehends to a greater or less degree all

the others. Therefore, to say as does our author, that a large class of agents is used not to affect tissue change but to modify nervous action, is hardly an exact statement of the case, though the qualificatory remark that they probably do influence structure in some unknown way, relieves it of its baldness to a considerable extent. When we come to see the application of this plan, its value or want of value becomes of course more apparent. We notice here some matters of detail that are perhaps open to animadversion under this arrangement, such for example as placing quinine under the agents favoring constructive metamorphosis and salicine and salicylic acid under topical remedies, but our space will not permit us to more than merely mention them. Taken altogether, however, the classification adopted is about as good as any that could be looked for in a work of this class, and better, as not attempting so much, than the one adopted in the work of Dr. H. C. Wood. It is very evident, however, that this classification, which, by the way, is not so very different from that adopted in a recent German work, is far from being altogether satisfactory, even in its own very general and unelaborate way. Its author's claims for it are modest enough, he says that he does not hold that it is superior to others — only that its simplicity is in its favor.

A noteworthy feature of the book is the attention given to some subjects that do not usually receive any very extended notice in works on therapeutics, for example, the section on aliments is the longest in the volume, and we find mention of several remedies that are not officinal or often included in works of this class. Then we have here also classified notices of most of the more important mineral springs in this country, a good practical section on hydrotherapy, and another on electricity. In fact, the practical character of the book is prominent throughout, while at the same time it embodies all or most of the recent scientific advances in this department. While it is less prominently a critical treatise on scientific therapeutics than is the work of Dr. H. C. Wood, it is if anything more practical, and perhaps better fitted for the use of the student commencing this study. A bibliographical list appended to the remarks on each drug or remedial agent, is a useful feature, though with references in the text to the same authorities, it would, we think, be still more valuable.

We can cordially recommend this volume as a text-book, not as supplanting, but as introductory to others more critical and elaborate.

V.—SHORTER NOTICES.

I. A Treatise on the Theory and Practice of Medicine.
By John Syer Bristowe, M. D., F. R. C. P.   Edited with
notes by James H. Hutchinson, M. D.   Philadelphia:
Henry C. Lea, 1876.   1089 pages.   Chicago: Jansen,
McClurg & Co.

II. Chemistry: General, Medical, and Pharmaceutical,
Including the Chemistry of the U. S. Pharmacopœia.
A Manual on the General Principles of the Science, and
their Applications in Medicine and Pharmacy.   By John
Attfield, Ph. D., F. C. S.   Seventh Edition.   Revised
from the Sixth (English) Edition by the author.   Philadel-
phia: Henry C. Lea, 1876.   668 pages.   Chicago: Jansen,
McClurg & Co.

III. The Electric Bath, Its Medical Uses, Effects and
Appliances.   By Geo. M. Schweig, M. D.   New York: G.
P. Putnam's Sons, 1877.   134 pages.   Chicago: Jansen,
McClurg & Co.

IV. Epitome of Skin Diseases, with Formulæ for Students
and Practitioners.   By Tilbury Fox, M. D., F. R. C. P.,
and T. C. Fox, B. A. (Cantab), M. R. C. S.   Philadelphia:
Henry C. Lea, 1876.   Chicago: Jansen, McClurg & Co.

V. Hay Fever, or Summer Catarrh: Its Nature and
Treatment.   Including the early Form or " Rose Cold:"
the later Form or " Autumnal Catarrh:" and a middle Form
or July Cold, hitherto undescribed.   Based on Original Re-
searches and Observations, and containing Statistics and
Details of several hundred cases.   By George M. Beard,
A. M., M. D.   New York: Harper & Bros., 1876.   266
pages.   Chicago: Jansen, McClurg & Co.

I.   Dr. Bristowe's work is one of the more comprehensive but
less exhaustive class of text-books, which, taking up a depart-
ment of medicine, cover the whole field in a fashion, giving ab-
breviated but still pretty fair accounts of all the principal
general and special diseases to which man is liable.   There are
objections to this plan in text-books, and they have some force,
especially when we consider that many graduates of our medical
schools seem to consider that the works of this kind used by
them in their first studies, embody the sum of human knowledge

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of their subjects for all time. For a student's manual simply such books are less objectionable, and there are even advantages in their comprehensiveness.

The only valid objections that can be brought against the present work, are that it is unnecessary, as we have already a sufficient number of works on the practice of medicine, and second, that from the brevity with which it treats important subjects it is of a class to lead to superficial study and imperfect knowledge. To the first of these objections it may be replied that we have no general works on the practice of medicine that in many respects are so fully up to the results of modern researches, and in so far as this is true, it is a satisfactory answer. In reply to the second objection, we may say that it contains far more than the average medical student can be supposed to know at his graduation, and that it is designed, as the author himself states in his preface, for students, and not for old practitioners as an exhaustive work of reference. On the whole, it seems to us that we can say as much in its favor as we can of many more ambitious works, and can in the main, agree with the American editor in his statement, that it fully meets the wants of the medical student.

The section on nervous diseases which occupies a little over two hundred pages at the close of the book, is commenced with general remarks on the anatomy, physiology and general pathology of the nervous system, an excellent feature as it seems to us. Afterwards follow the special pathology and therapeutics, which alone appears in most works of this kind. The same method is adopted in treating of diseases of other organs, such as those of the respiratory, vascular and genito-urinary systems, but not in all, nor generally to the same extent. This plan is not particularly novel, but we like the way in which it is carried out, especially in regard to the nervous system.

There are many points of detail in which we disagree with the author, but the limits of the present notice forbid us discussing them here to any extent. Thus he sometimes misplaces affections in his classification; such examples as cerebro-spinal meningitis among the specific fevers, and Basedow's disease and angina pectoris among the vascular disorders are sufficient to illustrate this fact. Others might also be cited.

In conclusion, we can recommend this work to students as a text-book up to the times, but, of course, not as containing all that a practitioner will require.

II. The various successive editions of Attfield's chemistry that have appeared since its first publication nine years ago, sufficiently attest the favor in which it is held by the medical profession. It is the medical student's chemistry *par excellence* and will probably long retain its position as such. The present edition includes a notice of all the substances of the British,

the United States and the Indian pharmacopœias and will be found to contain some mention of nearly every product used in medicine. We know of no work of the same size and scope that equals it for the medical man, certainly none that excels it.

III. Dr. Schweig's little book treats of a subject that has largely been neglected by the regular profession. There are reasons enough for this; the method of electrical treatment here described has been largely in the hands of irregular specialists, it requires especial and expensive appliances, and its apparent results have been, with some show of reason in many cases, attributed to the moral effect produced by the surroundings rather than to a really therapeutic effect of the electricity. It is also, or at least appears to be, a very general shotgun kind of method, which does not *prima facie* recommend itself to the careful trained physician.

Still there is also good reason to think that this method has been undervalued by the profession, and this little work may serve to support this opinion. There is a class of disorders which do not admit of local applications with benefit, and to them this method seems on the whole to apply very well. Others again, more localized in their pathology, can also be benefited by applications of this kind. Dr. Schweig gives the result of his experience with the electric bath in the treatment of cases of various diseases, such as rheumatism, neurasthenia, neuralgias, hysterical affections, etc., and shows, we think satisfactorily, that it may be a valuable therapeutic aid in the management of many of these affections. Still we do not think it by any means, the only effective measure, and it is fortunate that it is not, since it is practically out of the reach of the great majority of patients at the present time. We can recommend Dr. Schweig's book as an exposition of the merits of this method of applying electricity for the cure of disease.

IV. This is the most abbreviated manual of any important class of disorders that we have recently met with. It is in fact too brief to receive our entire approval, whatever that may be worth, for a work on this class of diseases. It is, it appears to us hardly to be recommended to the student, and the practitioner should depend on larger and fuller works than this. Its principal value would be as a reference book for the pocket, and it is not an evidence of the highest qualifications to be obliged to utilize such an article. Still, we have no doubt that the book will be a profitable one to the author and publisher.

V. The lengthy title of this little work is almost a description of its contents. Dr. Beard has given a very complete monograph of the increasingly prevalent disorder known as Hay Fever, describing its history, symptoms and its geographical distribution, and also giving accounts of a number of illus-

trative cases. a good general statement of its probable pathology, and a critical review of the various methods of treatment. His views as to the nature of the affection as well as of the essential identity of the three forms described. seem pretty well supported by the facts and the arguments set forth. There seems to us to be no reasonable doubt of the neurotic basis of this affection. We think that the researches here detailed will be of real value to the medical profession. and also to the highly respectable class of the laity that are sufferers from this disease.

Editorial Department.

IN an interesting article — in the November number for 1876, of the London *Lancet*,—on "the duality of the vaso-motor system" of nerves, certain views are set forth, which we believe to be erroneous, and since they relate to important questions in the physiology of the nervous system, we offer a few remarks on them. The particular point made by the writer of the article in question, is that " the doctrine of inhibition and inhibitory nerves is dying." The writer says that the "most cursory glance over the modern tendencies of physiological research is sufficient to show this." But what doctrine are we to hold in regard to the facts, which the hypothesis of inhibitory nervous action was devised to explain?

The writer in the *Lancet* says, that the "substitute for the doctrine of inhibition may be summed up in the phrase, the duality of the vaso-motor system." " It considers that all the vascular nerves may be resolved into two mutually antagonistic sets,— the constrictor nerves and the dilator nerves,— the normal tonicity of an artery being due to the balance kept between these two vaso-motorial agents. If that balance be destroyed, the part supplied by the vessel becomes either hyperæmic or anæmic, according as the dilating or contracting power has the upper hand.

"Thus whereas on the theory of inhibition vascular dilatation was a passive process, a suspension of the usually operating vaso-motorial influence or state of contraction of the vessel, on the present view, there is held to be as much action about the dilatation of an artery, as there is about the narrowing of its caliber." Various reasons are given for the rejection of the theory of inhibition, whether in relation to the vaso-motor, or

other parts of the nervous system. But to us these reasons have never seemed sufficiently cogent to warrant the conclusion reached by the writer in the *Lancet.*

Though our readers have had this question often treated in the pages of the JOURNAL, yet we do not think a re-statement of what we believe to be the correct view as to vaso-motor nervous action, will be superfluous.

Then in the first place as regards inhibitory nervous action, our author says in general, that " it is dying," and that this fact is shown by even a cursory glance over the field of modern physiological research. But what is inhibitory nervous action? It is simply the restraining action which one nervous center exerts through the medium of connecting nerve-fibres over another center, usually the weaker and more peripheral of the two. Will it be seriously denied that the action of a subordinate center cannot be controlled by the action on it of a higher one? Is any fact in nervous physiology better established? Will it be denied that a higher center can excite a lower one to action? We do not see how either position can be denied as correct. The examples of the control of a lower by a higher nervous centre are so numerous, that we deem it unnecessary to cite even one of them. So much for the general question of inhibitory action in nervous physiology.

But next as regards its application to the vaso-motor nervous system. Must we deny this form of action to that part of the nervous system, as the writer in question supposes? Let us see. The view combatted in the article in the *Lancet,* supposes the muscular arteries to be furnished with sensory or excitor nerves, which proceed from their inner coats to certain small ganglia or ganglion cells, which lie in or near the muscular walls of the vessels in question. These nerve cells are stimulated to action by the excitor nerves which convey impressions to them from the vessels. The action of the ganglion cells causes a motor impulse to pass out from them along the motor nerves which terminate in the unstriped muscular fibre of the coats of the adjacent vessels, and in this way the coats of the vessels are nominally kept in a constant state of contraction which varies greatly according to circumstances. This is what is called vascular *tonus.* The vessels always em-

brace the column of moving blood with some firmness. This we say is the so-called *tonus.* The little nervous apparatuses which we have described, constitute the local peripheral vaso-motor apparatuses. But they are not isolated and devoid of means for co-ordinating the action of various vascular areas throughout the body.

The regulating apparatus lies more deeply within the recesses of the economy, near the other great central nervous mechanisms, and they are found partly within the cord and medulla. A certain set of fibres pass out, it is supposed, which serve to connect the peripheral with the central or spinal vaso-motor apparatuses. These connecting fibres are of one kind, and they terminate peripherally, not in the muscular coats of the vessels, but in the ganglion cells of the peripheral vaso-motor apparatus. And their action on it is to *arrest* the natural action of these peripheral tonic centers. Naturally they are always in gentle action during the continuance of health. If these nerves do not act, then the reflex tonic action of the peripheral apparatus is unrestrained, and the vessels contract to a small caliber. But if the action of the nerves which proceed from the cord is increased, they arrest in a certain degree, the tonic action of the peripheral apparatus in which they terminate, and the vessel relaxes and enlarges by reason of the expansive pressure of the contained blood. The vessel, or rather its nervous apparatus, is not paralyzed, its *tonus* is simply diminished. In this simple way do the vessels contract and expand, and hence regulate the supply of blood to a part. Is this view unreasonable or in opposition with any known facts in nervous physiology?

But we will admit the true condition of the case to be, that instead of one, there are two sets of fibres passing between the cord and the peripheral vaso-motor apparatuses, one of these exciting it to increased, and the other to diminished action. The former are called *vaso-constrictors* because their action leads to contraction of the vessels. The latter are called *vaso-dilators*, because by their action they lead to dilatation of the vessels. But what do these two sets of nerves act on peripherally? Do they act on the muscular tissue itself directly, or the peripheral vaso-motor apparatus as already

stated? All the probabilities are in favor of this latter view. But whether the two sets of nerves mentioned act directly or indirectly, on the muscular coats of the vessels, they are supposed to conflict in their action. But suppose for the time that they act on the peripheral vaso-motor apparatus in which they are supposed to terminate, how do they act?

Manifestly the vaso-constrictors act by exciting or strengthening the action of the peripheral apparatus, while the vaso-dilators must either diminish its action or arrest the action of the vaso-constrictors, but in either case the action of these last nerves is one of inhibition or arrest. But if we suppose, as hardly seems probable under the circumstances that both the vaso-constrictors and vaso-dilators terminate in the muscular coat directly, they must conflict even there in their action, by a sort of mutual inhibitory process, for they must be supposed to be in continual antagonistic action.

So in whatever way we regard the case, we can hardly escape the admission of inhibitory action, not even by the hypothesis of "duality of the vaso-motor system." As to the appearances of vermicular activity, as well as simple dilatation, which the vessels present at times, we have been led to explain them in a wholly different way to that of supposing the clumsy hypothesis of special vaso-dilator nerves; but we cannot now give the explanation, for want of space.

The following letter speaks for itself. We insert it to add as far as it is in our power to its circulation among thinking members of the medical profession:

November 1, 1876.

SIR:—The Superintendents of the American Institutions for the Improvement of Idiots and Feeble-minded Children having formed an Association for the more rapid advance and spread of their special part of medical science, resolved, not only to unite their efforts, but to seek the assistance of physicians in general practice who can help them to elucidate the causes of idiocy and kindred affections.

Previously, when searching individually for these causes, we met with three obstacles: one from the parents, whose ignorance or false delicacy could not, or would not, tell the truth; second, one from our mode of pro

cedure, which was to not communicate nor put in common the findings of our individual experience; and a third, worse yet, to send abroad printed inquiries whose specifications were so worded by conceited theories as to force the answers towards biased issues, thereby rendering these data untrustworthy, if not truthless.

Now, knowing better from experience, we send you no syllabus, but we rely upon your own intelligence to write a short communication of the causes of idiocy which have come to your knowledge from reliable witnesses or personal observation.

The names will be either omitted or made use of at your request.

Please address, as soon as convenient, the Secretary of the Association, I. N. KERLIN, M.D., Superintendent of the Pennsylvania Training School for Feeble-minded Children, Media, Pa.

Members of the Association:

DR. GEO. BROWN, Barre, Mass.
" G. A. DOREN, Columbus, O.
" I. N. KERLIN, Media, Pa.
" H. M. KNIGHT, Lakeville, Conn.
" E. SEGUIN, New York.
" E. C. SEGUIN, New York.
" HENRY TUCK, Boston, Mass.
" H. B. WILBUR, Syracuse, N. Y.
&c., &c., &c.

We have received the first number of the *Quarterly Journal of Inebriety,* a new journal published under the auspices of the American Association for the cure of inebriates. It contains besides the Proceedings of the Association and the President's Address, an article by Dr. Geo. M. Beard, on the Causes of the Increase of Inebriety in the United States. We wish this new journal, occupying as it does a very important special field, all possible success.

The American Medical Bi-Weekly, the successor of *The American Medical Weekly* of Louisville, has appeared and presents quite an attractive appearance.

ꬍeriscope.

a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

The Functions of the Cerebellum.—Nothnagel *Virchow's Archiv.* LXVIII., I. 59, sums up, as follows, the conclusions drawn from his investigations as to the functions of the cerebellum. He states, however, that as he did not aim to make an exhaustive work on this organ, these conclusions are only the results of his experiments, as far as they went.

(1.) The cerebellum acts in certain motor processes, its function is therefore, in a certain sense, a motor one. This connection with phenomena of motility is evidenced in excitation, as well as in destroying experiments.

(2.) In all probability there are functional (and therefore also anatomical) connections between the two halves of the cerebellum.

(3.) Complete destruction of the same portion of the cerebellum, irritation of which produced definite transient motor effects, had no visible permanent effects ("*Ausfallserscheinungen*" Goltz), as a result.

(4.) The destruction of one hemisphere, of both hemispheres alone, or of the anterior upper portion of the vermis alone, produces no disturbance of co-ordination. On the other hand, mechanical irritation of these parts is followed by motor irritative phenomena.

(5.) The recognized disturbances of co-ordination follow only such injuries in which the mass of the hemispheres and the vermis are simultaneously involved.

The Functions of the Corpora Quadrigemina.—Prof. Kohts, of Strasburg, publishes *Virchow's Archiv.*, LXVII., Hft. 4, a paper on the functions of the corpora quadrigemina, taking, as his text, a case of a man in whom the symptoms, during life, were vomiting, severe headache, optic neuritis, bilateral paralysis of the motor oculi nerves, etc. The symptoms in this case he divides according to the time of their occurrence into two series, the first, comprising those which were observed for a period of some ten months previous to a fall which developed the second series, consisted first in disordered locomotion, and after a few months alternate divergent strabismus, temporary trouble with the bladder, irritative and paralytic phenomena in the region of certain motor and crania

nerves besides those implicated in the production of the strabismus; there were no troubles of speech nor complaints of pain, as in ataxia, no involuntary movements or disturbance of sensibility.

The second period of the disease came on, as stated, very suddenly after a fall, and the symptoms were those indicating a cerebral tumor: intermittent paroxysms of pain in the occiput, vomiting, a noticeable tendency to fall forward to the right, and sometimes backward, and optic neuritis. The autopsy revealed a tumor of the size of a cherry, occupy ing and destroying the posterior corpora quadrigemina.

Dr. Kohts follows this with a detailed account of experiments on frogs, pigeons, and dogs, and, from the whole, deduces the following con clusions:

(1.) The centre for the maintenance of the equilibrium on which the finer adjustment of the movements depends, is situated in frogs (con firmation of Goltz's experiment), and in birds in the lobi optici, which correspond to the corpora quadrigemina of the higher animals.

(2.) The posterior corpora quadrigemina are to be classed among the co-ordination centres, since after their destruction the co-ordination of complicated combinations of movements is impossible.

(3.) It may be accepted that the disorder of the muscular sense, the ataxia, in injuries to the corpora quadrigemina, is dependent usually upon a lesion of the tegmentum (*Haubenbahn*), which, according to Meynert's researches, serves for a route for reflex impulses, and takes its origin in the thalamus and corpora quadrigemina.

THE SENSATION OF SOUND.—At a recent meeting of the Vienna Academy, a paper was communicated by Dr. Isidor Hein, "On the Re lations between Perceptions of Touch and of Hearing." His conclusions are these: 1. The sound produced by striking a solid body is always accompanied by a sensation of touch, which, like the sound, differs ac cording to the nature of the body. If the sound is different in different parts of a body, there goes along with the variation of the sound a vari ation in the touch sensation; and if the surface be divided into several sections, according to differences in sound, a convenient division may be made on the basis of touch. 2. On bringing a struck body towards a reflecting wall, the sound and touch-perceptions show similar variations. 3. To the touch-perception in question correspond vibratory motions of the exterior body, produced even with the weakest striking, whereas sound only begins to be perceived with impacts of a certain intensity. 4. The sense of touch is capable of perceiving vibrations and comparing the differences of these. It brings hereby to consciousness, a special quality of touch-sensation, which is to be distinguished from sensation of pres sure. 5. This distinguishing power of the organ of touch, not suffici ently appreciated hitherto, offers practical medicine a peculiar mode of investigation, which greatly enlarges the doctrine of the physical symp toms of the human organization, and for which the author suggests the (German) name of "Erschutterungs-palpation." *Nature*, November 30.

THE INFLUENCE OF FARADIZATION AND GALVANIZATION OF SPECIAL NERVES IN MAN ON THE LOCAL TEMPERATURE.—The following are the results of a series of experiments by Przewoski, undertaken at the investigation of Eulenberg, and reported at the meeting of the Med. Verein at Greifswald, August 5, 1876, (*Deutsche Med. Wochenschrift*) No. 43. The temperature variations were measured, partly by thermo-electric methods, and partly by thermometers.

(1.) Faradization of the cervical sympathetic of one side caused an immediate cooling of the cheek of that side, (according to the length of the application, from 2 to 12 minutes, ranging from 0.5 ° to 1.75 ° Cent.). This lowering of temperature continued for some time after the discontinuance of the irritation, finally giving way to an elevation of temperature of 0.5 ° cent. above the original figure.

(2.) Fradization of the ulnar nerve caused an immediate decrease of temperature in the region supplied by it, between the third and fourth fingers, of from 0.7 to 2.53 cent.

(3.) Irritation of the peroneal nerve causes a rapid fall in the thermometer attached to the outer margin of the foot, or between the fourth and fifth toes. The decrease of temperature is more considerable than with faradization of the sympathetic or the ulnar nerve, (amounting to 3.6 ° C.).

(4.) Application of the constant current, particularly in the way of cathode closure, to the right cervical sympathetic, causes a lowering of temperature in the right cheek and hand. That of the cheek is still more considerable than that of the hand. It occurs in both (thermo-electrically determined) directly after the closure of the circuit, and undergoes a gradual increase during the time of the application of the cathode.

(5.) Application of the anode to the cervical sympathetic causes, on the other hand, a noticeable, though slight increase of temperature in the corresponding hand and cheek, after the closing of the circuit.

(6) Closure of the cathode on the ulnar nerve causes a decrease of temperature between the fourth and fifth fingers of the hand (in one case 1 °. 0 C., lasting 40 seconds; in another 1.06 ° C., for 65 seconds). Application of the anode, on the contrary, was followed by a rise of temperature in the same region, (as much as 1.5 ° C., for 25 seconds.).

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THE INNERVATION OF THE SUB-MAXILLARY GLAND.—The following are the conclusions of an article by G. Giannuzzi, *Lo Sperimentale*, April, 1876, (abst. in *Revue des Sci. Médicales*), on the influence of the sympathetic in the secretory functions of the sub-maxillary gland:

1. The cervical sympathetic has an influence on the sub-maxillary secretion, varying in intensity in the different varieties of dogs operated upon.

2. This influence is always well marked, and manifests itself sometimes on the gland, sometimes on the neighboring parts.

3. The secretory nerves of the sympathetic are furnished by the third and fourth dorsal roots; they are altogether different from those that affect the pupil.

4. The vaso-motor come from the fifth or sixth dorsal roots.

5. Even after the section of the chorda tympani, we can provoke reflex action of the glands by exciting the buccal mucous membrane. This phenomenon is not constant in all dogs, and is not easily observed when the cord has been cut some days previous to the experiment.

6. The action of the sensory nerves on the salivary secretion is not due to an increase of the circulation, but to a reflex influence exercised on both the secretion and the circulation.

7. The excitation of the superior portion of the cervical cord, instead of diminishing the flow of blood in the glandular vein, increases it.

8. When we irritate the first dorsal roots, we have an argumentation of the cardiac pulse. The author promised to study this last influence in a future memoir, but unhappily, his very recent death prevents us from obtaining the results of the continuance of his investigations.

THE PHYSIOLOGY OF THE LACTEAL SECRETION.—Dr. A. Roehrig, *Virchow's Archiv.* LXVII., I. 119, publishes an interesting paper on the physiology of the secretion of milk, and especially its nervous mechanism.

The failure, or negative results of Eckhard's experiments in this direction are attributed by Roehrig to his having failed to consider that the lacteal glands do not possess an excretory duct that is in constant functional activity, but that artificial aid, in the way of suction, is required. In his own experiments, which were performed on goats, he overcame this difficulty by the aid of a special form of catheter, which he describes.

Having first determined the regular rate of secretion by experiment, the author next investigates the influence of the nerves supplying the glands, upon the secretion. Excluding the cutaneous nerves of the udder of the goat, these are derived from the external spermatic nerve, which, arising by two roots from the lumbar portion of the cord, divides into three branches, the upper one of which (the animal being in the dorsal position), passes to the abdormal muscles, while the other two follow the crural and external pudendal arteries to the udder. These two latter are the only ones that are of interest in this connection, and of these, the first, or the middle branch of the spermatic nerve, gives out, in addition to certain cutaneous branches, three twigs, as follows:

(1.) A small twig which follows the course of the vasa pudenda exterior, in order to ramify on its walls.

(2.) A much larger *ramus papillaris*, which may be followed into the teat.

(3.) One, rarely two, *rami glandulares*, which passes at once to the larger milk ducts, the cistern, and the principal evacuatory passages to ramify on their walls.

The inferior branch of the external spermatic nerve passes directly between the external pudendal artery and vein, not leaving them, but its stem may be traced to its finest branches, sometimes a little behind, sometimes directly between the vessels.

In order to prevent complications in his experiments, from struggling, on the part of the animals, the author employed narcotics and curare, and discovered an extraordinary tolerance of these drugs, in the goat. As much as eighteen grains (1.2 grammes) of morphia were given without the production of complete narcosis, and as much as two grains of curare (138 milligr.) injected into the cervical veins, to produce complete immobility.

The results of the experiments are given as follows:

(1.) Section of the papillary branch of the median divisions of the external spermatic nerve, is accompanied with no alteration in the process of the secretion of milk; the only visible effect is relaxation of the tissues of the teat, and a perceptible twinge on the part of the animal at the moment of the section.

Electrical irritation of the peripheral portion of the divided nerve caused a perceptible erection of the breast-warts, but had no influence over the milk secretion.

Centripetal irritation of the central portion of the nerve increased the secretion of milk in a reflex manner.

(2.) Section of the glandular branch, or of the whole median division, before its division into papillary and glandular branches, has for result a visible instantaneous slowing of the secretory process, while electric irritation of the separated nerve portion causes a perceptible quickening of the same.

(3.) Section of the inferior branch of the external spermatic nerve, which follows a course between the pudendal artery and vein, causes a very considerable increase in the amount of milk secretion (as much as twenty times its original amount); peripheral irritation of this nerve brings it to a stop.

From these results, Roehrig divides functionally the nerves of the milk gland into three classes:

(1.) Sensory, or reflex nerves.

(2.) Motor nerves.

a. Such as cause the erection of the udder prominences.

b. Tonic nerves, innervating the contractile elements of the milk passages.

(3.) Vaso-motor nerves, which render possible certain alterations in the calibers of the blood-vessels of the udder.

In other words, the median branch of the spermatic is a mixed nerve, sensory and motor, the former function being exercised by its papillary branch, and the motor power by it and the glandular division. The inferior division of the external spermatic nerve is, on the other hand, vasomotor in its functions.

Dr. Roehrig likewise experimented upon the action on the milk secretion of various poisons that affect the vaso-motor centres, such as strychnia, caffeine, jaborandi, chloral hydrate, etc., and which might be

supposed to affect it more or less materially. With the subcutaneous in-
jection of strychnia, which, from the great increase of blood-pressure that
accompanies its use, might be supposed to be of much effect, it was
found that the secretion was primarily very much increased, then
diminished, and finally almost suppressed. This primary increase was
not attributable to excitation of the glandular nerves, for these had been
cut. The experiments with digitalis and caffeine had somewhat similar
effects, but were complicated, in the case of the former, at least, with un-
pleasant symptoms of poisoning, goats being very sensitive to the action
of digitalis. Jaborandi was found to exert a still greater influence than
any other drug, in increasing the milk secretion; chloral hydrate and
bromide of potash very notably diminished it. Experiments were also
performed with graphic apparatus, and with sections of the vagi, which
proved incontestably the connection between the secretion and the blood-
pressure. The execution of the author's further design to test the con-
nection between the lacteal glands and the sexual organs, was necessarily
deferred on account of lack of material. He intends, however, to re-open
the investigation on the first opportunity.

THE PHYSIOLOGY OF THE VAGUS.—Rossbach and Quellhorst, *Verhand·
d. physik-med. Gesellsch. in Wurzburg*, IX. 13–31, (abstr. in *Centralbl. f. d.
med. Wissensch.* No. 42, 1876), demonstrate that vaso-motor fibres pass in
the lower vagus to the abdominal organs, and through their irritation a
contraction of the abdominal vessels, and an increase of the blood-press-
ure is produced. In order to excite the abdominal vagus, the vertebral
ends of certain ribs are resected laterally to the process of the vertebræ,
and in this manner an opening is made in the thoracic walls, the vagi are
dissected clear of the œsophagus, cut through, and the peripheral ends
electrically or mechanically irritated. In both cases there was a notable
increase of the blood-pressure in the carotid and crural arteries; the fre-
quency of the pulse remained unchanged. If merely the cervical vagus
was divided, and the heart stopped by irritation of its peripheral portion,
there followed the cessation of the excitations a rise in the blood-pressure
above the normal figure. According to Rossbach's observations, this
increase took place also after the activity of the cardiac terminations of
the vagus was excluded by the use of a moderate dose of atropine (0.004
gramme), so that excitation of the vagus can no longer produce stoppage
of the heart, and in this case directly after the excitation. This increase
of blood-pressure depends, according to Rossbach and Quellhorst, in
either case upon the excitation of certain vaso-motor fibres, which
traverse the vagus on their way to the abdominal organs; since it is reg-
ularly absent if the vagi are divided.

AT the Session of the French Acad. des Sciences, November 20, last,
M. Onimus reported the results of his experiments on the pneumogastric
and the pretended nerves of arrest. Many authors have considered that

the function of the pneumogastric was one of arrest; these authors had based their views on the fact that excitation of this nerve by induced currents, caused, at least for a few seconds, an arrest of the heart. M. Onimus has assured himself that, far from arresting the heart, a moderate and isolated excitation of the pneumogastric caused its contraction. When the excitations are too numerous, and very rapid, they have as a momentary result, an arrest, but it is only the consequence of a perturbation.

THE NERVES OF THE LUNGS.—At the session of the Biological Section of the British Association for the Advancement of Science, at its recent Glasgow meeting (rep. in *Nature*, September 21), Dr. Stirling, of Edinburgh, gave a very lucid account of his discovery of small nerve ganglia in many parts of the lung, and especially in relation to the bronchi at the base of the lung. These small collections of ganglion cells may be either in the course of the nerves, or at their forks. They are directly continued by two extremities into the gray or sympathetic nerve fibres. Dr. Stirling believed that these were local nerve-centres for the muscular fibres of the blood-vessels, controlling their calibre, and thus regulating the amount of blood passing through them.

Dr. Gardner threw out the idea that these local nerve-centres might have another function, that of regulating the capacity of different bronchi, and so varying the amount of air admitted to or expelled from particular regions of the lung. He had long believed that some such arrangement must exist, in consequence of stethoscopic observations, both on the healthy and diseased subject. Dr. Stirling suggested that this regulating power might reside in the higher nervous centers, for stimuli could be sent down through any limited number of fibres of the whole respiratory nerves.

EFFECTS OF EXCITATION OF SENSORY NERVES ON THE HEART, RESPIRATION, AND CIRCULATION.—M. Franck, Report of meeting of the French Association for the Advancement of Science in *L'Union Médical*, September 26.

The author adopts as a general formula, in summing up his observations, this idea of M. Cl. Bernard: "Arrest of the heart, or syncope, may be produced under the influence of an intense painful excitation of any nature whatever." Under the influence of a painful excitation, the heart is arrested, and this arrest is more or less marked according to the intensity of the impression, the sensibility of the animal, etc. Suppression of the pain by anaesthetics involves the suppression of the cardiac reaction, because the instrument of the cardiac manifestation is in default, the pneumogastric nerves being paralyzed.

THE ELECTRIC APPARATUS OF THE TORPEDO.—At the session of the Acad. de Médecine, of Paris, October 17, 1876, rep. in *L'Union Médicale*, M. Charles Rouget read a communication relative to the electric apparatus of the torpedo, illustrated by microscopic photographs of the terminal nerve net-work. We have, in a former number, given the princi

pal points of a former communication describing these nerve termin-
ations, to which M. Rouget refers in the remarks we quote below:

"It results," said the author, "from the histological analysis of the con-
stituent elements of the electric disks of the torpedo, described in our two
previous communications, that we do not meet in these organs, apart from
the ramifications of nerve fibres, and the reticulated nervous plate, with
anything but vessels and elements pertaining to the connective tissue
(cells, fibres, membranes). The nerve elements alone are part of the cate-
gory of organic formations (muscles and nerves), in which we observe a
development, or rather a transformation of form. As Koelliker showed,
as far back as 1857, the nerves themselves are the sole source of the elec-
tricity of the electric organ of the torpedo. By what mechanism do these
nerve elements produce these effects? This is a question, the solution of
which is, I believe, to-day possible. The trunks and the ramifications of
the electric nerves possess, as we are aware, properties and functions sim-
ilar to those of the motor nerves; they are centrifugal nerves, transmitting
the discharging force necessary for the transformation of the organic po-
tential energies (forces of tension) into active force. The action of the
nervous discharge over the force of tension, accumulated by nutrition in
the muscles (contractility), in the cells and net-works of the central gray
matter (neurility), in causing to pass into the condition of active force,
of mechanical work, of excito-motor force, sensation, or psychic action, is
also exercised on the reticulated nervous plates in question, the structure
and disposition of which presents the closest analogy with that of the
net-works of the central gray substance in vertebrates (Gerlach), and of
invertebrates (Leydig).

"In the muscles and the nervous centres, just as the activity of the or-
ganic forces is manifested under the forms of contraction, of sensation, or
of thought, a fraction of these forces of tension passes into the condition
of active force under the form of heat and electricity. In the reticulated
nervous laminae of the electric apparatus, in which neither movement or
sensation is manifested, nearly the whole potential energy (neurility), ac-
cumulated by nutrition in the terminal nerve net, is transformed into elec-
tricity. It is only a special case of these transformations of organic into
cosmic forces, and inversely, which are the very essence of vital mani-
festations of life.

The following are the titles of some recent papers on the Anatomy and
Physiology of the nervous system.

HEUBEL, On the Dependence of the waking condition of the Brain
upon External Irritation, *Pflueger's Archiv.* XIV. 2 and 3 Hft.; STEFANI,
Studies on the Functions of the Semi-circular Canals and Relation of Ex-
periments undertaken for the purpose of ascertaining their functional
Relations with the middle Peduncle of the Cerebellum, *Lo Sperimentale,*
Dec. 1875; ELISCHER, Nerves of the Ovary, *Centralbl. f. d. med. Wis-
sensch.,* Dec. 9; KUHNT, The Intermediate Medullary Sheath of the Nerve
fibre, *Ibid.,* Dec. 2; GERGENS, On Crossed Reflex Action, *Pflueger's
Archiv.* XIV, VI and VII, Nov. 20; LANGWIESER, On Accountability,
Wiener med. Wochenschr. Nov. 26.

b.—PATHOLOGY OF THE NERVOUS SYSTEM AND MIND AND PATHOLOGICAL ANATOMY.

APHASIA.—In the *Gaz. des Hopitaux*, No. 132, 1876, M. Luys gives an account of a woman aged 40, who died at the Salpetriere of slow asphyxia dependent on a bulbar lesion by descending atrophy of the pyramidal bundles. Eight years previously she had suffered with a complete right hemiplegia, the remote primary cause of which was traced back to acute articular rheumatism affecting the endocardium. With the hemiplegic attack, there was complete aphasia, which, according to the patient's account lasted about eighteen months, and then speech was very rapidly recovered. At the time of admission the speech was good; intelligence apparently intact, and the motor power to a considerable extent regained.

At the autopsy, some very striking changes were presented in the two sides of the brain; on the left side there was a spot of softening involving especially the third frontal convolution, the posterior portion of which was destroyed. The convolutions of the insula, the inferior point of conjunction of the frontal and ascending parietal, had disappeared, the first temporal was also involved, in fact, the cortical substance of the whole region of the insula and its marginal convolutions was abolished. The ascending parietal and frontal convolutions were intact in their upper three-fourths as likewise was the angular gyrus of the second and third temporal. On the inner face of the hemisphere, the first frontal, the paracentral lobe, and the crested convolution were intact; the quadrilateral lobe, on the contrary, was less developed than that of the opposite side. The grey substance of the nucleus lenticularis was completely destroyed, that of the nucleus caudatus persisted in the shape of a remnant; the optic thalamus was notably atrophied.

The right hemisphere on the other hand, presented a remarkable development in comparison with the other. This difference may be relatively represented by the following data of the weight of the two after their immersion in a bath of nitric acid; previous to drying, the left hemisphere weighed two hundred and forty-five grammes (7 2-3 oz.), the right weighed three hundred and sixty grammes ($11\frac{1}{4}$ oz.), a difference of one hundred and fifteen grammes (3 7-12 oz).

In this hemisphere there was noted a remarkable development of the first, the second, and especially of the third frontal convolutions. The ascending frontal and ascending parietal were likewise notably developed. The superior parietal region joining the ascending parietal had acquired a volume and sinuosities quite unusual in this region. The angular gyrus and the first temporal convolution were of the normal dimensions; the second and third temporal on the other hand were hard to define.

On its internal face the same hemisphere, passing from front to rear, showed a very marked development of the first frontal convolution which was very flexuous, also the paracentral lobule was strongly developed. The crested convolution was likewise clearly marked, and the quadrilateral lobe also; the fusiform convolution was very notably developed. The convolutions of the insula were very clearly defined.

The central nuclei also partook of the augmentation of volume observed in the hemisphere generally.

The left cerebral peduncle was very notably atrophied, and this atrophy extended across the pons; the pyramid of the opposite side was also affected. The same was the case with the most internal portion of the left anterior pyramid. Some lacunae existed in the gray substance of the pons.

In reply to a question of M. Ranvier in the Soc. de Biologie, when the paper was read, M. Luys stated that no microscopic examination was made. Even without this, however, the communication is of great interest and suggestiveness.

HYSTERIA.—Dr. Frank Woodbury, in an interesting paper entitled "On Cases called Hysterical," in the *Med. and Surg. Reporter*, Dec. 2, finishes with the following conclusions:

1. What has been called Hysteria is not a disease, but a symptom of disease.

2. Where the pathological source of such symptom resides in the uterus or ovaries, cases may, with some show of propriety, be termed hysterical.

3. Where the uterus and organs associated with it in function are not in a morbid condition, no symptoms can be correctly called hysterical.

4. When the diagnosis of hysteria is made, the burden of proof rests with the user of the term, to show, first, that there is co-existing uterine disorder, and secondly, that it is the direct and sole cause of the pathological phenomena in question.

5. Symptoms called hysterical may be due to reflex irritation of the great nerve centres.

6. The source of the irritation causing the reflex symptoms may reside in any other organ than the uterus.

7. Where the cause exists elsewhere than with the uterus, the symptoms are improperly termed hysterical.

8. As there is nothing in the symptoms themselves to indicate whether they are uterine, or not uterine, in their origin, the word Hysteria is of doubtful propriety, being in one case incorrectly applied, and in the other having nothing to commend it, that would counterbalance its positive disadvantage of imposing a definite pathological character upon a disease in advance of the diagnosis.

9. Medical nomenclature offers more precise expressions for the various uterine diseases than the word hysteria, while its use to describe a pure neurosis is evidently incorrect. In all cases called hysterical, the diagnosician should not be misled by a name given to a group of symptoms,

but should investigate their nature and source, and apply, in preference, a title that more clearly describes their pathological relations.

10. The progress of pathology requires the use of the word Hysteria should be very much restricted, if not finally discontinued.

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SECONDARY DEGENERATIONS OF THE CORD FROM CORTICAL CEREBRAL LESIONS.—At the Soc. de Biologie, Oct. 21, (rep. in *Gaz. des Hopitaux*, No. 124, 1876), M. Pitres offered a communication relative to the secondary degenerations of the cord in cases of cortical lesions of the brain. These secondary degenerations are not observed in all cases of cortical lesions. Certain ones, at present perfectly well known, can produce them. It results, in effect, from a great mass of observations collected by M. Pitres in the service of M. Charcot:

1. That every lesion of the brain, of whatever extent, if without the cortical motor zone, cannot produce these secondary descending degenerations of the cord.

2. That a lesion, even of very slight extent, occupying the motor points of the cerebral cortex, causes this descending secondary degeneration.

M. Pitres exhibited a number of histological specimens in support of these views.

M. Charcot called attention to the importance of these facts in relation to the question of cerebral localizations. They are indeed physical facts anatomically demonstrating the existence of certain localizations. Thus if the ascending and descending parietal convolutions and the paracentral lobe are injured, no matter to how slight an extent, it gives rise to secondary degenerations of the spinal cord, to a gross lesion visible even to the unaided eye, consisting of a gray sclerous band which may extend even as far as the lower portions of the cord. All the other regions of the brain may be previously destroyed without causing this secondary degeneration; it is not produced so long as the regions we have named are respected. These regions have not therefore the same anatomical connections as the others; they seem to form a brain within a brain.

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PATHOLOGICAL ANATOMY OF THE FACIAL PARALYSIS OF INFANTS CAUSED BY THE USE OF THE FORCEPS.—J. Parrot, and E. Troisier, *Archives de Tocologie* Aug. 1876 (abstr. in *La France Médicale*).

Paralysis in the face in new born infants, caused by the use of the forceps, was first noticed about the year, 1837 by P. Dubois, and has since been well described in the thesis of Landouzy. Its pathological anatomy, however, still remains to be determined. In all the autopsies hitherto published, the facial nerve seemed either intact or very slightly injured. The following are the results of these observations by the authors.

Alterations of the facial nerve exist, especially a diminution of consistency and change of color; the nerve appears either grayish red or grayish and semi-transparent when separated from its neurilemma. At the horizon of the stylo-mastoid foramen, we find a well marked line of demarca-

tion between the external portion which presents the appearance described and the cranial portion which preserves its normal characters. Microscopic examination shows that the nerve is steatose for its whole extent. The myeline of all the tubes is replaced by fatty matter which, according to its stage of degeneration, appears in larger or smaller globules or very minute granulations. We find no trace of the cylinder axis, and the nuclei of the neurilemma and the perineurium are fewer than in the normal condition. These alterations are met with in the same degree from the stylo-mastoid foramen to the muscular filaments in the muscles of the face.

In one of these observations in which the examination was made twelve days after birth, the muscles presented no appreciable alteration, but in the other two cases, in which the trouble was a month old, the muscles of the paralyzed side, were found to be less in volume and less ruddy in color than those of the opposite side. The fibres, for the most part, preserved their transverse striae, and did not seem to have been lessened in size, but it is probable that a certain number of them were atrophied and that others had even disappeared entirely.

We have, therefore, the degeneration of the nerve and simple atrophy of the muscles innervated by it; these lesions evidently resulting from pressure exerted upon the nerve even to the stylo-mastoid foramen. The paralysis makes itself evident immediately after the application of the forceps, as the contusion produced by the instrument is sufficiently severe and prolonged to destroy the relation between the muscles and the nervous centres. It persists as long as the consecutive degeneration of the nerve exists, and disappears as soon as the regeneration takes place. The average duration of the affection, which always terminates favorably, is six weeks. If the paralysis lasts only some hours or some days, there is reason to think that the contusion of the nerve has not been sufficient to alter its nutrition, and that it therefore only temporarily destroys its functions, and we can understand also, why the paralysis affects sometimes only a part (upper or lower) of the face, if the compression was limited to particular branches of the facial nerve.

TRAUMATIC LESIONS OF THE SYMPATHETIC.—Dr. A. Seeligmueller offered to the surgical section of the *Deutsche Naturforscher Versammlung* at its meeting last year in Hamburg, the following short paper which we extract from the *Deutsche med. Wochenschrift*, of Oct. 28.

Gentlemen: If I permit myself to bring before you a subject of not special surgical interest, you will excuse me, since surgeons are the ones who are really in the position to best observe traumatic lesions of the cervical sympathetic—affections that are by no means as rare as would appear from the small number of reported cases. In support of this statement, I can say that of the thirteen cases published, up to the present date, I myself, with quite limited opportunities of observation, have seen eight, plainly, because during the past six years, I have given special attention to the subject. Of these thirteen cases symptoms of paralysis were observed in ten, of irritation in only three.

As in physiological experiments, so the symptoms of traumatic lesions of the cervical sympathetic may be divided in three series: *viz.*, oculo-pupillary, vaso-motor, and trophic phenomena.

1. As oculo-pupillary symptoms in paralysis of the cervical sympathetic, we observe, partial closure of the eye, contraction of the pupil, and enophthalmus; in cases of irritation of the sympathetic, there was widening of the lid-opening, prominence of the orbit, and dilatation of the pupil. It would be going too far, should I dwell upon these symptoms. The most constant are the pupillary ones; the advance or retrocession of the orbit cannot always be made out with certainty. In regard to the width of the space between the eyelids, I have observed in two cases of irritation of the sympathetic, that it was not, as was expected, dilated, but notably contracted. Especially do the at present received views of the functions of the Mueller's muscle fibres need revision, since there are still other phenomena not in agreement with them. For example, we ascribe to the smooth muscular fibres in the eyelids the function of opening their eye. How comes it that in paralysis of the motor oculi, the eyelids remain shut in spite of them?

2. The second class of phenomena, the vaso-motor are much more rarely observed, than the oculo-pupillary ones. In many cases I think I have found the explanation of this peculiarity, in that the vaso-motor phenomena are not continuous, but appear spasmodically and in definite "vaso-motor attacks." These vascular symptoms in ten cases of paralysis of the sympathetic were observed only twice; in one, there is only a greater redness of the side of the face corresponding to the lesion, and in the other I observe besides a heightened temperature. In the three cases of irritative lesion of the sympathetic, described by myself, in only one did I detect vaso-motor phenomena, but in this, in so perfect a degree, that even a non-professional eye would at once notice the contrast between the pallor on the side of the lesion and the rosy whiskey-blossom tint of the other side of the face. Moreover the temperature in the meatus of the injured side was almost a degree centigrade lower than that of the other.

3. Trophic disorders are also comparatively rare, only five times in thirteen cases. As regards the space of time after the injury in which these should show themselves, we lack sufficient data. Still it seems that they may appear with extraordinary rapidity in cases of irritation of the sympathetic. In the one case, in which the vaso-motor symptoms were so prominent, the wasting of the cheeks was so far advanced on the affected side, in eight days time that not only I observed it, but the patient himself noticed it without having his attention directed thereto. In these cases of atrophy after irritation of the sympathetic, we can most readily explain the nutrition disturbances following the injury so rapidly, with Brunner, by a chronic contraction of the vessels. In cases of paralysis of the sympathetic, on the other hand, we can find no satisfactory explanation for the complicating hemiatrophy, in my opinion, except under the condition of admitting special trophic nerves.

Finally, as regards the nature and locality of the lesion in the ten cases of paralysis; in six, it was a gunshot wound, in one a punctured wound,

twice, fracture of the clavicle, and once a severe contusion in the region of the shoulder, and in the three cases of irritative trouble, the cause was contusion of the shoulder, once with, and twice without fracture of the collar-bone.

As is well known, Hutchinson has called attention to the frequent coincidence of paralysis of the brachial plexus with affections of the cervical sympathetic. That this statement of Hutchinson's is not altogether incorrect, is shown by the fact that of our thirteen cases, in nine was the brachial plexus either partially or wholly paralyzed.´ In such cases it is probable that, not the main cord of the sympathetic but the rami communicantes between it and the brachial plexus affords the parts, wounding of which produces the sympathetic symptoms.

In regard to this point the experiments I have undertaken on animals have produced as yet no certain results. A greater amount of clinical material would be useful in determining more satisfactorily the special regions, injury of which has its consequence, the usual sympathetic phenomena, and would allow us to draw conclusions as to the points of selection for the lesion that will involve more or less exclusively the fibres of the sympathetic.

It would afford me great pleasure if this short communication should induce closer observation for complications on the side of the sympathetic in all cases of injury in the region of the neck and shoulder, for I am assured that if that were done, the number of observed cases of traumatic lesions of the cervical sympathetic would soon become comparatively large.

THE DISORDERS OF SENSIBILITY IN IDIOPATHIC CONTRACTURES OF THE EXTREMITIES.—At the session of the medical section of the French Association for the Advancement of Science last summer (rep. in *Gaz. des Hopitaux*, Sept. 12), M. Manouvriez *fils* (of Valenciennes), drew from the histories of six observations of idiopathic contracture of the extremities, the following conclusions.

1. Besides various sensory troubles (painful or simply abnormal sensations), there constantly exists in tetany, outside of the attacks and after their definite cessation, a more or less accented paralysis of the sensibility to pain, to contact, to temperature and to tickling, of the skin, and often even of the mucous membranes; a paralysis having its locality preferably in the parts affected by the contractures and accompanied only exceptionally with alterations of sense. These alterations of sensibility, accompanied, moreover, with circulatory disturbances, justify the employment of bromide of potassium, which has been tried with good results by the author against this disease.

2. In the study of sensory paralyses, the æsthesiometer is indispensable to rigorously appreciate the state of tactile sensibility.

3. Clinically the analgesia resolves itself into analgesia properly so-called, or loss of sensibility to pain, so to speak, physiological, immediate or provoked, and into anodynia, or abolition of the sensibility to pain, so to speak, pathological, consecutive or spontaneous. (In one case, in

fact, a burn which was not felt at once as an injury, was felt later as a pathological process.)

4. The various kinds of sensibility (æsthesia, algesia, odynia, pallesthesia, thermesthesia, the muscular sense, and even the sense of taste, may be altered independently of each other. Perhaps it may some day be possible to show that special conductors correspond to these various sensibilities or at least that the peripheral terminal corpuscles for each of them are anatomically distinct.

ALTERATIONS OF THE BRAIN AND CARDIAC GANGLIA IN HYDROPHOBIA. —Wassilief, *Centralbl. f. d. med. Wissensch.* No. 36, Sept. 2, gives the results of microscopic examination of the brain and heart of a young woman dead from hydrophobia. The parts of the brain examined were the hemispheres, the corpora striata, the thalami optici, the pons, the medulla and the cerebellum.

The microscopic examination of the hardened and colored sections gave the following results:

1. Certain nerve cells of the medulla appeared muddy, their contours dimmed, and their nuclei obscured. Similar but more marked appearances were observed in certain of the cerebellar Purkinjes cells. 2. In the interstitial tissue of the brain there were noticed a great number of indifferent round elements, of the size of white blood corpuscles, which were very strongly tinged by the coloring matter employed. These elements (probably due to exudated white blood corpuscles) were most numerous in or in the vicinity of the perivascular spaces, though in some cases they were seen grouped six or ten together, at a distance from them in the neuroglia (proliferated neuroglia nuclei?). Finally such occur in the pericellular spaces and even in the protoplasm of the nerve cells (Kolesnikoff). 3. The blood vessels were strongly dilated and filled with blood corpuscles, the endothelium swollen in stellate patches; here and there were vessels of which the walls consisted of a finely granular, yellowish, strongly refracting substance soluble neither in absolute alcohol nor in turpentine. The most striking phenomenon, however, was the presence of the special, slightly shining, strongly refracting substance in the perivascular spaces, especially in the cortex of the hemispheres. Sometimes this substance so collected around the vessel, that in cross section it appeared to be surrounded with an irregular ring, which exercised so strong a pressure upon it that it was perceptibly narrowed; in other cases this hyaloid (according to Benedikt) substance laid in little masses, which frequently surrounded the vessel so regularly that it suggested epithelium. This hyaloid substance would take no coloring substance, and was soluble neither in strong alkalies (boiling with caustic potash), nor in powerful acids (acetic and hydrochloric); the same negative result was met with, with the employment of alcohol, turpentine, and the reaction on the amyloid substance. In other parts of the brain the perivascular spaces were more or less dilated.

In the ganglia of the heart the following appearances were noted.

1. The endothelium of the sheath surrounding the nerve cells was

swollen in stellate patches; and within the sheath and in the interstitial tissue of the ganglion were round elements of the size of a white blood corpuscle. The blood vessels around the ganglion, the great vein stem only excepted, seemed for the most part free from blood. 2. In the nerve cell itself, the protoplasm had more or less of a muddy appearance and on this account the nuclei were much obscured or quite invisible; in some cells there could be seen a collection of a finely granulated pigmentary substance. The most remarkable and unfailing peculiarity was that the nerve cells did not perfectly fill the sheath of the ganglion, but that there existed between them a free space through which the processes of the cells pass. (An altogether similar appearance was noted by Lubimoff in the cervical sympathetic ganglia in œdema from heart disease.)

In order to ascertain with certainty whether this appearance was due to œdema or to shrinkage of the cells, the author made very careful micrometer measurements and for comparison shows similar ones made by Dr. Iwanoff on normal ganglia. The result proved that the phenomenon observed was not due to shrinkage of the cells, but to expansion of the sheaths due to œdema.

THE INFLUENCE OF POLITICAL AND SOCIAL COMMOTIONS ON THE DEVELOPMENT OF MENTAL DISEASES.—The following are the conclusions of a recent work by M. Lunier, as given in the *Gaz. des Hopitaux*, No. 108, 1876.

The events of 1870–1871 caused, more or less directly, between the 1st of July, 1870 to the 31st of December, the explosion of seventeen or eighteen hundred cases of insanity. During this same period the French asylums received thirteen hundred less patients than in the corresponding period of 1869 and '70. The events of 1870–'71, therefore, had the immediate result of considerably diminishing the number of admissions into the asylums, and consequently the number of those remaining at the end of the year. The number of insane patients on the first of January, 1872, which ought to have been, all things being equal, 40,056 was in reality only 37,451, a difference of 2,605 below what might under normal conditions have been expected. The diminution of the number of admissions between July 1st, 1870 and Dec. 11, 1871, should be attributed to various direct and indirect causes, among which it is necessary to cite : *a.* the disturbance produced in the service of the asylums by the events of these years; *b.* the parsimony of certain departmental administrations; *c.* the suspension of certain etiological influences, which in periods of calm and prosperity often produce mental alienation.

The character as to the acuteness of the cases of insanity observed in 1870–'71, and their consequent rapid termination by death, or far more frequently by recovery, has likewise contributed in a certain degree to diminish the number of the patients remaining at the end of these years. But at the close of the year 1871, the number of admissions began to increase, in and 1872, it presented an altogether exceptional increase (2,785); in 1873 the additions were only 872, a proportion very near the average. This *recrudescence* in the number of admissions, which, moreover, must be at-

tributed to diverse causes, and the character of chronicity, and hence the hopeless nature of the disease in many of the newly admitted, had the effect of augmenting in very large proportion; starting with the year 1872, the number of those remaining at the end of the year, which was 40,236 at the end of 1872, and 41,108 at the end of 1873. According to all the probabilities, this last figure differs very little from that which would have been the case, without the occurrence of the disastrous years we are considering.

The occurrences of 1870–'71 temporarily diminished, but did not arrest the progressive increase of the relative number of insane placed in special asylums, which was in 1869 about 1 in 989 inhabitants, and in 1874, 1 in 964. The increase in insanity since the beginning of 1872 has been, moreover, nearly uniform throughout France.

The mental diseases caused by the occurrences of 1870–'71 were more frequent among males than among females. The increase in the entries since 1872, seems, on the other hand, to affect females more than males, but in both cases the difference is slight. Hereditary predisposition played only a relatively unimportant part in the genesis of mental alienation due to the events of these years. It has only been noted in 24 cases in 100, while under ordinary conditions its influence in various degrees, is seen in 63 cases in 100.

Among the cases of mental disease attributed to the occurrences of 1870–'71, some act only indirectly by provoking emotions that are often in ordinary times causes of insanity; but which during these years were more numerous and more accented; others acted directly on the individual. These last have been observed only in the departments occupied or threatened by the enemy; the others on the contrary, have been observed in all parts of France. The most frequently observed determining causes were, the inquietude produced by the enemy's approach, the shame and chagrin at being under their flags, the departure for the army of a cherished friend, the physical and moral fatigues of the war,—and especially the siege of Paris,—the emotions felt during a battle or a bombardment, changes of position or fortune resulting from the events, chagrin caused by the news of our reverses, politico-social excitements, the occupation of the country by the enemy. Although the causes of insanity in the patients were especially of a depressing and debilitating kind, we have observed nearly every one of the forms and varieties of mental disease commonly met with in the asylums. The expansive forms were even more frequent than the depressive ones.

If, therefore, the perturbations produced by physical causes in the functions of the brain present almost constantly the same characters, those induced by moral causes have generally no relations or only accidental relations, with the causes that bring them about. An attentive study of cases of relapse shows that in the same individual; A, the same moral cause may induce altogether diverse forms of insanity; B, causes completely unlike, produce sometimes the same and sometimes different forms of insanity.

In many of our patients, nevertheless, notably those of intemperate habits, or who were profoundly anæmic, certain symptoms of the disease suggested up to a certain point, the causes that had induced it. Among

the more frequently observed morbid phenomena, we ought to mention stupor, panophobic anxiety, sitiophobia, ideas of suicide, megalomania, hallucinations of hearing, and insane ideas of persecution.

CERTAIN REFLEX SYMPTOMS IN NERVOUS DISEASES.—We copy the following from a recent article on certain reflex symptoms in nervous diseases by Dr. V. Henze from the *St. Petersburger med. Wochenschr.* No. 35, Oct. 30, 1876.

The reflex symptoms on which I may say a few words are the following:

1. The *sinew reflex* of Erb and Westphal (*Arch f. Psych.* 1875, V. p. 792–802 and p. 803–835). Erb found that if when in a sitting position, the lower leg dependent and the foot unsupported, the ligamentum patella below the knee or the quadriceps tendon above it is lightly percussed, the leg undergoes a quick involuntary extension. Erb called this, already well known phenomenon, the "knee phenomenen." Further, repeated, passive dorsal flexions of the foot produce rapidly repeated contractions of the muscles of the calf (*foot phenomenon* of Erb). Westphal (l. c.) confirmed Erb's statements; both found that in certain affections of the cord (tabes) these reflexes were lacking, but that they were exalted in others, (lateral sclerosis).

2. The *cremaster reflex* of Jastrowitz (*Berl. klin. Wochenschr.* No. 31, 1875) serves as a diagnostic sign in dubious cases of unconsciousness in consequence of cerebral apoplexy and similar causes, in which one cannot determine the paralyzed side, and is produced by strong pressure on the thigh of the side examined, a hand-breadth above the internal condyle of the femur. If there is no paralysis, the testicle of the corresponding side is drawn up by contraction of the cremaster, but this does not occur on the paralyzed side, (Dr. Hinze states in a note, that he and his colleagues have often tried this test in recent cases of apoplexy, but in most cases without decided results; often mere raising and dropping the limb is sufficient to decide as to its paralysis, etc.).

3. The *abdominal muscular reflex* of Rosenbach (*Arch. f. Psych.* VI, 1876, p. 845). Rosenbach, repeating Jastrowitz' experiments, sought for a similar symptom that would not be dependent on sex, and found it in the contraction, or failure to contract, of the abdominal muscles when the finger nail is passed over the surface or cold substances are applied. He found in ten cases of cerebral hemiplegia that the cremaster and abdominal reflexes were lacking on the paralyzed side, the sinew reflex was, on the other hand, usually exalted. Tests as to the relation of smooth muscular fibres to cerebral hemiplegias on the nipples, showed that they did not contract or wrinkle on the paralyzed side from tickling. I shall name this fourth reflex the *nipple reflex.*

I have in my service alone, (in the nervous department of the Obuchow Hospital) examined these reflexes in sixteen hemiplegias and two tabetic patients, on three suffering from spinal meningitis, two from spinal myelitis and three from sciatica, twenty-six in all, and will here report the results. I would mention here that the cremaster and abdominal reflexes

are very easily excited by light faradization of the skin over the places named in healthy persons on the unparalyzed side of the hemiplegias. I found in hemiplegias, adding the ten cases of Rosenbach, the following:

The sinew reflex was present in the paralyzed side of fourteen cases out of seventeen; the cremaster reflex was wanting on the paralyzed side eighteen times in twenty-one cases, the abdominal reflex twenty-one times in twenty-five, the nipple reflex eighteen times in twenty-two cases. The disagreements of these members among themselves is due to the fact that Rosenbach did not observe all the reflexes. In one case of right hemiplegia complicated with aphasia, of five years duration when observed, the cremaster reflex was very pronounced on the paralyzed side while all the others failed; the motor paralysis of the arm and leg was well toward recovery, the sensible paralysis diminished, but the aphasia persisted. There were, it may be mentioned, decided ataxic movements of the right hand.

The statements of Rosenbach seem to me to be sufficiently confirmed. I must still add that the contractions of the abdominal muscles and the nipples cannot always be well distinguished, and that I made the most of my observations in a rather dark summer barrack, where many insignificant contractions may have escaped me. I cannot agree with Eulenberg's opinion that these muscular reflexes more commonly fail in recent cases, for I could not satisfy myself that the duration of the disease had such an influence.

The statement of Westphal that the sinew reflex fails in ataxia is confirmed by my two cases, it also failed in the cases of myelitis. Those of meningitis gave no constant result. In the three cases of sciatica it was interesting to notice that the cremaster reflex was suppressed during the disease and reappeared again upon recovery.

The question now occurs how these reflexes take place. The first author, Erb, describing the sinew reflex, laid the "knee phenomenon" directly to mechanical irritation of the sinew of the patella and quadriceps. The "foot phenomenon" is explained by him as follows: that by the tension of the tendo-Achillis a contraction of the calf muscles is incited in a reflex way; by cessation of this contraction a spasm causing extension again takes place, and soon. Westphal thinks that the cause of each contraction must be sought in a direct mechanical phenomenon,— extension of the muscle through concussion of the tendon, and although the existence of sensory nerves in the tendons is doubtful, their presence is not an essential condition of these reflexes. These very plausible theories, which have strong support in the well known great mechanical irritability of the muscles, especially in pathological conditions, are contradicted by some very valuable experiments on animals by Schultz and Feurbringer (*Centralbl. f. d. med. Wissensch.* No. 54, 1875). They experimented upon rabbits, with the following results. 1. After laying bare the patellar sinew and the surrounding muscles they caused contraction of the quadriceps and the flexors of the foot by a blow on the tendon, not only of the same but of the opposite side. 2. Division of the quadriceps tendon did not prevent the phenomenon. 3. Section of the crural nerve destroyed the sinew reflex, but not the mechanical irritability of the muscles (in

opposition to Westphal's views). 4 Paralysis of the nerve by curare destroyed the sinew reflex but left the mechanical irritability of the muscles intact. These experiments clearly showed that for the sinew reflex, as for other reflexes, the integrity of the sensory nerves was a *sine qua non*. But the presence of such nerves in the sinews of a number of animals has been proved by Sachs (*Reichert a DuBois Reymond's Archiv.* IV. 1875). Sachs says (*Deutsch med. Wochenschr.* No. 5, 1876): The point of entrance and course of the nerves show characteristic relation for each separate sinew. The final termination, as a rule, is in the passage into its non-medullated branchlets which are perceived thick and bushy in a plate-like mass. In an interesting sinew (M. Sterno radialis, Cuvier) there are peculiar terminal bodies (*Sehnenendkolben*) plainly for the perception of the tension which the sinew receives from the muscular force.

The route of the sinew reflex is somewhat as follows: sensory nerves of the sinew, crural nerve, the four upper lumbar nerves as sensory routes, the lumbar cord as the place for the reflex centre, and the motor fibres of the crural, the muscular nerves of the quadriceps and the extensors of the foot as the motor routes.

As to the suppression of the cremaster reflex in the paralyzed, Jastrowitz thinks that it depends directly upon a motor disorder of the abdominal musculature, set up simultaneously with the other motor disturbances by the apoplectic lesion. Since the cremaster is formed from the muscular bundles of only the lower portion of the united mass of the transverse and oblique muscles, and moves with them, he assumes a paralysis also of this part. To the possible objection to this view, that so partial a paralysis of two muscles could not take place leaving so much of them intact, he offers the fact of the not very infrequent paralysis of the gluteal muscles in hemiplegia.

The routes of the cremaster reflex are; nervous cutaneous femor. ant. med., the two sapheni, and the anterior branch of the obturator as sensory routes, the upper part of the lumbar cord as relay, and the external spermatic nerve as a motor route.

Rosenbach also refers the failure of the abdominal muscle reflex to a paralysis of the abdominal muscles, and rightly denies the possibility of a direct muscular irritation acting in the production of these reflexes, as when we strike the muscles with a percussion hammer.

As routes for this reflex, he gives the ilio-hypogastric and ilio-inguinal nerves, and the anterior abdominal cutaneous nerves as the sensory ones. the lumbar and dorsal cord as reflex centres, and the ilio-hypogastric nerve as the route of motor transmission.

The nipple reflex is only incidentally here of interest.

In reviewing the experiments for the explanation of the absence of these reflexes in hemiplegias, it appears that no observer has paid attention to the almost inevitable accompaniment of the motor paralysis, the hemianæsthesia. I very often found in my examinations the points from which these reflexes are excited, anæsthetic, and this fact suggests to me the question whether or not the paralysis of the sensible integument or the sinew nerves, was not the cause of the failure of the phenomenon, and not the paralysis of the muscle. The cause of the sensory paralysis in hemiplegia, as well as in

the spinal affections is in the nervous centres, which, in case the peripheral irritation is not conducted to it by senory nerves, cannot reflect the impulse along the motor nerves. The above mentioned case in which with the recovery of the central apparatus the reflex also re-appeared.

My material is still too scanty, and the time as yet too short to produce all the desired results; but it seemed to me that I ought to assert my view, since, should it prove correct, it would afford a very useful symptom for the prognosis of hemiplegias, in the re-appearance of these reflexes. Clinically, these reflexes have no very great importance, except the sinew reflex. I desire with my communication to incite investigation in other interesting points in regard to this subject. Perhaps it would be in looking up the following questions.

1. What are the relations to each other as to time of the motor and sensory paralysis, both in regard to their appearance and their disappearance?

2. Are the muscular reflexes always absent in unilateral anæsthesia?

3. Is the improvement in the motor paralysis parallel with the re-appearance of the reflexes?

SPASMODIC TABES DORSALIS.—Isadore Bétous *These de Paris*, 1876 (Abstr. in *Rev. des. Sci. Med.*)

Under this name the author describes a rare affection of the nervous system, already noticed by Erb, and to which M. Charcot has devoted an interesting lecture. The dominant character of this new disease is spasm and contracture, at first slight, but which become more and more pronounced and soon reduces the patient to complete debility. Motility is alone affected in these patients, the reverse being the case with ataxics in whom various troubles exist. It is rational to suppose that the anatomical lesions occupy the lateral columns, but the lack of an autopsy up to the present time prevents any localization of the alteration in the cord.

The evolution of spasmodic tabes dorsalis is eminently slow; it may be divided into three periods characterized each by special symptoms:

1. Gradual paresis of the inferior limbs, accompanied rarely with painful symptoms; no disorders of sensibility.

2. Contracture invading the same lower limbs and affecting the progressive in a peculiar manner. Spontaneous and induced tremors, the former constant.

3. The paresis and contracture attack the superior members; this condition may persist, improve or disappear. The contracture of the inferior members, increases so far that progression generally becomes impossible; it sometimes affects the abdominal muscles.

Sensibility is always preserved intact, spontaneous painful sensations are rare. There is neither muscular atrophy nor rectal, vesical, or genital disorders. The general state of health is good, the cerebral functions are accomplished normally.

This affection should not be confounded with transverse myelitis. The latter begins brusquely, is accompanied with paraplegia, urinary disorders anæsthesia, and only in the late stages of the disorder do we see contractur

and tremors. It is the same with myelitis by compression and locomotor ataxia, which differ essentially from spasmodic tabes. Multiple sclerosis resembles this disease somewhat in paraplegia of the lower limbs, and the tremor, but we never meet in this affection with the tremor of the members, the embarassment of speech and the disorders of intelligence, and of the centres which are the regular thing in multiple sclerosis.

Amyotrophic lateral sclerosis, is of all spinal affections, the one that most resembles spasmodic tabes, but the muscular atrophy of the superior members, never met with in tabetics, suffices to distinguish it.

The progress of the disease is slow, and it does not endanger life.

Treatment has so far been ineffectual, still the use of the ascending current, and hydropathy are to be advised.

TWO DIFFERENT FORMS OF TETANUS DIAGNOSED BY PNEUMOGRAPHY. Ch. Richet *Gaz. Méd. de Paris*, No. 14, 1876, (Abstr. in *Rev. des Sci. Méd.*)

Death in tetanus coming on most frequently from asphyxia due to contraction of the respiratory muscles, it becomes a matter of interest to make a minute analysis of the mechanical phenomena of the respiration by means of the pneumograph.

The respiratory trace obtained some hours before death from a patient suffering from tetanus showed that at certain moments there was a sort of pause in the respiration and that it occurred during the expiration. There was therefore expiratory spasm, one that might involve the expiratory muscles and those of the glottis.

In another case death occurred in a sort of ill-characterized comatose condition, but without asphyxia. But in him the tetanic pause was during inspiration and not expiration.

Thus, in those two cases we have marked differences, since in the first there was arrest of expiration and in the second, arrest of inspiration. One practical consequence follows. If the spasm is due to the inspiratory muscles, tracheotomy is useless, and we may hope for a cure since the hæmatosis will suffice to permit therapeutic agents being absorbed and modifying the medullary excitation. If, on the other hand, there is spasm of expiration the prognosis will be graver, since hæmatosis at a given moment will be *nul* or insufficient, and asphyxia imminent. In such cases as these M. Verneuil has been able to revive by tracheotomy the unfortunate victims of tetanus who were becoming asphyxiated.

Thus in a diagnostic point of view in view of the prognosis and the treatment, it is of importance to recognize two forms of tetanus: that in which the tetanic spasm affects inspiration, and the other, in which it involves the expiration.

ALBUMINURIA AS A RESULT OF EPILEPTIC ATTACKS.—Dr. Otto, *Berliner klin. Wochenschr.* publishes the results of his investigations as to the presence of albumen in the urine of epileptic patients. He analyzed the urine in thirty-one cases, and found one or more of the albumen reactions in twenty-two, fourteen times a precipitate and ten times turbidity. Hence,

he concludes that transitory albuminuria is to be considered one of the symptoms of the epileptic attack. Still, the frequent absence of this symptom and its short duration deprive it of any special practical importance.

The following are among the articles recently published on the Pathology of the Nervous System and Mind and Pathological Anatomy.

WITKOWSKI, On the Melancholic Initial Stage of Insanity, *Berl. klin. Wochenschr,* Dec. 11; BERNHARDT, Communication on Athetosis, *Deutsch. med. Wochenschr.,* Dec. 2; SARTISSON, Color Blindness and Railway Service, *St Petersburger med. Wochensch.* Nov. 20—Dec. 2, 1876; MOOS, On the Connection between Diseases of the Auditory organs and those of the Fifth Nerve, *Virchow's Archiv.,* LXVIII, III, Nov. 13; A. MITCHELL, Contribution to the Statistics of Insanity, *Jour. of Mental Sci.,* Jan. 1877; D. HUCK TUKE, On the Prevalence of the causes of Insanity among the Ancients *Jour. of Ment. Sci.,* Oct. 1875; PEDDIE and BUCKNILL, Letters on the Relation of Drink and Insanity, *Ibid;* CARRE, Nervous Hæmoptysis, *Arch. Gen. de Med.,* Jan. 1877; ALBERTONI, The Influence of the Brain on the Production of Epilepsy, *Archivio Italiano,* Nov. 1876; BOUCHUT, The Nature and Treatment of Tetany or Contracture of the Extremities in Infants, *Gaz. des Hopitaux,* Dec. 12; DU SAULLE, The Epileptics, *Ibid* (cont. art); HUGHLINGS JACKSON, On the Embolic Theory of Chorea *Brit. Med. Jour.* Dec. 23; MACKENZIE, Coincidence or Correlation? A Note on the Embolic Theory of Chorea, *Ibid.;* BERGER, On the Pathology of Rheumatic Facial Paralysis *Deutsche med. Wochenschr.* Dec. 9; BIANTE. General Paralysis as a predisposing cause of Fractures, *Ann. Med. Psych* Nov. 1876; SIMON, The Imagination in Insanity, *Ibid.*

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c.—THERAPEUTICS OF THE NERVOUS SYSTEM AND MIND.

HYDROBROMIC ACID.—Dr. A. McLane Hamilton, *Phil. Med. Times,* Oct. 28, gives the following testimony on this agent.

" In appearance it is a straw-colored liquid, with an agreeable acid taste, and a slight odor of bromine. It combines very readily with many substances, and may be given with tinct. ferri. chlor., strychnine, etc. It prevents the headache caused by the iron, when given to persons who are anæmic (Fothergill). It dissolves a large amount of quinine; and Gubler found that the head-effects of that drug were not produced when this combination was employed.

" In small doses it acts very much as the bromides do, but with much more intensity. Half a drachm is fully equal to one drachm of the bromide of potassium. It differs, however, in the want of permanence of its effects, the base of the bromic salts seeming to favor retention. In epilepsy it is not serviceable. I have used it in three cases. One case in which the attacks were monthly, and bromide was administered, was aggravated by H. Br. The attacks increased in violence and frequency. Large doses did no good; and it was only in one case that it accomplished anything.

"In hysteria, Fothergill found it to be a valuable remedy.

"Its indication, I think, is in that form of cerebral hyperæmia which is of sudden origin and dependent upon a disturbed heart action and general debility. In the nervous condition following the abuse of tea, coffee, or tobacco, alcoholism, insomnia due to congestion, and a number of varieties of disturbed cranial circulation it is the best remedy I know of."

After giving accounts of four illustrative cases of insomnia, cerebral disturbance, etc., Dr. Hamilton concludes with the following paragraphs:

"It may be given just as the bromides are and the observers I have mentioned, (Wade and Fothergill) have used it to allay bronchial irritation, to stop the vomiting of pregnancy and for other morbid states accompanied by reflex excitement.

"Its advantages are its reliabiliiy, its ready decomposition in the stomach, and its agreeable form.

" I have prescribed it with essence of lemon and sugar, in which form it makes a pleasant drink. It should always be well diluted with water."

In regard to the above statements of Dr. Hamilton, Dr. J. Milner Fothergill, *Phil. Med. Times,* Dec. 9, says as follows:

"I have just perused with satisfaction the remarks of Dr. McLane Hamilton on this useful therapeutic agent. His cases are very illustrative of its action. A friend told me the other day that he had prescribed it for the nervousness and flushings of the change of life with excellent effect.

"I may add that it is most useful in that form of excito-neurosal palpitation found in women along with general nervousness. In such cases, given with quinine, and in some cases, a small dose of digitalis where the heart is weak, it produces the most satisfactory results.

"It also forms part of a really charming cough-mixture, efficient as well as palatable. The form is as follows:

R. Sp. Chloroform, - - M. XX.
Hydrobromic acid, - - ℨss.
Syr. Scillæ, - - - ℨi.
Aq. ad ℨi, ter in die.

"The dose, of course is reduced for children. Any other acid in this mixture is very agreeable, that the hydrobromic acid, from the effect of bromine upon reflex mechanism, allays the cough often so troublesome. It possesses much the same action as opium, without the ill effects upon the digestive organs or on the bronchial secretion."

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GALVANIZATION OF THE SYMPATHETIC.—O. Tschetschott, *Diss. Russ*. St. Petersburg, 1876, (Abstr. in *St. Petersb. med. Wochenschr.* No. 32. Oct. 9-21, 1876.

Since attention was called by Remak to the therapeutic uses of galvanization of the sympathetic, this method has come more and more in repute, during the past ten years, and has been employed in the most various disorders, both with and without effect. Its literature has become very extensive; the quantity, however, is more noticeable than the quality. In his introduction, the author has undertaken the difficult task of collecting together and classifying this literature as far as is possible. The diseases in which this method has been employed he divides into three groups: 1. Diseases or symptoms that depend exclusively upon pathological alterations of the fibres or ganglia of the cervical sympathetic, such as hyperidrosis unilateralis, hemicrania, hemiatrophy, etc. In most of these cases some results were obtained. 2. Diseases dependent, either directly or indirectly upon some alterations in the brain, such are vertigo, hypochondria, insanity, hemiplegia, etc. 3. Various disorders for which one can state no well defined reason why galvanization of the sympathetic should be of any value. Here belong sciatica, anæsthesia, ataxia, arthritis nodosa, chronic eruptions such as eczema in spinal affections, etc. Since the literature shows that there are so many different views as to the employment of this method, the necessity is evident that their scientific basis should be laid down with greater precision and with the aid afforded by physiology. The author applies the experimentally determined facts to man, which show that the function of the sympathetic is the same in man and the lower animals, and that the galvanic current affects the cervical sympathetic directly when applied to the living subject. He gave himself to the task of confirming by experiment the fact that in man also, alterations of temperature in the region of its distribution followed from the direct action of the galvanic current on the cervical sympathetic. For this purpose he constructed a special form of thermometer bent at a right angle, so that, when its bulb was inserted in the external meatus, it could be fastened to the head by a band. With this he was able to obtain accurate measurement of the variations of the temperature. He made four series of experiments to ascertain (1) what alterations of temperature were produced in healthy persons by the irritation by the cathode of the upper cervical sympathetic; (2) what changes resulted in sound persons from various applications of the current; (3) the same in invalids; (4) the action of the induction stream. The experiments were so performed that after the thermometer was inserted in the meatus, one electrode was placed in the temporo-auricular-fossa just under the ear, and the other over the sternum.

1. Placing the cathode on the cervical sympathetic (12 experiments) produced each time a slight decrease of heat, (0.2° to 0.5° Cent.) and pallor of the skin of the corresponding portion of the head. With the use of stronger currents a dilatation of the pupil frequently ensued.

2. Placing the anode on the cervical sympathetic produced similar effects, but in a lesser degree. If the current was turned on gradually by

means of the rheostat, no effect followed the use of the cathode; the anode caused an increase of temperature in the region supplied. (6 experiments.)

3. The nine experiments on insane patients, melancholics, etc., produced no result, which was likewise the case with

4. Those undertaken with the induction current.

As regards the various methods of application of the electrodes, the author has instituted some collateral experiments and found that if he placed them in the region of the cervical or dorsal vertebræ, the temperature phenomena were less constant and apparent. As to the alteration of the pulse during the galvanization, he states that in a number of experiments no influence was noticed, but in some there followed a retardation and alteration of the sphygmographic trace; either the curve was shorter and steeper or the dicrotism disappeared and the ascending line increased and the descent followed more gradually.

His conclusions as to the therapy are summed up as follows: galvanization of the sympathetic is to be employed in direct affections of the nerve, or where disease is due to alterations of the organs that stand in direct connection with the functions of the sympathetic: in all other cases it has no scientific basis and is useless.

THE ACTION OF ALCOHOL ON THE BRAIN.—At the meeting of the British Association for the advancement of science last year, (rep. in *Nature*, Sept. 28) Mr. C. T. Kingzett read a paper on the action of alcohol on the brain. He said the question of what became of alcohol taken into the system had been extensively studied. Thudichum was the first to determine quantitatively the amount of alcohol eliminated by the kidneys from a given quantity administered, and the result he obtained was sufficient to disprove the elimination theory then widely prevailing. Dupre and many others continued these researches from which,—according to Dupre, they might fairly draw three conclusions: (1.) that the amount eliminated per day did not increase with the continuance of the alcoholic diet, therefore all the alcohol consumed daily must, of necessity, be disposed of daily, and as it was certainly not eliminated within that time, it must be destroyed in the system; (2.) that the elimination of alcohol following the taking of a dose was completed twenty-four hours after the dose was taken; and (3), that the amount eliminated in both breath and urine was a minute fraction only of the amount of alcohol taken. In 1839, Dr. Percy published a research on the presence of alcohol in the ventricles of the brain, and indeed, he concluded "that a kind of affinity existed between the alcohol and the cerebral matter." He further stated that he was able to procure a much larger proportion of alcohol from the brain, than from a greater quantity of blood than could possibly be present within the cranium of the animal upon which he operated. Dr. Marcet, in a paper read before the British Association in 1859, detailed physiological experiments which he considered to substantiate the conclusions of Dr. Percy, inasmuch as they demonstrated that the alcohol acted by means of absorption on the nervous centres. Lallemand, Perrin, and Duroy had, moreover, succeeded previously in extracting alcohol from

brain matter in cases of alcoholic poisoning. But all these researches left them entirely in the dark as regarded the true action, if any, of alcohol on cerebral matter, and no method of investigation was possible until the constitution of the brain was known. Thudichum's researches in this direction, together with some more recent and published investigations by Thudichum and the author, had placed within reach new methods of inquiry regarding the action of alcohol on the brain. In his research he (Mr. Kingzett) had attempted this inquiry by maintaining the brains of oxen at the temperature of the blood in water, or in water containing known amounts of alcohol. The extracts thus obtained had been studied in various ways, and submitted to quantitative analysis, while the influences exerted by the various fluids on the brain had been also studied. These influences extended in certain cases to hardening and to an alteration in the specific gravity of the brain matter. Water itself had a strong action on brain matter (after death) for it was capable of dissolving certain principles from the brain. It was notable that water, however, dissolved no cephaline from the brain. Alcohol seemed to have no more chemical effect on the brain than water itself, so long as its proportions to the total volume of fluid did not exceed a given extent. The limit would appear to exist somewhere near a fluid containing 35 per cent. of alcohol. But if the percentage of alcohol exceeded this amount, then, not only a larger quantity of matter was dissolved from the brain, but that matter included cephaline. Such alcoholic solutions also decreased to about the same extent as water, the specific gravity of brain substance, but not from the same cause; that was to say, not merely by the loss of substance and swelling, but by the fixation of water. Many difficulties surrounded the attempt to follow these ideas into life, and to comprehend in what way these modes of action of water and alcohol on the brain might be influenced by the other matters present in blood. On the other hand, it was difficult to see how any of the matters known to exist in the blood could prevent alcohol, if present in sufficient amount, from either hardening the brain (as it did after death) or dissolving traces of its peculiar principles to be carried away in the circulation; that was to say, should physiological research confirm the stated fact that the brain in life absorbed alcohol and retained it, it would almost follow of necessity that the alcohol would act, as he had indicated, and produce disease, perhaps *delirium tremens.* Dr. McKendrick said Mr. Kingzett's researches into the chemistry of the brain and the action of various agents upon it were a valuable step in the right direction. This was essential if the mode of working of the brain were ever to be understood; but it would be a long way from the knowledge of the dead tissue to the comprehension of its vital action. No doubt alcohol had a marked effect upon the connective tissue elements in the brain. He suggested as a useful method of research the submitting of a certain class of animals for a length of time to the action of a definite amount of alcohol, and then examining their brains to discover what effect was produced. The investigation was of very great importance as regarded the treatment of drunkards; no doubt in many cases where it was thought that they had to do with merely moral evil, there was a fundamental change in physical organization. Prof. Burdon-San-

derson said the question was one that ought certainly to be taken up by government, and the best man in the country should be engaged upon the inquiry. It had a most important bearing upon the welfare of the community and the diminution of human suffering.

NITRITE OF AMYL.—Sigmund Mayer and J. J. Freidrich, *Arch. f. exp. Path.* V. p. 55. (Abstr. in *Revue des Sci. Méd.*)

Apparently the most rational procedure in experimenting on the effects of nitrite of amyl is to cause it to be inhaled through a tracheal fistula, or or to there insufflate it in curarized animals. This was the method likewise employed by Filehne.

The authors have investigated the effects of this drug.

1. On the circulatory apparatus.
2. On the respiratory apparatus.
3. On the motor apparatus, represented by the striated muscles.
4. On the economy in general.

1. Small or moderate doses, *i. e.*, the continued inhalation from 4 to 60 seconds, produced the same effect in the rabbit and the dog,—accelerations of the heart beats. This acceleration, as numerous experiments have demonstrated, is solely due to weakness and excitability of the moderator centres. When the inhalation is prolonged over a minute, ordinary excitants become almost inactive as regards that part of the brain corresponding to the nerves of arrest.

Intra-vascular pressure is considerably diminished; moreover, nitrite of amyl also paralyzes the muscular fibres of the heart and induces the arrest of its pulsations when introduced directly into its substance by injection. It appears certain that this diminution of pressure is attributable to the loss of muscular tonicity and not to the influence of the vaso-motor system.

2. The respiratory movements are directly influenced by nitrite of amyl, and present a considerable acceleration and increase of depth. Nevertheless, when the dose is large, the respiration becomes slower and more superficial than before. We have then, not a reflex action but a direct action on the respiratory centres.

3. The motor apparatus is quickly super-excited by nitrite of amyl; small doses produce cramps, larger ones induce tetanic contractures. These effects have for their direct cause the excitation of certain portions of the brain, and not modifications of the circulation or innervation of the striated muscles.

4. The general action of nitrite of amyl may be interpreted in the following manner:

Small doses act only on the nervous centres, causing diminution of the tonicity of the vagi, the excitation of the respiratory nerves, and of the nervous centres. Large doses go farther and produce a poisoning of the striated muscles.

THE TREPHINE IN CEREBRAL TRAUMATISMS.—The following condensed statement of the relations of the skull to the brain and their practical im-

portance is taken from the editorial columns of the *Progres Médical*, Oct. 14, 1876.

In 1851, M. Broca, in connection with his investigations as to the influence of lesions of the third frontal convolution on the faculty of speech, studied the relations of the frontal lobes with the skull by driving wooden pegs through the bony walls of the cranium. These researches have recently been taken up in England by Turner; in Germany by Huftler and Ecker; in Russia by Landzert; in France, by a pupil of M. Charcot, M. Féré, by de la Foulhouze, and lastly by M. Broca himself, a second time. M. Féré, in particular, has indicated in a very precise manner the procedures for finding in the living man through the integuments, the relations of the principal portions of the brain to the cranium. He first determines the position of the fissure of Rolando. When the head is perfectly horizontal, the lower end of this fissure of Rolando is on a line, parallel to the horizon, drawn from the external part of the arch of the eyebrow to the point of junction of the auriculo-bregmatic plane, or the vertical plane passing through the external auditory meatus. The posterior and superior extremity of the fissure is, in the average of cases, about 45 millimetres behind the bregma, in the female, and in the male 47 millimetres, or more simply $4\frac{1}{2}$ to 5 centimetres. But the bregma is situated at the point of junction of the vertical auriculo-bregmatic plane with the vertical median antero-posterior plane of the skull. With the aid of this elementary conception, it is easy to determine quite closely the sub-osseous arrangement of the principal lobes, and of some groups of convolutions.

Furthermore, it is sufficient to remember that it is in the neighborhood of the fissure of Rolando, we find the motor regions of the cerebral hemispheres. The usefulness of this experimental result in cases of injury of the skull is unquestionable. When, following a fall on the head, a patient presents persistent disorders in the upper or lower extremities or in a single one of either, or in the face, when he is affected with epileptic attacks always beginning in one or the other of these parts; in short when the symptoms are definitely localized, we may then suspect a lesion somewhere near the fissure of Rolando. Is it a simple contusion of the brain or a fracture with depression and compression of nervous substance by fragments of bone? It seems to us that, according to our actual knowledge of physiology, a simple contusion would be rather characterized by a local paralysis, at least before the period of inflammatory reaction.

The persistence of a primitively appearing localized contracture, and frequent epileptic attacks may justify the opinion that there is compression from a splinter of bone. We judge then that the exact point to apply the trephine is in the vicinity of the fissure of Rolando. When such a lesion occupies the frontal lobes, properly speaking, according to the doctrine of localizations, we ought not to meet with motor troubles unless the injury is very profound, reaching the central ganglia.

There are no more simple theoretical suppositions than the considerations we have developed. Among the facts most worthy of attention we may cite a successful case of trepanation by M. Lucas-Champonniere in

the service of M. Panas. The patient presented successively transitory paralysis, contracture, and epileptiform attacks in the arm for many days; he was cured by the application of the trephine and the removal of an osseous scale which contused and compressed the brain in the vicinity of Rolando's fissure. M. Broca in his remarks before the Acad. de Médecine, said that, guided by these ideas as to the relations of the brain to the skull, he could empty at once an intra-cerebral abscess the location of which was then indicated.

Perhaps in the future we may be as fortunate elsewhere when observation shall have indicated a localization of a lesion in the recently discovered motor centres. Moreover, if we are to credit a long and conscientious memoir, that appeared recently in the *Archiv. f. klin. Chirurgie*, the time for the rehabilitation of the trephine is not far distant: under the influence of the searching criticisms of Malgaigne we have perhaps too quickly condemned this operation, which, though sometimes hazardous, should not have been completely abandoned.

It seems probable, therefore, that the diagnosis and treatment of cerebral traumatisms may find important indications in the symptoms furnished by localized lesions, when they become generally known, and when the importance they merit is accorded to them.

Butyl chloral.—Prof. Eulenberg at the session of the Med. Vereins at Greifswald, Aug. 5th, 1875 (reported in *Deutsche med. Wochenschr.* No. 43), gave the following as the results of a series of experiments on rabbits undertaken at his suggestion by Dr. Windelschmidt, to ascertain the physiological action of butyl-chloral ($C_4 H_5 Cl_3 O$). They confirmed in all essential points the results previously obtained by Liebreich. In small doses (0.28 to 0.49 injected hypodermically) it had a hypnotic action on rabbits. In larger doses (0.6 to 0.86) its effect was at first anæsthetic, beginning with the head and passing over the whole body, ending in complete narcosis. The respiration was quickened by the minimum dose, by small doses notably showed, and paralyzed by large ones. The pulse was unaffected by small doses and but slightly altered by larger ones, probably only as a secondary result of the decreased frequency of the respiration. The temperature was heightened at the beginning independently of the rise of the pulse, and later, was notably diminished.

Lactate of Soda as a Hypnotic.—Dr. Erler, assistant physician at Neustadt Eberwald Insane Asylum, publishes (*Centralblatt. f d. med Wissensch.* No. 37, 1875) the results of clinical experiments on the hypnotic action of lactate of soda in the insane. He used it in the cases of seven female patients, partly recent attacks, partly chronic cases, but with positive results in only one. In another case it appeared to have an effect on one occasion, but as similar effects were observed at other times in the same patient without this medication, it is uncertain that we are to attribute to it the calmative influence.

In the other cases no effects whatever were observed to follow the use of the drug. Still no unfavorable action could be attributed to it. Dr. Erler thinks, therefore, that he cannot, with the evidence, class lactate of soda among our generally effective sleep producers.

THE POISONOUS ACTION OF THE PHOSPHATES AND OF VANADIUM AND CHROMIUM AND THEIR COMPOUNDS.—At the recent meeting of the British Association for the advancement of science, in its biological section (reported in *Nature*, Sept. 21), Prof. Gamgee, Mr. Leopold Larmuth and Dr. Priestly presented the results of a valuable series of researches on the action of certain special poisons. Vanadium and its compounds had been especially investigated, and found to be irritant poisons, rapidly causing death, often preceded by paralysis, convulsions, etc. When much diluted the solutions act injuriously on bacteria, germinating seeds, fungi, etc. The results are the same whether the solution is injected into the skin, the veins, or the alimentary canal of higher animals. Both be fore and after division of the respiratory nervous centre, vanadium causes in the first instance a stimulation, and in the next a depression of respiration. When the muscles and nerves of a frog poisoned with vanadium were tested by electricity after reflex irritability was entirely destroyed, the work done by the muscles showed no differences from that of non-poisoned muscles. The action of vanadium in the heart of frogs is curious; when vanadium is injected, the inhibitory centres acting on the auricles are not affected, but the vagus nerve loses its power of inhibiting the contraction of the ventricle. This result causes a dilemma which cannot yet be resolved. for it appears that vanadium is not a poison of the muscular fibres. Experiments have also been made on the relative poisonous activities of the ortho, meta, and pyro-phosphoric acids, and they have been found to vary considerably in their intensity. Further, a relationship in the various phenomena produced has been made out between the differate phosphates and vanadates. Investigations relating to chromium, in which rabbits, guinea pigs and frogs were employed, demonstrate considerable differences in its physiological action from that of vanadium. At first it induces irritation of the alimentary mucous membrane, and secondly it acts directly on the principal nervous centres, causing convulsions, paralysis, vomiting, a fall of blood pressure, and a sudden and temporary stoppage of the heart in dilitation. It is not specially a poison of muscle or of nerve trunks.

In the discussion which follows the reading of these papers, Prof. Kronecker, of Leipzig, expressed his opinion that the vanadates were really poisons of the muscular substance of the heart, and he accounted for the difference between the action of the auricle and ventricle by supposing a certain difference between the muscular substance of these two chambers. Dr. McKendrick who presided in this department, said that Prof. Gamgee's researches showed the advantage of the combination of the highest chemical with physiological knowledge, and they led to the hope that ultimately some definite laws would be discovered regulating the relations

between chemical constitution and physiological action. The field of inorganic chemistry was a very fertile one for this purpose, and much more likely to yield great results of this kind than the more complex considerations of organic chemistry. One important result was confirmed by Prof. Gamgee's investigations, that the larger the molecule of a substance the more powerful was its operation, but this was affected also by the stability of the molecule.

SANGUINARINA.—Dr. R. M. Smith, *Am. Jour. of Med. Sciences*, Oct. 1876, publishes the results of a series of experiments on frogs, dogs, cats, guinea pigs, rabbits, etc., to determine the physiological action of sanguinarina. The preparation used was prepared by Hance Bros. & White, of Philadelphia, which he thinks from comparison with other samples, those of Merck for example, was fully as pure and strong as any. The general results of his experiments (153 in all) are summed up as follows:

1. Sanguinarina destroys life through paralysis of the respiratory centres.

2. It causes clonic convulsions of spinal origin.

3. It has no effect on either the motor or sensory nerves.

4. It causes marked adynamia and prostration from its depressing action on the spinal ganglia and muscles.

5. It decreases reflex excitability through irritation of Setschenow's centre, and by ultimate paralysis of the spinal ganglia, from large doses.

6. It produces in cats, dogs, and rabbits, a fall of pulse and blood pressure, the fall of the latter being preceded by a temporary rise after the administration of proportionately small doses.

7. The fall of blood tension is caused by a paralysis of the vaso-motor centre, and by a paralysis of the heart itself, probably of its muscular structure.

8. The temporary rise in blood pressure is due to irritation of the vaso-motor centre, previous to its paralysis, by small doses.

9. The reduction in the pulse is due to direct action of the poison on the heart, through paralysis of its motor power.

10. Sanguinarina has no action on the liver.

11. It causes marked salivation.

12. It slows the respiratory movements by prolonging the pause after expiration.

13. The reduction is caused by loss of tonus of the respiratory centre.

14. Small doses cause an irritation of the respiratory centre, and consequently, an increase in the number of respiratory movements.

15. Applied locally, sanguinarina soon causes complete paralysis of striped muscular fibre.

16. It always causes dilatation of the pupil.

17. It is an emetic.

18. It always lowers the temperature.

19. When introduced into the circulation, it diminishes muscular contractility.

These experiments were performed at the Physiological Laboratory of the University of Pennsylvania, under the supervision of Dr. I. Ott, demonstrator of experimental Physiology.

The following have also recently been published on subjects relating to the Therapeutics of the nervous system and mind.

SCHWEIG, A clinical contribution to the effect of acute Bromization, *N. Y. Med. Record*, Dec. 30; LASEGUE and REGNAULT, Therapeutics as judged from Statistics, *Arch. Gen. de Med* Jan. 1877; HAMMOND, Notes relative to the Physiological Effects and Therapeutical value of Picrotoxine *St Louis Clin. Record*, Oct. 1876; RINGER and BURY, The effect of Pilocarpine (the alkaloid of Jaborandi) on two cases of Bi-lateral Sweating, *Practitioner*, Dec. 1875; SALTER, Case of Acute Traumatic Tetanus, Treated Successfully with Chloral Hypodermically injected, *Ibid.*; TAGUET, Note on the Influence of Colored Light in the Treatment of Insanity, *Ann. Med. Psych.*, Nov. 1876.

BOOKS, ETC., RECEIVED.

Principles of Human Physiology: By William B. Carpenter M. D., F. R. S., etc. Edited by Henry Power, M. B. Loud, F. R. C. S. A new American from the eighth Revised and Enlarged English edition, with Notes and Additions by Francis G. Smith, M. D. Philadelphia, 1876. Henry C. Lea, 1083 pages. Chicago: Jansen, McClurg & Co.

Chemistry: General, Medical, and Pharmaceutical, including the Chemistry of the U. S. Pharmacopeia. A Manual of the General Principles of the Science, and their Applications in Medicine and Pharmacy. By John Attfield, Ph. D., F. C. S., Seventh edition, revised from the Sixth (English) edition by the author. Philadelphia, 1876, Henry C. Lea. 668 pages. Chicago: Jansen, McClurg & Co.

Recherches Cliniques et Therapeutiques sur l'Epilepsie and l'Hysterie, Compte-Rendu des observations recueillies a la Salpètriere de 1872, a 1875, par Bourneville, Paris. Bureaux des *Progres Medical.* 200 pages.

Hay-Fever; or Summer Catarrh, Its nature and Treatment. Including the Early Form, or "Rose Cold;" the later form, or "Autumnal Catarrh;" and a Middle Form, or July Cold, hitherto undescribed. Based on Original Researches and observations, and containing statistics and details of several hundred cases, By Geo. M. Beard, A. M. M. D. New York, Harper & Bros., 1876, 266 pages. Chicago: Jansen, McClurg & Co.

The Use and Value of Arsenic in the Treatment of Diseases of the Skin. By L. Duncan Bulkley, A. M., M. D., New York. D. Appleton & Co. 1876. 45 pages.

The Electric Bath, Its Medical Uses, Effects and Appliance. By George M. Schweig, M. D., G. P. Putnam's Sons, New York, 1877, 131 pages. Chicago: Jansen, McClurg & Co.

The Ovulation Theory of Menstruation: Will it stand? By A. Reeves Jackson, A. M., M. D. Reprinted from the Am. Jour. of Obstetrics, Vol. IX, No. IV., Oct. 1876. 34 pages.

Notes on the Burning of Theatres and Public Hall. Reflections on some of the Causes of the Great Mortality occasionally attending such Fires, and Suggestions for Improved Security to Life. The Antiquity of the Drama, and the Opening of Theatres in America, with a Chronological List of theatres and other Public Edifices burned. By J. M. Toner, M. D. 22 pages.

Annual Report of the Northern Hospital for the Insane of the State of Wisconsin. For the Fiscal Year ending September 30, 1876.

Reports of the Board of Directors and of the Physician and Superintendent of the Western Lunatic Asylum of Virginia for the fiscal year 1875–76.

Biennial Report of the Officers of the Vermont Asylum for the Insane, for the two years ending August 1st, 1876.

Twenty-first Annual Report of the Trustees of the State Lunatic Hospital at Northampton. Oct. 1876, Boston, 1877.

Twenty-third Annual Report of The Pennsylvania Training School for Feeble-minded Children, for nine months, ending Sept 30, 1875.

A Series of American Clinical Lectures, edited by E. C. Seguin, M. D. No. 15. Vol. II No. III. The Treatment of mild cases of Melancholia at Home. By E. C. Seguin, M. D. New York, G. P. Putnam's Sons. 1876. 24 pages.

The Medical Libraries of Boston. A Report read at the first Annual Meeting of the Boston Medical Library Association held on October 3d, 1876. By James R. Chadwick, M. D. 11 pages.

Governmental Supervision in the Management of the Insane. By H. B. Wilbur, M. D. 45 pages.

On Masturbation and Hysteria in Young Children. Remarks before the Medical Journal Association of N. Y. in Nov. 1875. Rep. from the Am. Jour. of Obst., Feb. and June 1876.

Transactions of the Colorado State Medical Society. Sixth Annual Convention, held at Denver, June 20, 21, and 22, 1876.

Transactions of the Iowa State Medical Society for the years 1872 to 1877, incl. Published by the Society. Ottumwa, 1876. 224 pages.

The Improved Electro-cautery Battery of C. Herz, M. D., of San Francisco. Extr. from the Western Lancet. Jan, 1875.

Health and Quarantine Regulations for the City and Harbor of San Francisco (Political Code of California). Together with the General Orders of the Board of Supervisors relating to the Public Health.

Ninety-fourth Annual Catalogue of the Medical School Boston) of Harvard University. 1876–77.

THE FOLLOWING FOREIGN PERIODICALS HAVE BEEN RECEIVED SINCE OUR LAST ISSUE.

Allgemeine Zeitschrift fuer Psychiatrie und Psychisch. Gerichtl. Medicin.
Annales Médico-Psychologiques.
Archiv fuer Anatomie, Physiologie, und Wissenschaftl. Medicin.
Archiv fuer Path. Anatomie, Physiologie, und fuer Klin. Medicin.
Archiv fuer die Gesammte Physiologie der Menschen und Thiere.
Archiv fuer Psychiatrie.
Berliner Klinische Wochenschrift.
British Medical Journal.
Bulletin Générale de Thérapeutique.
Centralblatt f. d. Med. Wissenchaften.
Dublin Journal of Medicine and Surgery.
Deutsche Medicinische Wochenschrift.
Edinburgh Medical Journal.
Gazetta Medica de Roma.
Gazette des Hopitaux.
Gazette Médicale de Strasbourg.
Hygiea.
Hospitals Tidende.
Jahresbericht u. d. Leistungen u. Fortschritte in der Gesammt. Medicin.
Journal of Anatomy and Physiology.
Journal de l'Anatomie et de Physiologie, etc.
Journal de Médicine et de Chirurgie Pratiques.
Journal of Mental Science.
La France Médicale.
Lancet.
Le Progrès Médical.
Lo Sperimentale.
L'Union Médicale.
Medicinische Jahrbuecher.
Nordiskt Medicinskt Arkiv.
Norsk Magazin for Lagensvidenselskabens.
Psychiatrisches Centralblatt.
Rivista Clinica di Bologna.
Revista Sperimentale di Freniatria e de Medicina Legale.
Revue des Sciences Médicales.
Revue Scientifique.
Schmidt's Jahrbuecher der In- und Auslandischen Gesammten Medicin.

St. Petersburger Med. Wochenschrift.
The Practitioner.
Upsala Lakareforenings Forehandlinger.
Vierteljahreschrift fuer die Prakt. Heilkunde.
Wiener Klinik.
Wiener Medicinische Press.

The following domestic exchanges have been received:

American Journal of Insanity.
American Journal of Medical Sciences.
American Journal of Obstetrics.
American Journal of Pharmacy.
American Medical Weekly.
American Naturalist.
American Practitioner.
American Psychological Journal.
Atlanta Medical and Surgical Journal.
Boston Medical and Surgical Journal.
Canada Medical Record.
Chicago Medical Journal and Examiner.
Clinic.
Cincinnati Lancet and Observer.
Detroit Review of Medicine and Pharmacy.
Indiana Journal of Medicine.
Medical News and Library.
Medical Record.
Medical and Surgical Reporter.
Nashville Journal of Medicine.
New York Medical Journal.
Peninsular Journal of Medicine.
Pacific Medical and Surgical Journal.
Pharmacist.
Philadelphia Medical Times.
Physician and Surgeon.
Richmond and Louisville Medical Journal.
Sanitarian.
St. Louis Medical and Surgical Journal.
St. Louis Clinical Record.
Virginia Medical Monthly.
W. Virginia Medical Student.

Original Articles, Selections and Translations.

Art. I.—PATHOLOGY AND TREATMENT OF NEU-RALGIA.

By J. S. Jewell, M. D.

NEURALGIA is one of the most common forms of dis-
order to be met with in the experience of the practicing
physician. It has been written upon most extensively at
various periods in the course of the history of medicine, and
never more profusely than in the past few years. But after
all it cannot be said that it is well understood, more particu-
larly as respects its pathology.

In the present paper it will be my purpose to consider this
subject, with some care, and with the hope of placing it in a
clearer light than it has been by most who have written on it
up to the present time. In the endeavor to carry out this
purpose, it will not be possible for me to enter into a history
of the progress of knowledge on this subject.

Neuralgia, as all are aware, signifies nerve pain, and
implies always, at least in my opinion, a material lesion of
some part of the sensory nervous apparatus. But what is its
true nature, and its true seat in the nervous system, and on

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what morbid changes does it depend? What is its real mechanism or make up, and the various steps in the morbid process? It seems to me, that we are now in a position to answer these questions in a somewhat definite manner. But before I begin to describe what I believe to be the true pathology of neuralgias, it is necessary that I should refer to certain points in the anatomy and physiology of the nervous system, as a basis for what is to follow.

The sensory nervous apparatus in its simplest form consists of a conducting tract and a central apparatus of nerve cells, for receiving and appreciating the impressions conveyed to them, by the way of the conducting tracts. This simple statement covers the whole case. So far as present knowledge goes to show, nerve *fibres* perform no other function than that of conducting impressions. They cannot feel them, as it seems may be done by the cells of the sensory nuclei to which they lead. Sever a nerve trunk or fibre in any part of its course, from its appropriate centre, and no degree of irritation will cause the part peripheral to the cut to react to the impression, so as to give us what is known as a sensation. This is not a matter for surprise, for nerve fibres *per se* are not the seats of true nerve sensibility. Simple as this fact may appear to be, it is important for us to remember, and use, in our investigations into the pathology of neuralgias.

In its normal state, as respects its nutrition, a nerve fibre has its maximum of conductibility, or as M. Vulpian would say, *neurility.* It does not seem possible for a nerve fibre to be brought into such a structural state, by disease, as to augment its conductibility, beyond what is natural to it, in a condition of health. But if there are doubts to be entertained, in respect to this point, there can be none, as to whether the conductibility of a nerve fibre may be diminished or even destroyed by disease. According to my view, this must in some degree, be always the result of morbid structural change in a nerve fibre. Any departure from its normal structural state, in which state it is most perfect as a conductor of impressions, in all probability, impairs this function. If this is a fact, as I believe it to be, it has an important bearing on the pathology of neuralgia, as I hope soon to show.

Let us now for a moment give attention to the central sensory apparatus of nerve cells. This is the true seat of nervous sensibility. Any impression, no matter how or under what circumstances produced on a nerve fibre, unless it shall reach this central mechanism of nerve cells, does not give rise to what is called a sensation. The capacity to recognize, or appreciate sense impressions, is found alone, at least in its perfection, in nerve cells. Certain it is, it does not exist in nerve fibres. No form or degree of disease can confer on nerve fibres this capacity, of which they are naturally devoid. If there is doubt as to whether nerve fibres can have their conductibility increased in any way above the normal level, there can be none, as to whether sensory nerve cells may have their sensibility rendered more acute than natural, or increased to an unhealthy degree, under certain circumstances. Neither can there be any doubt as to whether their sensory capacities may not be diminished or blunted by various means, healthy as well as morbid.

If the foregoing remarks are true, a decrease or loss as compared with the normal degree of sensibility in a part, may depend on *first*, a loss in conductibility of the related nerve fibres, without any involvement necessarily of the receptive capacity of the related nerve centre, or it may depend on a change in the condition of the nerve centre itself, without any unhealthiness of the nerve fibre as to its structure and conductibility. But an increase in the sensibility of a part does not find its explanation in the condition of the nerve fibres, but rather on a change in the condition of the sensory nerve cells themselves. The apparent increase in sensibility does not depend on a condition of the *conducting* tract, which might be supposed thus to magnify as it were the sense impression, as it passes along toward the sense centre, so as to exaggerate there, the effect that would otherwise have been produced under normal conditions, but rather on a change in the cells of the centre which heightens their receptive capacity and thus renders them more responsive, more easily disturbed, more unstable. In case then of any augmentation of sensibility above what is normal, we are led to fix attention on the centres rather than on the sensory nerves themselves. And

this as I hope to show, is what must be done in a true pathology of neuralgia.

Excluding for the time being the special senses from our view, we will fix attention on what has been called *general sensibility.* It is found, among other parts, in the cutaneous surface. When we consider the general sensibility of the skin, we find it may be divided into different kinds. We may distinguish at least three kinds. 1. The sense of contact or of touch. 2. The sense of temperature. 3. And possibly the the sense of pain or of painful impressions.

These three forms of sensibility of the skin, and possibly others, may be variously increased or diminished, or even qualitatively changed without involving the same kind or degree of change in the other forms. Thus the sense to painful impressions may be impaired or even abolished, or otherwise changed, and yet the sense of touch or the sense of temperature remain intact. And the same may be said of the other forms of sensibility. It would be easy to give examples of these cases.

Now how can such phenomena be accounted for? How can we have the sense of pain either abolished or increased, without involving at the same time a parallel change in the other forms of sensibility which belong to the skin? Have we separate sensory nerve fibres to conduct the different kinds of impressions? If not, how can we account for the phenomena?

Dr. Brown-Sequard and others, hold, or have held, to the hypothesis, that these different kinds of sense impressions including all different kinds of sense impressions, have different kinds of nerve fibres to conduct them.

The sense of touch has one kind, and the sense of pain others, etc. If one set of these fibres should become diseased, then the form of sensibility to which they are subservient would be impaired or lost, while other forms of sensibility in the same part of the body might remain intact.

But there is another mode of explaining the co-existence of different forms of sensibility in the skin, besides that of supposing they each have different kinds of nerve fibres. It is that the sensory nerve fibres of the skin are of one kind. pathol transmit indifferently and often simultaneously, different

forms of sense impressions, such as those of tact or touch, and pain and temperature.

The physiological analysis of these different impressions, is made at the centre to which the nerve fibres lead. This notion may be illustrated, by a reference to the followng diagram:
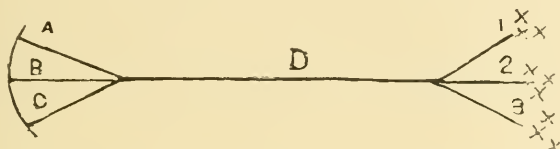
Let A, B and C represent different kinds of cutaneous sensory impressions, produced on the peripheral end or apparatus of a nerve fibre. Let D represent the fibre along which the impressions are conducted. Next, let 1, 2 and 3 represent different groups of nerve cells at the central end of the fibres. In this case it is supposed that the fibre is made to connect, more or less directly, with all these groups of cells. The impressions of any or all forms, which are conducted along the fibre, are transmitted equally to all the centres or different cell groups. But though they all receive the three different kinds of impulses, yet each group is fitted to react to one kind of impression only, and none other. If the group of cells at No. 1 is fitted to react alone to impressions of contact, it will not recognize impressions of temperature, though they really reach it quite as forcibly perhaps as they do the cells of No. 2, which latter are fitted to react to them, but not to impressions of touch. In this way as can be readily seen, may we account for modifications of one form of sensibility without of necessity involving other forms closely related. If some modification should be made in the condition of the group of cells at No. 1, we might witness as a result, increase or decrease, or even a total loss of the sense of touch in the related parts, without the nerve fibre itself participating in any way. Happily we can illustrate this point in a very beautiful manner by a reference to the telephone of Mr. Elisha Gray, of our city. By it he is able, as we know, to communicate several dispatches—as many as eight—simultaneously over the same wire, and by a set of peculiar receivers to deliver them at the

other end. This we know is done by communicating to each current a vibration which corresponds to that of a given musical note, and this vibration the current maintains faithfully to the point of delivery, where it is reproduced by a musical mechanism attuned to respond to it, and to it alone. And so in the transmission and central analysis of sense impressions. These latter phenomena do not depend on the conducting fibre, but on the central apparatus, which, however, the clumsy hypothesis of Dr. Brown-Sequard encourages us to overlook. But I cannot now tarry to give certain other anatomical and physiological reasons, for the belief that the view I have presented is the true one. Though it is not novel as I am well aware, yet it has not been utilized as it must and will be in the future.

Such are a few of the anatomical and physiological points to which I have felt it necessary to call attention, before passing to what are more commonly considered to be questions of pathology proper. It will be seen by what I have said, that in this case I lay very great stress on the central apparatus of sense, as compared with its so-called peripheral portion, or the nerve fibres. But what relations do such considerations sustain to neuralgia? It will be one aim in the remainder of my paper to try and give an answer to this question.

Neuralgia depends on a *material lesion*, however diffuse or slight, of some part of the sensory nervous apparatus. Of the correctness of this statement, I expect to offer proof as we pass along.

The form of general sensibility to which neuralgias in the proper sense of the word must be referred, *as a rule*, is the *sense of pain, or of painful impressions*. It is true that there may be changes in the sense of touch. It may be exalted or diminished. The former is properly called hyperæsthesia, and the latter anæsthesia in the present use of these terms. Then again, the sense of temperature may be increased or diminished. But there are no well recognized terms by which to express these variations in the sense of temperature. But it is hardly in reference to either of these forms of sensibility that the term neuralgia or *nerve pain* is used. It is true that there is a form of neuralgia in which the sense of

temperature may be involved, and hence form a factor.   I now refer more particularly to that form of burning neuralgic disorder, described by Dr. S. Weir Mitchell which he has denominated *causalgia*.   But I do not now undertake to limit the meaning of the term neuralgia.   There may be various forms of disorder of sensibility, both general and special, to be comprehended under the term.   It is not my purpose to inquire into this subject now.   It may be truly said, however that neuralgias as a whole, belong to the sense of pain, or of painful impressions.   They occur mostly within this domain, and hence our attention will be directed chiefly to it.   It may be either exalted or diminished.   If the former we have strictly speaking *hyperalgesia*, not *hyperæsthesia*, if the latter, *analgesia*, not *anæsthesia*.   It is true that the terms anæsthesia and hyperæsthesia might be so used in a generic sense, as to cover all cases of increased or diminished sensibility without violence to their etymological meaning.   But they have been so loosely used and misused in medical writings, as to render a new nomenclature necessary for distinguishing different kinds of disorder of sensibility.

That these remarks are not arbitrary but founded on fact is apparent, when we remember that in many if not most cases of chronic neuralgia, the parts to which the affected nerve is distributed, have in a measure lost their sense of touch, as may be determined by the æsthesiometer.   This is true, especially in many cases of trigeminal neuralgia.   Often the slightest touch on the surface of the face within the region of distribution of the affected nerve, is the cause of excruciating pain.   Nothing is more common than to speak of such cases as being hyperæsthesic, when in point of fact, a more careful investigation shows the existence of positive anæsthesia.   There is however, in all such cases, *hyperalgesia* or a morbid increase of the sensibility to painful impressions, so much so, that impressions not at all painful in the normal condition of the sensory apparatus, produce in it pain.   Now the question next in order is, what is the seat of the final lesion, whatever it may be, which must be one of the chief factors in a neuralgia?   I have already tried to show that augmented sensibility cannot depend solely on any state of the related nerve fibres, but

rather on the nerve centres, which are the real seats of sensi-
bility, at least in its higher forms. Upon this point must we
fix our attention; viz.: upon the condition of those groups of
sensory cells, in which the sensory nerve fibres partly termi-
nate, and which are fitted in a peculiar manner, at present
unknown, to react to painful impressions. Now in what con-
dition may we suppose these cells to be, when we consider the
typical phenomena of neuralgia? It cannot be one of absolute
loss or destruction of their proper structure. All the phenom-
ena of the case forbid such a view. They must preserve their
fundamental structure. The disorder of the cell groups to
which reference is made, is not of an inflammatory nature.
It is invariably, to come at once to the point, *a lesion of
nutrition*, having its essential seat, so far as neuralgia is
concerned, in those cell groups, which have the capacity of
appreciating sense impressions of pain.

No other parts of the body are so bountifully supplied with
blood for purposes of nutrition, as the nervous centres. This
is because of their comparatively extraordinary functional
activity. The lesion of nutrition referred to is simply one in
which the cells have been worn from various causes, in their
intimate structure, beyond the possibility of immediate repair
by the regular process of nutrition, for this is the only way in
which the repair can be effected. In some cases, for reasons
that I will try to give hereafter, the lesion is practically irre-
parable. No organ can act, no cell can act, without a waste
of its substance as a consequence. This process goes on ordi-
narily until it can go no further without endangering the
integrity of the structure involved. At this point, that semi-
painful state sets in, which is called *fatigue*. If it is disre-
garded, and action is continued, the fatigue becomes painful, or
even insupportable. This painful feeling is the natural
inarticulate language, as we may say, of the worn tissue, and
which may be regarded as a warning, that the process of waste
or wear and tear, has been carried as far as it can be without
endangering the fundamental integrity of the tissue. Ordi-
narily, when this point is reached, we obey the demand of
nature, and *rest*. But rest simply means opportunity for
repair of damage done.

When the repair is accomplished, we feel rested, as we say, the fatigue has given way to a sense of comfort, and the instincts and capacity for action begin to be manifested, to be followed sooner or later by a recurrence of that degree of intimate structural impairment, which gives note of its presence by the sense of fatigue. As far as the nervous system is concerned this nutritive lesion may not only take place in the motor tract, but the sensory as well. The action of the central motor apparatus or of the cells which gives rise to the motor impulse, which passes out along the motor nerves, no more certainly involves nutritional waste, than does the action excited in the cells of the sensory apparatuses by the sense impressions reaching them. The sense apparatuses become fatigued by continuous action or excitation, just as truly as the motor. If we rest them as soon as they become fatigued, they are repaired, and a sense of well-being comes as a result. But if for any reason the sensory tract, or any parts of the same, are over-excited by stimulation too intense, or too long continued, the process of nutritional impairment goes beyond the ordinary limit, and a painful or irritable condition sets in, as a testimony to overwear or excessive waste. The damage done is too profound to admit of repair in the time ordinarily allotted, or in any given case actually allotted for rest, or in other words, for nutritional repair. Hence the succeeding period of action or stimulation is begun, before the repair is complete. And this condition of things, if it goes on, as it often does go on, adds day by day to the preponderance of waste over repair, and hence to a permanent sense of fatigue, or in case of the sensory tract to irritability and pain, until a thorough loss of balance is sustained, which nothing short of prolonged rest and the best nutritional supply, and possibly the use of special means to quicken the processes of nutrition or, in other words, tonics, and the incidental aid of nerve sedatives, etc., can repair or restore.

In this worn and irritable state, it seems to me it is easy to see that any renewal of action or excitation will only increase the mischief and give rise to unpleasant symptoms, generally pain.

This process of gnawing, by nutritional waste, farther into

the already worn and irritable nerve cells, as must happen, if action in them is renewed, provokes pain. This morbid process as it becomes, does not lead to the destruction of the apparatus, simply to its impairment as I have endeavored to describe. It is rendered irritable, hyper-sensitive to the impressions made on it or conveyed to it, by the nerve fibres,—it is unstable, too responsive, easily thrown into disturbance, difficult for it to suspend its activities when they are once aroused. But in whatever way it is produced, this is the state in which a nervous centre is, when it is in the "*neuralgic condition.*" I shall give ample though indirect proof of the correctness of this view hereafter.

There is no inflammation, no absolute dissolution of parts, simply the worn, irritable, hypersensitive condition described, not of the nerve fibres, (though they are often diseased in such cases) but of the sensory nerve cells of the central sensory tract, which is the sole seat of true nervous sensibility, as that term is ordinarily understood. This is *the one ever-present, essential factor, in all forms of neuralgia, whether general or local.* It is the background, the undertone in the picture, wherever seen.

Now in what ways may the condition of the sensory tract which we are contemplating, be produced? There are three principal modes of producing it, all of which may be present or not in any given case. The nutritive lesion about which I have spoken may be produced, first, by too much action or over-excitation, or second, by a deficient supply of nutritive materials, either with or without too much action, or third, it may be, as it often is hereditary, being recognized under the head of the "neuralgic temperament," or of "hereditary neuralgia." I will now consider these causal conditions in relation to the nutritive lesion which I have tried to describe as the fundamental element of neuralgia, irrespective of its form or locality. Then : 1. Those cases arising from peripheral over-excitation or irritation. These cases are divisible into two classes for practical purposes. *a.* Those in which there is no recognizable hereditary tendency to neuralgia, nor any marked deficiency as regards general nutrition, and in which the neuralgic condition of the sensory tract is produced

chiefly by the severity and duration of the over-action or
excitation, which sooner or later leads, in spite of the naturally
healthy condition of the parts to that excessive tissue waste in
which pain sets in, on comparatively slight provocation.
b. Those cases in which the excitation or action may be very
moderate, as measured by ordinary standards, but in which,
either by reason of hereditary instability of the nervous
structure, or by reason of a deficiency in the supply of nutri-
tive materials or both conditions combined, as often happens,
the same nutritive lesion; viz., an unhealthy preponderance
of waste over repair is produced. For practical purposes only,
these two classes deserve to be distinguished from each other,
for at bottom they are the same. But, I wish now to fix atten-
tion for the moment, solely on the fact of peripheral irritation.
If a sensory nerve fibre is healthy, every impression made on
it is instantly transmitted to the centre to which it leads, and
if the latter is in condition to receive the impulse, a more or
less decided impression or disturbance is produced there. If
the parts are healthy, and severe mechanical or other violence
is done to the peripheral nerves, by which they are suddenly
injured, the impression conveyed to the related centre, gives
rise instantly to pain. But the pain is not necessarily to be
explained in such a case by a reference to an abnormal condi-
tion of the centre, but on the simple ground of the peripheral
lesion, and hence by the severity of the impression made on
the centre which may have been at the time perfectly healthy.
We attribute the pain solely to the visible external injury, as
in cuts, lacerations, burns, etc. Such pain is not called neu-
ralgic, though it is so in one sense, for it is nerve pain arising
from nerve injury. But when very severe nerve pain *seems* to
arise spontaneously, or is aroused by some slight touch or im-
pression, never painful to a healthy organism, then it is called
neuralgic. As I have said in substance, already, this condition
of things cannot be explained except by going to the related
nervous centre for the key of the explanation. But the condi-
tion itself in which the centre is found in such cases, cannot be
understood except we take into account the action upon the
centre of the sensory nerves which terminate in it. They are
the natural channels through which the centres are excited to
action.

If the impressions made on a given peripheral nerve, say the trigeminus, are severe, or, whether severe or not, are long continued and frequently repeated, the nervous centre becomes sooner or later in that state, as respects its nutrition, that I have already described, worn, sensitive, and ready to react with what we call pain, even to slight impressions reaching it by way of its nerves, and indeed in other ways as well, as I will endeavor to show hereafter. The requisite degree of irritation of a sensory nerve, may be produced in many ways, but generally by irritative disease of some kind. Generally the disease of the peripheral nerves arises, because they are involved in the structural diseases of the organs and tissues to which they are intimately distributed. Thus it happens to the trigeminus, in diseases of the teeth and jaws, which involve twigs or large branches of the trigeminus, and in diseases of the antrum, orbit, nasal fossae, in rheumatic, gouty, syphilitic, and other affections of the superficial and deep parts of the face, in injuries of the face and closely related parts, in severe exposures to cold, etc. In all these ways, and many others, may irritative disease be produced in the peripheral portions of the trigeminus, which more or less steadily transmits to the trigeminal sensory nucleus, that degree of irritating or exciting influence, which will, sooner or later, in different cases, cause that degree of nutritional disturbance, which I have called to your attention, as the fundamental factor in neuralgia. The nervous centres may be as easily worn, and with as bad results, if not worse, this way, as by over-action in a natural way. The same kind of action on particular sections of the sensory tract of the spinal cord, may be brought to pass, by disease of the mucous membrane of the stomach, or of the mucous membrane of any part of the alimentary canal, especially that of the lower part of the colon and rectum, and in various ways, as by dysentery, acute or chronic, especially the latter, by the habitual occupation of the colon, by hard and irritating fœcal masses, as happens in obstinate constipation, or in case of irritative disease of the kidneys, and more particularly of the bladder, as in chronic cystitis, prostatic disease, abnormal genital excitation, sexual excesses, irritative uterine and ovarian diseases of many kinds, disease of the

liver, of the lungs, etc., etc. In all these cases, and many others, the visceral irritative disease almost necessarily affects the sensory nerves which ramify in the organs, and from these diseased nerves, a more or less steady tide of irritative and wearing, nervous impressions, is transmitted practically without cessation to certain parts of the sensory tract, to which the sensory nerves from any given part may go, and as a result, sooner or later, the central sensory apparatus of cells is brought into that worn, painful, irritable state, which I have described above. Every observing physician can recall from his own experience cases of this kind. We frequently find irritable and tender spines in the lumbar or lower dorsal regions, depending on irritative pelvic disease. Under such circumstances, the spine may be tender to the touch, with neuralgic pains radiating from the seat of irritation in the cord, downwards, or in other directions, differing in different cases? We also often find neuralgic pain come and go in association with gastric disease, either in the upper dorsal spine, or under one or both scapulæ, or around the course of certain of the intercostal nerves, or extending down the course of the brachial plexus, or its branches.

In all these cases the disturbed state of the nutrition of the sensory tract, or some definite part of it, is produced by excessive stimulation through the channel of the diseased nerves. The effects in the end are similar to those produced by over-action in a normal way. This state may be produced not alone by over-stimulation of the sensory nerves, or by irritative diseases affecting them, but by too long-continued, or too severe muscular exertion. Over-use of the arms in a variety of manual employments, over-use of the lower limbs, as in protracted standing or walking, or the use of the sewing machine, overstrain of the muscles of the back of the neck, as happens in the case of those whose occupations make it necessary for them habitually to bend the head forward, so as to put the muscles in question on a more or less continuous strain, etc., may bring about a similar condition, indirectly of the sensory tract. These protracted muscular efforts cannot be made without over-use of some part of the cord, and hence without involving the same nutritive

lesion of the motor and sensory tracts combined, that I have
already referred to. Nothing but rest, or in other words,
repair will truly relieve such cases. Action must cease that
waste may cease, and that repair may go on without dis-
turbance.

But the nutritional lesion, about which I am speaking, may
be produced in other ways, as well as those already described.

One of the most prolific sources of the neuralgic condition,
is defective nutritive supply. But few conditions are more
common than this. How often do we find cases, in which by
reason of privation of food, or disease of the digestive system,
which impairs the appetite, or if not so, impairs digestion and
hence disturbs the digestive process and vitiates its results, or
where by choice or habit, unsuitable or unnutritious food is
taken, and hence, in one way or another from the side of supply,
anæmia is produced? Then, again, how often do we find cases
in which though digestion is good, and the supply of food
fair, yet by reason of some excessive discharge as by hemor-
rhage, impoverishment of the blood occurs leading to that
worn, irritable condition of the nervous system as regards its
nutrition, which is the prolific soil out of which the number-
less forms of neuralgia may spring. Under such circum-
stances a local neuralgia is often produced by very slight, local,
irritative action, or over-action, for the essential background is in
such a case gratuitously supplied. It takes but a comparatively
short period in time to produce that irritable hyper-sensitive
condition in the sensory tract, which is found at the bottom of
every neuralgia proper. And when the neuralgia is once
under way, in such a case, there is but little prospect for a
recovery, while the plain conditions on which it depends are
unrecognized or remain the same. In every case of neuralgia,
the condition of nutrition of the patient should be made the
subject of the sharpest inquisition.

One of the commonest forms of unrest, and hence of exces-
sive waste, takes place from loss of sleep. This is at bottom
not anywise different from the form of nerve wear and tear,
already described. It simply deserves a mention for practical
purposes. Sleep is brain rest, or it is nothing. If so, then
lack of sleep entails, of course, that preponderance of waste

over repair, and hence the neuralgic state with which we are
already familiar.   In this way do many if not most painful
or neuralgic conditions of the brain partly arise.   The pain
growing out of this impaired nutritional state of the brain,
may be general or local, slight or severe, paroxysmal or con-
tinuous, as I have often seen.   It is not even necesary that we
should have pain physiologically speaking.   It may be what
has been called "psychical pain."   It may be a painful
emotional state, such as we see in distressing forms of melan-
cholia, a kind of pain, if it can be called so, sometimes harder
to bear than mere physical pain.   I am much inclined to
agree with Krafft-Ebing, who calls melancholia a " psychical
neuralgia."   In my opinion it arises out of the same condi-
tion exactly as regards brain nutrition,—say of the cerebral
cortex,—as that which I have described as the fundamental
organic basis of neuralgia.   Essentially the same causes pro-
duce both, and they are relieved by similar means for cure.
But I cannot speak on this interesting topic to the extent it
deserves.   But one of the chief causes of the impaired nutri-
tion of brain, on which the neuralgias, more particularly of
the brain depend, is the loss of sleep.   I know very well this
is not new, and I also know it is seldom practically heeded
and thought about, as it certainly deserves to be.

Then let me once again, recall to your notice, that the
unstable condition of nervous system, as regards its nutrition,
its solidity and perfection of structure, may be hereditary,
and if so, is a serious, even an irremediable matter.   It is
necessary above all else, to recognize this factor, in the respect
to prognosis.   It will save you much trouble and confusion,
for it will often prevent you from making promises, or at least
from exciting hopes for recovery, destined, as might be seen
before hand, not to be fulfilled.   It will be of value also, to
recognize the hereditary factor in pointing out specific
hygienic courses to our patients.   They cannot without risk,
do or endure, what others may with impunity.

But one of the more prolific immediate causes of neuralgic
attacks, is fluctuation in blood supply.   There are two ways
in which these fluctuations may operate.   If there is too little
or greatly too much blood supplied to a nervous centre, in a

given time, there must be some change in its nutrition, generally a diminution to a point below the normal rate, and if so, a tendency toward that condition of nutritional impairment, which has been described as the most essential pathological factor in neuralgia.

Not to speak at present of congestions, and the nutritive disorders to which they may be supposed to lead, I would call your attention for a few moments to the opposite condition—anæmia. It has been supposed by some writers, that we may and do have a condition of the circulation of even quite limited tracts of the cord or brain, in which through excitation of the related vaso-motor nerves, the vessels are made to contract to much less than their normal size, and hence, in this way, to exclude a portion of the blood, which they should normally convey to the part to which they lead. It is held by those, who are of this belief, that this condition of vasal spasm may endure for even months in extreme cases, without any considerable periods of relaxation. Now if this condition does actually occur, and I believe it is possible, it can be seen by you all, how it might naturally lead to impaired nerve nutrition, and hence to the neuralgic condition. This condition of the circulation, it is maintained by my friend, Dr. Hammond of New York, occurs in the so-called spinal irritation, to which I have referred incidentally, already. He supposes that in spinal irritation, there is anæmia of the vessels of the *posterior columns* of the spinal cord, produced by vasal spasm. If this actually happens, it would lead to the neuralgic condition I have described. It is not my purpose this evening to discuss the view of Dr. Hammond, to which I have alluded, but I will remark in passing, that while I think the circulatory anomaly *possible*, yet I doubt its existence in such cases. There is no real proof of the correctness of the hypothesis. It is simply inferred from a group of phenomena, which may be explained in a better way, so it seems to me; viz., by reference to a nutritional lesion, to explain which, such a hypothesis is not necessary. But admitting that such circulatory disorders may and do occur, it is easy to see how they might conduce to bring about the "neuralgic condition." But it is not so much to changes *in the rate of nutrition*, brought about by quanti-

tative changes in blood supply, to which I wish to direct your attention this evening, but rather to *changes in blood pressure within the cranio-spinal cavity and the results of the same.*

I know very well what are the opinions which have been entertained in relation to the question, as to whether the quantity of blood within the cranio-spinal cavity is, or may be subject to much variation. I know it has been held by some, that being air-tight unyielding cavities, into which the blood is forced by atmospheric pressure, on principles well understood, in other relations, there could not be any appreciable variation in quantity of blood at one time, as compared with another. If you have more blood in any given part of the brain at one time, it must be for the reason that some other part or parts have in proportion less than they should have. This is one position, and it is true, as I believe, to a degree, which does not obtain for other organs not situated similarly. I know also that the contrary opinion has been and is now maintained. It is also partly true, according to my view. But for my present purpose, this question may be put out of sight altogether.

If it is doubtful whether there can be more blood in the brain, as a whole, at one time than at another, there can be no doubt, so it seems to me, as to whether there may be more *pressure*—vascular pressure—at one time, than at another in the cranio-spinal cavity. It may, and in point of fact, does vary, greatly. There can be no question in my own mind after a pretty careful study of the subject, that the situation of the brain in an airtight unyielding cavity, insures a steadiness and fullness of blood-supply, not possible under different physical conditions. This is necessary in the case of organs, of such high vascularity, and having such delicate and important functions. But for various reasons, not only the quantity, but the pressure of the blood in or into the brain and cord, may vary. Vascular pressure within the cranio-spinal cavity, may be changed, either in one or the other of the following,— among other ways :

1. By changes in vascular tonus, affecting the energy of the heart's action. The action of the brain and cord may be deeply affected by a loss of energy of the heart, by which vas-

cular tension within the cranial cavity is diminished, giving at
times,—that is if the brain is in that condition as respects its
nutrition, which I have called neuralgic,—those severe neural-
gic headaches of anæmia, which are aggravated by the upright,
and are made better by the recumbent, posture, and by the use
of certain remedies, amongst them such as will excite the heart
to stronger action, and will hence increase vascular tension,
especially within the cord and brain.   In such cases, alcoholics,
quinia, the preparations of opium, &c., are found beneficial.
But if the brain should be in the worn, neuralgic condition I
have supposed, any condition which would increase the force of
the heart's action, and hence suddenly increase vascular tension,
will often cause pain in the head.   This is seen in the effect of
strain in severe physical effort, which increases venous ten-
sion, and also in any rapid exercise, which temporarily in-
creases the force and frequency of cardiac action, and in the
posture of stooping in such way as to cause blood to flow more
freely to the head, so as to increase blood pressure, and finally
in the throbbing character of such pains, the throbs correspond-
ing to the heart beats, and further in the relief produced by
the means calculated to diminish the force of cardiac action,
or in any other way by diminishing vascular tension, or con-
tributing to a quiet and steady circulation.

Then, again, vascular tension within the cranio-spinal cav-
ity may be produced by means of certain climatic changes,
more particularly the following: In the first place, by changes
in temperature.   Either extremes of heat or cold, if they are
suddenly brought to pass, may change blood tension in the
cranio-spinal cavity.   If the surface is suddenly exposed to
cold, the skin and its small vessels contract, and as a conse-
quence, less blood circulates in the surface, than is necessary
to preserve an equilibrium, as between superficial and deep-
lying parts, and hence more blood circulates in the visceral
cavities, or tends to do so, than under different circum-
stances.   Or, on the contrary, if the surface is more or less
suddenly exposed to a high temperature, the cutaneous vessels
dilate and admit more blood to the exterior of the body, and
hence less relatively goes to the interior, thus giving rise fre-
quently, though not necessarily, to diminished vascular ten-

sion, in the cranio-spinal cavity. Under such circumstances, it is very common for a person to complain of temporary loss of nerve power, or languor, and other symptoms which can hardly be ascribed to mere changes in nutrition, but in part, at least, to changes in pressure. Under such circumstances, neuralgias, if the neuralgic condition exists, are very likely to set in on slight provocation.

In the second place, changes in barometic pressure, give rise, under the peculiar circumstances of the circulation in the cranio-spinal cavity, to changes in vascular pressure. This may be seen in extreme forms in the effects of high altitudes on the nervous system, and in the effects of increased atmospheric pressure, a good example of which is seen in the case of workmen operating in the condensed air of the caissons beneath the piers of certain bridges. The atmospheric pressure on the surface varies greatly from time to time, as is well known, and certainly leads to changes in vascular pressure within the airtight, unyielding cavity, in which brain and cord lie. This, as I believe, from pretty close observation, is *one* of the prime exciting causes of the nervous symptoms,—and neuralgia is chief among them,—which seem to depend on the " weather." This view will also aid in explaining why steady, mild climates, are, as a rule, the most favorable to the removal of those nervous symptoms, which appear to be influenced by climatic changes. Within the past year, I have seen some unpublished charts, drawn up by Dr. S. Weir Mitchell, of Philadelphia, in which he had made the endeavor to trace, by means of lines which resembled isothermal and magnetic curves, the relations of neuralgia to certain climatic changes. I do not know what conclusions he may have reached, but it is a subject to which I have given considerable attention, and I am convinced that much of the influence exerted by climatic changes on certain nervous affections depends on variations in vascular pressure, produced by temperature and barometric changes operating in the way I have just described. I am far from thinking that these or similar exciting causes, can produce neuralgias in a healthy nervous system, but that they may do so, in cases where the "neuralgic condition" already exists, as it so often does.

But without exhausting these practical considerations as to the causes or conditions of neuralgia and their modes of operation, I will call your attention finally to what I consider a most important means for producing changes in vascular pressure in parts of the sensory tract. I now refer to *disordered vaso-motor action.* The muscular vessels, notably the small arteries, just previous to their termination in the capillaries, are abundantly supplied by vaso-motor nerves, which are distributed to the muscular tissue in their walls. This muscular tissue is under the control of the vaso-motor nerves in question, just as truly as the voluntary muscles are under the control of the ordinary motor nerves distributed to them. The vaso-motor nerves in a certain sense—but in a certain sense only—arise from vaso-motor centers, situate somewhere in the gray matter of the spinal cord and medulla oblongata. These centers are taken as a whole in close relations with the sensory tract of the cord and medulla, and what may be called the "emotive" tract of the cortex of the cerebrum. Excitation of the sensory or "emotive" tracts, in a reflex way, excite the vaso-motor tract in the cord and medulla, just as truly as the motor tract proper may be, from which the motor roots of the spinal nerves proceed. In this way; viz., by reflex action, any part of the vaso-motor tract in the cord and medulla may be excited by means of influences reaching it by way of the sensory tract proper of the cord and medulla, or from the cortex, as a result of emotional or other excitement. Excitation of the central vaso-motor tract, or any part of it, may and does give rise to an outgoing motor impulse, along the related vaso-motor nerves leading to some definite locality, some definite vascular area, which may be exceedingly small, or on the contrary, very large. This vaso-motor impulse aroused in a reflex way, passes as just said, along the vaso-motor nerves which are distributed to a certain vessel, or its branches, and suddenly they dilate or contract as the case may be, and in this way is blood-pressure suddenly, even violently, changed, in some particular locality—external or internal—of the body. Almost innumerable examples can be given. A familiar instance of this kind of action was shown a long time ago, by Brown-Sequard and Tholo-

zan.   It consisted in dipping one hand in ice cold water,
and maintaining it there for a time, while the other grasped
a thermometer.   In a short time a very considerable fall in
temperature of the hand not in the iced-water, was indicated.
By repeated test experiments, it was proved that the fall in
temperature of the one hand was due to the fact of the severe
chilling of the other.

The explanation can be readily given in view of what has
been said as to the mechanism and mode of action of the vaso-
motor system.   The severe impression made on the sensory
nerves of the hand in the cold water, was transmitted to the
upper dorsal and lower cervical cord, and transferred to that
portion of the central vaso-motor tract situated there, and
in a reflex way an impulse was sent along down the vaso-motor
nerves of the other arm, which caused sudden contraction of
the muscular vessels, to which these nerves are distributed,
and as a consequence, the supply of blood to that arm was
suddenly diminished, this caused, very naturally a partial
cessation in nutritive activity, and hence finally a diminution
in the amount of heat produced, the production of which de-
pends on tissue change, and this is the explanation of the fall
of temperature in the arm.   But the point to which I wish to
direct your attention, is not the fall of temperature but rather
to this mode of suddenly changing the vascularity, and hence
the blood-pressure of a part, more especially the sensory tract
of the central nervous system.

There is one point in connection with this subject to which
I desire to call your especial attention.   It is that changes in
vascularity in the brain and cord produced by the channel of
the vaso-motor nerves, may be, and frequently are in the
strictest sense of the word local.   Very small portions or
regions of the brain or cord, may, by the means now under
consideration, become the seat of an intense congestion with-
out surrounding parts being to a similar degree, or at all af-
fected.   These " regional" fluctuation in blood supply are of
very great importance in the physiology and pathology of the
nervous system.

I have good reasons for thinking they are one of the chief
factors, especially in typical or periodical, and occasional

neuralgias, such as are due to malarial influence, in chronic
hemicrania, and other particular forms. In hemicrania the
attacks occur once a week, or once a month, or at other
intervals. I am fully persuaded in the first place that the
sensory nucleus of the trigeminus, in part or altogether,
on one or both sides, is the seat of that form of nutritive
lesion which I have described as the most essential factor
in a neuralgia, and that at the time of the attack it be-
comes the seat of a sudden local change in vascularity, and
hence of blood-pressure, which produces that degree of physi-
cal shock in the painful irritable center, which is the immedi-
ate cause of the pain. This may and does occur often, without
the slightest sign of peripheral disturbance as for example in
hereditary cases. The attacks often come on, and pass off,
too suddenly, to enable us to explain them in any other way,
so it seems to me, than by a reference to a local disturbance of
the circulation. In making the statement that attacks of hem-
icrania often come on which cannot be referred to any adequate
peripheral disturbance, I do not forget the opinions which
have been emitted by writers like Du Bois Reymond, Moellen-
dorf and others. The former in his studies of hemicrania as
it occurred, unhappily in his own person, found that the region
to which the pain was limited, (the first division of the fifth
pair on one side) was pale and cold as compared with the op-
posite side, and the temporal artery which supplied that
region, was not only smaller but harder under the finger than
natural.

His opinion was, that the pain was caused by spasm,
of the branches of the temporal artery, produced by irritation
of its vaso-motor nerves. The firm contraction of the muscular
tissue of the vessel, it was supposed, compressed or pinched the
sensory filaments of the trigeminus, and so produced the pain.
Du Bois Reymond seems to have been almost solely occupied
with a consideration of the condition of the *peripheral* termina-
tion of the nerve supplying the affected region, rather than with
the condition of the center, which, in my opinion, is in all such
cases the chief seat of disease. As proofs that his view was incor-
rect, it may be remarked that soon after, Moellendorf described
a form of hemicrania in which the opposite condition of the

vessels in the painful region is found, and also that cases
occur in which there is no appreciable external vasal disturb-
ance, and that it often happens that quite as marked vaso-
motor disturbances occur in the same region without produc-
ing pain at all. These and other reasons seem to disprove the
correctness of that view, which ascribes the pain to peripheral
vaso-motor disturbance. No doubt when the center back to
which the nerves from the affected region lead, is in the pain-
ful condition I have described, earlier in this paper, any periph-
eral disturbance, however slight, may excite pain. But this
should not mislead us, as it has so often done, so as to attract
our attention from the center, which is from first to last the
most essential seat of disease. This being so, we are not left
in a state of surprise at the continuance of a neuralgia, after
the removal of the peripheral irritation, in which ·it in part
had its beginning, even after section of the nerves which are
the apparent seat of the neuralgia. The reason why such means
are sometimes not effectual, is because the real seat of the es-
sential disease is not where it is commonly supposed to be;
viz., *in the peripheral nerves* of the affected region, *but in
the center.*

One means among others for affecting painfully these dis-
eased parts of the sensory tract, no matter where they are, is
sudden and violent changes in blood pressure, and one important
way for producing this state is by disordered local vaso-motor
action, brought about in a reflex way. The starting point of
the irritative action, may be in any part of the peripheral ner-
vous system, at the skin or mucous membrane of the stomach,
or of other sections of the alimentary canal, the uterus or ovaries,
or, on the other hand, from the cortex of the brain, by means
of emotional excitement, for the cortex is, in my judgment,
the seat of the emotions proper. But from either or both
these directions, and in a multitude of ways, may the exciting
or irritative agencies act which may give rise to the impres-
sions which, when they are conveyed into the sensory tract,
may be transferred to closely related vaso-motor centers, from
which, in turn an impulse may be emitted along the out-going
vaso-motor nerves, which produces in the way already de-
scribed the changes in vascular pressure, which, probably, in a

mechanical way, gives rise to the central pain, which is referred to the periphery of the related nerves in accordance with the "law of eccentric projection" of Romberg.

But here I must terminate my summary of conditions and causes of neuralgia, without completing it. I have said enough to indicate somewhat clearly, I hope, my views as to the pathology of neuralgia.

The subject is becoming more important every year, among civilized peoples, for the tendency seems to be, that painful affections of the nervous system increase as civilization becomes more refined and artificial, and the occupations of the race more intellectual and emotional, and the domain of sensual enjoyment and excess, more extended.

From the foregoing discussion, we may formulate perhaps usefully the following general results, or conclusions:

1. Neuralgia has its essential seat in the central apparatus of sensory cells, called the sensory tract, lying within the confines of the gray matter of the cerebro-spinal axis. Its essential seat is not in the peripheral nerves. Hence, it is incorrect and misleading, to speak of, or describe, neuralgias as "diseases of the peripheral nervous system."

2. The essential morbid condition in a neuralgia, is a nutritive lesion of the central sensory apparatus of cells, which are the seats of true nervous sensibility. This state is frequently caused by disease of the peripheral nerves, but even in such cases the more irritable state of the sensory tract is the main factor reached by judicious analysis of the phenomena of neuralgia. In this condition it reacts with pain to even trivial impressions made on the sensory nerves, which terminate in the affected region.

3. The attack of pain may be due to overexcitation, and hence over-wear and waste of the affected center, produced by the channel of its sensory nerves, or by changes in blood-pressure in the affected center, caused by loss or increase of tonus of the peripheral vessels, or a change in cardiac action, or by changes of posture, or of temperature, or of barometrical pressure, or by influences acting on the vaso-motor nerves, distributed to the diseased center, and which may be affected from either the peripheral nervous system, or from the cortex cerebri.

Many proofs could be given of the substantial correctness of the views I have set forth, besides those already offered, but I do not have time to give them in this paper, already beyond ordinary length.

Passing by the proofs referred to, I wish to call your attention for a few moments, in conclusion, to the treatment of neuralgia. It is one of the highest tests of the truth of a pathological doctrine, when it points, as it were, *a priori,* clearly to the true line of remedial action to be adopted. Judged by this standard, the pathology of neuralgia, as I have endeavored to expound it, must take a higher rank than that of a mere hypothesis, to account for the phenomena of the case. If the above described doctrine, as regards the intimate nature of neuralgia, is true, it points naturally, and inevitably, to three prime indications for treatment. They are as follows:

1. *Rest, or cessation of action or excitation.* This is plainly required by the supposed worn, or wasted, irritable condition of the sensory tract, or some limited part of it. It is one of the clear dictates of the case. In point of fact, nothing is more important in the treatment of neuralgia than the fulfillment of this indication. It is not appreciated as it should be in the profession. The necessary quiet of the diseased center may be secured, in many ways, according to the source and kind of irritation or excitation. It prescribes that all irritating substances or diseases shall be removed at once whenever possible, even if there must be division of the sensory nerve trunks, of the affected region, to cut off from the diseased center, all peripheral excitation. It prescribes that all central disturbance, such as arises from changes of blood pressure and other causes in the diseased sensory tract, shall be remedied.

2. It prescribes that inasmuch as there is too much waste, in the sensory apparatus, as the prime condition of the neuralgia, that every means should be employed: (*a*) to supply the greatest amount of nutritive material of the best kind for the purpose of repairing the supposed nutritive lesion; (*b*) that everything which may be faulty in the action of the organs, or in the processes on which nutrition depends, must be corrected, especially by the use of alteratives, and of

tonics which are best calculated to facilitate or hasten the
nutritive processes, and, in this indirect way, to increase
strength.

3.   Lastly, it prescribes that some means shall be employed
as occasion arises, to allay the abnormal nervous excitability,
or the pain, for the sake of immediate relief, or to give that
tranquility, so much needed, and which it is often impossible
to obtain, except by artificial means.

Now, these are the prime indications clearly dictated by the
pathological conditions exposed, and it so happens that they
are exactly those gradually sanctioned by ages of the best
practical experience in dealing with neuralgia, prior to any
correct notions as to its pathology.   But with this outline of
views, I must close my somewhat hasty paper.   You will ob-
serve that no claim of novelty is made in behalf of the views
presented.   They are presented simply because they are sug-
gestive, and believed to have, if thought on, greater practical
value than they have been supposed to possess.

Art. II.—PATHOLOGY AND TREATMENT OF RE-FLEX MOTOR SYMPTOMS—PARALYSIS, CONTRACTIONS, ETC.

A Discourse Before the County Medical Society of New York, Jan. 8th, 1877.

By Eugene Dupuy, M. D.

(Member of the American Neurological Association; Corresponding Member, Paris
Societe de Biologie. etc., etc.)

Paralyses from destruction of conductors only take place in
the spinal cord.   Yet even when conductors are diseased or
destroyed in that organ, the palsy which is observed is mostly
not due to the loss of function of the parts destroyed, but to
an influence exercised by the diseased portion upon other areas
of the cord.   Provided a very few fibres are left healthy, the

functions are accomplished. Paralysis depending upon des-
struction of the grey matter is very rare—only very little grey
matter being necessary to perform all functions—not so, how-
ever, if considered as a consequence of alteration of the grey
matter, which diseased grey matter exercises a paralytic in-
fluence upon other areas of the same tissue. Both conductors
and cells can be destroyed to a very great extent, and yet no
appreciable trouble in motion is detected. The nature of the
lesion has little, if anything, in it, to differentiate kinds of
reflex paralysis.

Reflex paralyses, therefore, are all the kinds of palsies which
can be explained by the foregoing definition.

I consider all paralyses, in a word, to be reflex; some are due
to inhibition, others to alteration of the blood vessels (func-
tional), but reflex, nevertheless. In a word, I take it, that
when there is a patch of sclerosis, or a small tumor, or a piece
of bone protruding, and paralysis is observed, that symptom is to
be ascribed to the irritative, inhibitory, or vascular irritation,
to the influence exerted by that diseased portion of the cord
upon other parts of the same.

For contractions, the same holds good, the difference
between paralysis and contraction, appearing to me not to be
due to a difference in the kind of irritation at work, but to the
degree and to the idiosyncrasies of the patient, as there are
numerous cases to prove that pressure by a protruding piece of
bone, for example, in the same locality of the cord, and damag-
ing it to the same extent, will cause in one case a palsy, in
another a contraction, and oftentimes the one passes into the
other, but more often the contraction becomes a palsy.

Some seventeen or eighteen years ago, Dr. Brown-Séquard
undertook to give an explanation of these reflex palsies, based
upon physiological results, obtained by experiments; he says,
speaking of paraplegia, " that it is due to an excitation that
has come to the spinal cord from a sensitive nerve. The
excitation after having reached this nervous centre, may be re-
flected on the blood vessels of this very centre, or on those of
the motor nerves or the muscles."

But since that time, not only has the theory of reflex
paralysis given by Brown-Séquard, viz., contraction of spinal

blood vessels, been questioned and criticised, but the existence even of such a kind of paralysis has been denied. Strange contradiction! for nobody ever doubted the existence of reflex contractions. I confess that Brown-Séquard's theory does not explain all the cases of palsy which he himself has recorded, but I believe we are not warranted to deny the existence of the disease. I will leave out older cases. It is the fashion now to reject cases recorded only a few years ago; let us comply.

First, I shall state that I include paralysis arising from brain disease, as well as from spinal disease, kidney disease, lung disease, etc., as reflex.

My learned friend, Dr. R. Lepine, of Paris, has recorded several cases of hemiplegia, occuring in persons suffering from pneumonia. At the autopsy there was no brain lesion to account for the motor symptoms.

Prof. Lendet has reported cases of empyema, in which, after the performance of the operation, for the withdrawal of pus, every time that a solution of carbolic acid was injected, paralysis of the arm on the corresponding side ensued.

Dr. Nicaise, one of the hospital surgeons of Paris, has reported two cases of strangulated hernia in the female, in which cases there was hemiplegia, complete, which, in one case, disappeared immediately after the operation of kelotomy and in the other case shortly after. In this last, subsequent autopsy disclosed no brain lesion.

Dr. Luys, of Paris, well-known for his ingenious but imaginative views on the physiology of the brain, has reported a case of aphasia in which the faculty of speech returned of a sudden. The Doctor has shown the brain of the patient to the Sociéte de Biologie. It was an example, he stated, of hypertrophy of the right third frontal convolution, consequent upon destruction of the left one, because it took up the function of the left one. This view, of course, is not tenable, because if it were as Dr. Luys states, the aphasia would have disappeared by degrees, and not all at once.

Dr. Proust has recorded another case of aphasia, in which there was a splinter of bone pressing on the dura-mater, far from the would-be seat of the faculty of speech, which, on being removed, allowed the patient to recover immediately,

before the dressing of the wound had been completed. There
are very numerous cases of the nature of those quoted above;
but these have been reported by their authors very lately, and
since the new ideas about localization of functions has come to
light, and are, therefore, not thrown out by the law of fashion
which will reject all old cases, although well observed and re-
corded. There are numerous cases of lesions of cutaneous
nerves which have caused contractions, more or less perma-
nent. A very localized patch of meningitis in some instances,
worms in the intestines in others. Prof. Verneuil and Dr. Berger
have lately reported, *de novo*, numerous cases of contractures of
one or both wrists, consequent upon hernia. Cases of pressure on
the dura-mater, spinal or cranial, with contractions following,
are very numerous. But it appears that irritations of the
nerve-endings are more prolific causes of contractions. I have
known a case of lock-jaw which lasted for two days, and
which depended solely upon this, that a piece of exploded shell
had lodged in the neighborhood of the scrotum and
the inguinal fold, having hardly lacerated the subjacent
tissues; it was during the French war, and as soon as the man
could be taken care of, the foreign body was removed and the
tetanus subsided. Another case in which there was a very
superficial wound, in fact, a mere tearing of the skin of the
inner upper part of the thigh, which, during the granulation
period, offered a fine opportunity of testing the reflex theory. I
discovered that every time that a camel-hair brush, loaded with
carbolized oil, was being passed upon a certain granulation
area, the patient at once became rigid for some time; that
granulation area was cut away by means of a pair of curved
scissors, and no more rigidity was observed afterwards.

All these cases, and many others which everybody can find
in medical papers, permit one to say, therefore, that paralyses
as well as contractions of a reflex character do exist.

The theory offered by Brown-Séquard, to explain them, was
first opposed by Dr. (now Sir William) Gull and Dr. Pavy, by
Weir Mitchell and by Jaccoud, and by many others.

Sir Wm. Gull and Dr. Pavy not only analyzed some cases
so as to show that there were in them some organic lesions,
but undertook a series of experimentations upon animals to

refute Brown-Séquard's affirmations.   Brown-Séquard had
written that, while tying the hilus of a kidney, or lacerating
or cutting away one supra-renal capsule, he had seen the blood
vessels of the spinal cord contract.   His two opponents de-
clared that not only did they never succeed in obtaining that
result, but even that the blood vessels of the spinal cord are
not to be seen.   The experiments of Sir William Gull and Dr.
Pavy must have been carried on in a very unexpert manner.
They do not state how long they have been in opening the
spinal canal, nor the amount of blood lost by the animal, and in
what dietetic state it was at the time of operation, which several
factors being considered, we can account for the results of a
negative character which they have obtained.   I have several
times repeated Brown-Séquard's experiment, and I have always
had the same results that he has reported.   Prof. Vulpian has
also seen that faradization of a communicating nerve of a dog
under the influence of woorara, will bring on a contraction of
the blood vessels in the corresponding side of the spinal cord.

Dr. Brown-Séquard has also seen, what everybody can see
by performing the same experiment, that punctures with a
needle through the olivary column of the medulla oblongata,
at once produce a spasm of many muscles, which may remain
contracted for hours, days and weeks.   So much for contrac-
tions.

But objections have been taken upon other grounds also, by
Drs. Gull and Pavy.   Sir W. Gull has made this remark, that
it is astonishing that an abscess of the kidneys will cause no
reflex paralysis, whereas a single irritation of the organ, as
supposed by Dr. Brown-Séquard, will produce it.   There is
here a strange forgetfulness of a well-known physiological
law, by one of the most distinguished British physicians.
When the kidney is the seat of disorganization—abscess—by
pus, the nerve-ends of the kidney parenchyma are destroyed,
and it is well known that they have a hundredfold more power
than nerve-trunks to arouse actions, normal or morbid, of the
cen... .   I cannot refrain from giving a very striking instance.
In those celebrated experiments on the guinea pigs, which are
rendered artificially epileptic, it is well known that a zone
exists covering the area comprised between the anterior angle

of the eye to the shoulder; hence to the median line and the base of the ear, back to the posterior angle of the eye, which, on being simply titillated, brings on an attack of epilepsy. Now, if a section of the skin be made so as to uncover the trunk of one of the nerves, or successively all the nerves which animate that area of skin, it is found that no amount of titillation, or of squeezing even, will bring on anything else but pain. The same experiment may be repeated upon any number of epileptic animals, and will give identical results.

Weir Mitchell and Dr. Jaccoud have endorsed the criticism of Dr. Gull and Pavy, and say that the paralysis under consideration is due to exhaustion of nerve cells though work suddenly increased. They mean that if a sensitive cell receives too strong impressions it loses the faculty of registering any. They suppose, therefore, that in reflex paraplegia, for instance, the irritation from the kidneys which are the diseased organs producing the symptoms, being constant and of a more intense character, that area of the spinal cord in relation with the sensitive nerves of the kidney is exhausted, hence the paresis. It may be that such is the case in some instances, but I doubt it. If a nerve fibre is overstrained in the manner supposed by Weir Mitchell and by Jaccoud, the loss of function which ensues is seen to depend not upon exhaustion of the centre to which that nerve goes, but to the nutritive alteration, of that nerve itself, at the point irritated. This is a commonplace fact, in experimental physiology, and unless, therefore, it is proved that there is a necessity for the spinal cord to receive normal impressions from the kidneys in order to fulfill its functions with regard to the motor apparatus, I do not see the value of the argument; but, moreover, supposing things take place as Weir Mitchell and Jaccoud suggest, then it would be necessary also to admit that that area of grey matter of the spinal cord which receives the renal nerves is the functional centre for the innervation of the muscles of the legs? But even granting that point, since it is a generally received opinion that there are direct fibres from the bulb to the different parts of the nerve-centres, how is it that voluntary motion is also abolished, whereas if the theory were true only involuntary reflex movements ought to be abolished?

Handfield Jones has accepted both opinions, Brown-Séquard's contraction, and the 'inhibitory influence theory,' which he thinks explains the greater number of facts.

In Germany a certain number of experiments have been undertaken with the view of solving that disputed point.

Tiesler irritated the sciatic nerve of dogs and rabbits with substances likely to induce in it an inflammatory state. In the case of a rabbit he obtained paralysis which lasted for three days, when, the animal being killed, he discovered that although the central extremity of the nerve was not diseased except at the point irritated, yet there was an abscess at this point of the spinal cord, at which the nerve makes its exit from it.

Feinberg has also stated that he has observed softening of the cord after cauterisation of the sciatic nerve with caustic potash. He says that there was neither alteration of the central portion of the nerve nor inflammation ascending towards the spinal canal.

Leyden has based upon these facts his opinion that a great number of reflex palsies can be thus satisfactorily explained.

I have repeated these experiments of Tiesler and of Feinberg several times, but I have failed to obtain the results which they have reported. I am unwilling, however, to say that their results are only exceptional, because pathological science now possesses many observations carefully recorded, which show that a nerve in contact with a cancerous tumor, does present at times deposits of cancerous matter along its length, in several different portions, although the intermediate portions may be perfectly healthy. The results of the two above named authors, therefore, can be accounted for in the same way: migration of diseased matter through the lymphatic channels of the nerves.

Dr. Brown-Séquard has himself changed his views on the subject within the last eight years. He admits now that there are at least three modes of production of reflex paralysis. Reflex paralysis can be brought about by a reflex contraction of the spinal bloodvessels, by an inhibitory influence starting from a peripheral sensitive nerve end, and by ascending neuritis, or other degrees of alteration of nerves.

I believe that these three modes of production of paralysis do

give an explanation, satisfactory enough for all known kinds of reflex palsies, including of course palsy from brain disease. There are certain collections of nerve matter in the brain which seem to have a special endowment to produce palsies of that nature. There is for instance that very singular and characteristic hemianesthesia and hemiplegia, occurring after a destruction of the internal capsule, and which can be so complete that even the special senses are altered unilaterally. There are good grounds for believing that those disorders are not consequent upon destruction of a set of conductors nor of a centre, but are due to an inhibitory influence.

Prof. Vulpian, Prof. Charcot, and Dr. Burq, have recently shown, that faradic electricity and other modes of electricity have the power not only to cure for a day or two after each application, the motor and sensitive palsy, but also to restore sight, hearing and taste, to their former degree of perfection.

Now, on the other hand, if we take into consideration the saying of Dr. Hughlings Jackson, that motor paralysis can exist and disappear although the destruction of tissue is unrepaired in the brain, provided only a small portion of the motor tract is diseased, we will find that we must very certainly admit that these symptoms while producing entire paralysis of motion and sensation, do not recognize the *loss of tissue* as their cause, for, during the existence as well as after the cure of the motor symptoms, the same destruction of nerve matter exists in the brain. Therefore that destruction of nerve matter is not *per se*, immediately the cause of the palsy, but it exerts an influence on some other parts to cause the palsies; the morbid state of the parts diseased endowing them with that faculty. Moreover, the therapeutic results of Prof. Vulpian, Charcot, Gubler, Granett, and those more remarkable and much older ones of Dr. Burq, have shown that the same patient thus affected with hemiplegia and hemianesthesia—unilateral—can be cured for some hours and days, and then again become affected—all the time the same *organic lesion* being present in the brain, and being in the *same condition* apparently; as some of the patients experimented upon had symptoms of hemichorea, which is well known to depend upon secondary affections of nerve fibres.

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It is plain that the size of the lesion has no claim for consideration in these cases as Dr. Hughlings Jackson has stated—cases are numberless to prove that the destruction of a whole corpus striatum gives rise to no more complete paralysis, no more extensive, than lesion of only one of its ganglia, and *vice-versa*, etc.

If we leave out the cases of paraplegia occurring during the course of genito-urinary diseases, in which lesions of the sacral nerves were observed, or in which the muscles were diseased by rheumatism, etc., and which are, properly speaking, not reflex but peripheral paralyses, we will find that in every case of palsy, always some grey matter or ganglionic structures are found in the nervous arch. The law of Rouget is well known, that every time that we observe a reflex inhibitory action, we always discover a nerve cell on the tract of the nerve irritated. If we examine the pathology of contractures, we will find the same arguments equally serviceable. Every physician has seen cases of different degrees of contraction, due to so-called hysteria and organic lesions, give way for some time, or altogether, under the influence of the electrical fluid used in different modes, or of a fright or of a reflex action of some kind. Dr. Sayre has lately recorded some very interesting cases of contractures in young children, which gave way entirely after the operation for phimosis. In some cases operated on by Dr. Sayre, the relief has not been of long duration, if I am to judge from what I have seen, as some of those young patients have been under my care since the operation performed by Dr. Sayre, and are as yet suffering as much as before from the same troubles. I ought to say here *en passant* that these cases are not cases of reflex paralysis—as I have seen some similar ones recorded in different journals—but of reflex contractions; there is no paralysis of any muscle in the cases which I have seen. Those contractures following or dependent upon phimosis, or other disorders of the penis or clitoris in children from infancy up to the age of thirteen or more, are very peculiar—they affect identical groups of muscles in all the patients which I have seen; the extensors of the inferior limbs and the adductors, to such an extent that the limbs are crossed. They affect the deltoid, the biceps, the long supin-

ators, and the flexors of the palm of the hand. Also to some
degree the internal portion of the sterno-cleido-mastoid
muscles. The two internal recti of the eyes, also, so that the
patients have regular squinting, or often nystagmus since the
muscles contract or relax continually according as the head is
turned either side. In one case the tongue is in a state of tonic
contraction. In another, it is only so when the patient
tries to speak—in all there is an hypersecretion of saliva—
which last symptom is very characteristic of the nature of the
trouble, if the experiments of Cl. Bernard on the chorda tym-
pani are kept in mind.

I have satisfied myself that this state does arise from in-
creased tonicity of some muscles on the one hand, and of
diminished tonicity on the other. The contractions are never
very marked, nor do they occupy all the muscles just named,
except when voluntary movement is attempted—it is chorea of
some sort. But I have stated that palsies from brain diseases, as
well as from spinal diseases, are always reflex phenomena, not
due to destruction of centres or of conductors. I have given
some proofs already of that opinion. I shall now consider
aphasia, and show that it is also an inhibition or reflex paraly-
sis of the speech centre, if there is such a centre. Trousseau has
recorded several very interesting cases of aphasia, in which
the patients, unable to say anything voluntarily, all at once
the speech faculty being awakened by a reflex process,
said one or more words which they were unable to say again
when asked—as if a spring had been at work. My learned
friend, Dr. Onimus, of Paris, in a most interesting paper on
the subject, has even tried to establish that there is a phono-
motor apparatus in the brain. Whether such a centre
exists or not,—his arguments do not prove either very
conclusively—one thing comes out clearly. He reports the
case of a janitor who could say some words during recovery
from hemorrhage in the brain. When the first syllables were
uttered, he would readily complete the word; for instance, he
was asked to name a plaster of Paris statuette near at hand;
the doctor said *sta*, the first syllable of statue, but obtained no
response. Whereat the wife of the patient who was at hand
said *esta*tue, when the sick man repeated it at once. Dr. Oni-

mus then again said *est*, and the patient thereupon repeated
readily, an *estatue*. This one case is very remarkable, as it
shows that although the patient understood in either case, yet
the prompting, or rather suggestion, had no effect until the
mode of pronouncing used by uneducated people was adopted.

The cases of aphasia, for instance, which depend upon snake
bites, as I have the report of some cases in the bulletin of the
Societe d' Anthropologie, cannot be anything but reflex, unless
we take it for granted that the snake poison has an elective
action for the posterior portion of the left third frontal con-
volution,—where it is pretended by most that a speech centre
is localized—which I am afraid does not appear much less than
absurd.

These cases, together with cases of amaurosis following
trigeminal neuralgia, can be explained by the celebrated experi-
ment of Magendie, which shows that section of the tri-
geminal nerve does induce amaurosis. It also induces
anosmia and loss of taste. Certainly there is no other mode
of explaining these facts, but by admitting that an inhibitory
influence is exercised by the diseased trigeminus upon the
centres.

It is not, however, by directly acting on nerve cells that
those influences which cause what I call reflex paralysis and
reflex contractions take place. I cannot conceive how such
effects could well happen, for instance, in the cases reported by
Weir Mitchell, of lesions of nerves of one inferior limb being
followed by paralysis of one arm in the same side, etc. My
belief is, that effects take place through the agency of the vaso-
motor nerves. The diversity as to the seat of the reflex symp-
toms can be explained on the force of the law of "de locis
minoris resistentiae," so ably taught by Prof. Verneuil, of
Paris.

The vaso-motor nerves, I believe, do not take their origin
in the spinal cord, nor do they have their origin in the pons,
but run all along the spinal cord and middle encephalon, as
stated by Vulpian, Setchenow and others. I think that there are
experimental data enough to establish this theory, that the
vaso-motor system has its centres in the ganglia along the
spinal roots, and in disseminated ganglion cells found in the

meshes of the pia mater of the cerebrum, and more particularly, around the pons and anterior portions of the hemispheres. At the level of the spinal ganglia the nerves send branches into the spinal cord along with the nerve roots, chiefly posterior, and out towards the periphery along the nerves terminating in different organs.

If this view is admitted, it becomes easy to reconcile the otherwise contradictory results obtained by means of the Wallerian method. This is more than an hypothesis with me, as I have lately said and written much about it, and stated on what experimental grounds I support it. The modes of production of these sensitive, sensory and motor palsies are of three kinds, as I have said, and as Brown-Sequard teaches, but properly speaking, they are all explained in the force of the vasomotor influence.

It has been urged that a reflex or other contraction of blood vessels cannot be of long duration, without consequent alteration of the tissues—of course there is something in that notion, but who has not observed cases of hysterical anesthesia? In these cases, as I have lately again satisfied myself, the anæsthesia depends upon contraction of the blood vessels of the integuments of the skin. If in a hysterical anæsthetic patient a pin or a very sharp and fine blade be passed to the depth of one millimetre into the skin, not only no pain is felt but no blood oozes. If a fold is made of the skin, and this well squeezed several times, then the same operation is followed by pain and by a little blood oozing; moreover, it has been ascertained by different authors, and I believe among others, by Dr. Charles Richet, that the irritation of nerve trunks does give rise to some degree of pain, whilst that of the nerve ends is without effect. In a word, we have to deal then with a case of loss of function through anæmia.

I contend that in cases of so-called reflex paralysis of sensation, as well as of aphasia and of amaurosis, the same condition obtains in the centres. The irritation in these cases instead of starting from the centres towards the periphery, on the contrary proceeds from the periphery towards the centres; so that if pain is not felt in cases of hemianæsthesia from destruction of the internal capsule, for instance, it is not because the impres-

sion is not transmitted from the periphery, but because at the place at which the impression is received ordinarily, or where the mandates of the will act, there is functional inability, due to alteration of the blood supply; this can be brought about in two ways, either the irritative influence in diseases of the bladder, the urethra, the kidneys, the intestines and other peripheric nerves, the lungs or the pleuræ, has been so strong that a state of paralytic dilatation of the blood vessel has taken place, identical with the one which follows irritation of the sciatic nerve, separated from its centre, and which has been said to be due to the action of dilating nerves (about which more hereafter), and the exchanges between the blood and the tissues are stopped, or the contractions of the blood vessels have so acted as to exhaust the receiving nerve cells—have brought on an inhibitory action.   Only one set of nerves are concerned in these vaso-motor troubles—the vaso-constrictor.   Claude Bernard, Schiff, and Goltz, and many others have introduced into science the idea that there is another set of nerves whose function it is to antagonize the action of the vaso-constrictor.   But this is not the case.   Vulpian has said, after Bernard, I believe, that when a vaso-motor nerve is irritated, if a dilatation ensues, it is because an action has taken place very similar to the phenomenon studied under the name of interference by physicists—that is to say, that two actions take place in the ganglionic nerve cells which are scattered in the blood vessels and which are thus paralyzed, being the victims of antagonizing forces.   But more recent researches have proved that such is not the case, as it has been shown that the rapidity of the interruption of the current used serves to explain the phenomena.   A rapidly interrupted current does bring on, after a *very* short period of contraction, a dilatation, but a current interrupted very slowly—one or two interruptions in a minute—does not bring on a dilatation. The cause is obvious.   The nerve cells and nerve fibres require time for recuperation of their power through nutrition, and if they are made to work faster than they can recuperate lost energy they are paralyzed.   This same theory explains what takes place in the centres—spinal cord and brain—in cases of reflex paralysis and contractions.   In dogs I have

laid bare the spinal cord and observed that irritation of one
kidney with electricity does bring on a paraplegia. I have
seen the blood vessels of the pia mater become invisible for
some time, and then gradually enlarge until no more pulsa-
tion could be perceived in them, the blood appearing of a more
pale color. The same experiment can be made on the ears of
rabbits. The blood vessels relax to such an extent that the
exchanges between the blood and the tissues do not take place
at all, or not according to a normal standard.

It may be said that in such a case the paralysis might be
due to the proliferation of new elements through the blood
vessels — to diapedesis — to squeezing of the nerve tissues,
by the consequent proliferation of connective tissues—on the
strength of Cohnheim's theory. But if it is kept in mind that
paraplegia in dogs and in rabbits takes place within an hour
after the beginning of the experiment, and that no white cor-
puscle has ever been seen to come out completely through the
sheath of a blood vessel within that time, and that they do
not increase in their new media very rapidly, it becomes
necessary to reject that theory, so far at least, as used to
explain the paresis in the very beginning.

But so much for paralysis. Contractions more often recog-
nize for their cause similar disorders in the membranes of the
cord. It is often seen that the mere laying bare of an inch
length area of the dorsal dura mater, will induce a tonic con-
traction of the whole posterior extremity of a dog, lasting for
hours and for days. The cerebral dura mater can be experi-
mented upon very easily, and these effects are readily observed.

The experiments made on the cortex cerebri go a good
way to show reflex actions at work to inhibit speech or vision.
It is seen that when one of the so-called psycho-motor centres
is taken away, a paralysis ensues which lasts some days or
weeks, but which ultimately disappears; just as readily does
the same thing happen when some other area of the cortex is
similarly irritated. The loss of the senses of sight, of hear-
ing (very seldom), of smell, of speech, etc., are due to inhibi-
tory influences, started by the lesions of the cortex, but not
because the so called centres have been removed, as there is no
relation whatever between the degree of the alteration of the

cortex, its kind, and its seat on the one hand and the palsy, or the contraction or the loss of faculties observed on the other, as I have established elsewhere.

As to the treatment of these affections, I have relatively little to say, because the indication of therapeutic agents is determined by the nature of the disorder, or rather by a consideration of the physiological pathology.

First among the internal remedies is strychnia—but strychnia administered in such a dose as to obtain the full physiological action. Of course, it is only possible for me to make general statements on that subject of therapeutics, as so many circumstances need to be taken into consideration in every case. But when the paralysis is not due to inflammation, the remedy is good for all kinds of paralysis.

Arsenic, in cases of children chiefly, affected with contractions of that nature which seems to be associated more particularly with phimosis, and which are nothing but choreic, will give good results if pushed to high doses. Generally there is no need of the operation of circumcision, as lotions made of bromide of ammonium and water freely used answer the purpose; if the prepuce is adherent, of course it has to be detached, but I believe that circumcision in many cases does not relieve the patients, because the glans penis is exposed, its nerves are hyperæsthetic, and therefore it is a source of continual irritation.

In cases of paresis or of contractions, methodic movements very slowly executed will be of great service. For both, rest as recommended by Weir Mitchell will work well.

Among the iron preparations, I believe that none will be more useful than the protochloride, as it has been proved to be the state into which all iron preparations are reduced in the stomach; in anæmic cases it is very useful. Those cases in which there is hemianæsthesia and chorea, or aphasia, etc., derive no little benefit from the use of faradic electricity on the anæsthetic parts—*very* weak currents should be used and the metallic brush be applied as one electrode, the other being a wet sponge. Dry the parts well before using the brush. Sometimes it is necessary to use a rather strong current, but very seldom. It has been observed that a strong

current will dull the sensation which was awakened by a mild
one. The sensation returns very rapidly and the temperature
of the parts rises as does the muscular power. But chiefly in
cases of aphasia, amaurosis and hemichorea, the actual cautery
applied to the nape of the neck is exceedingly useful, the in-
strument to be heated to whiteness and not allowed to burn
the skin—to be used with a light hand and often repeated.
When there are traces of inflammatory processes of the mem-
branes or of the nervous tissues, blisters of mustard on the
points of the spinal cord tender to the touch are very useful,
and ergot internally, or belladonna, etc.

These are the general indications, which, I trust, are in
accordance with what I believe is the pathology of the diseases
considered, and which have, besides, when used empirically also
afforded good results.

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Art. III.—THE PSYCHOLOGICAL PATHOLOGY OF PROGRESSIVE PARESIS.

By Edward C. Spitzka, M.D.

IN the choice of progressive paresis, for my subject, I was
determined, not so much by the intrinsic interest of this
affection, as by the fact that through its pronounced and con-
stant characters it furnishes the best illustration of certain
general propositions, whose enunciation is my chief object this
evening. I do not intend to give a detailed and continuous
account of either the clinical symptoms or the morbid lesions
of progressive paresis, but rather to exhibit the physiological
relation existing between these two factors.

As this is a comprehensive and controversial subject, it necessitates a reference to almost every attainment of modern nerve physiology; and I have therefore thought it well, in order to avoid infringing on the subject proper by the discussion of side issues, to briefly define my position with reference to some recent views, as a preliminary.

Among those diseases which may be considered pathological gnages of the value of the experiments performed by Hitzig and Ferrier, the subject of this paper is certainly one. I myself began my investigations into the pathology of this disease, with an expectation of finding localization of the morbid processes, in particular areas of the cortex, corresponding to the motor peripheries chiefly involved. Naturally, these expectations were only partly realized; at the same time I should state that I was never convinced of the conclusiveness of Hitzig's experiments. There are too many sources of error in his methods, and that author has himself recently confessed that, on endeavoring to verify his conclusions by extirpating his so-called motor centres, he obtained negative results alone! I do not object to Hitzig's deductions on the same grounds which several French investigators have urged, namely, the supposed action of the electric current on the nerves of the pia mater! Even if this took place, the general truth of the principle of localization, would not be thereby invalidated, for these experiments could only gain in value if it should be shown that they acted by causing a local hyperæmia, strictly comparable to a functional determination of blood towards an active centre; an experiment which so closely imitated natural conditions would be the very ideal of the physiologist!

The exceptions to be taken are of a different nature, they are founded partly on the diffusibility of currents in a parenchyma, close to which conducting strands run in all possible directions, partly on the intimate connection, which we are forced to admit *a priori*, as existing between motor and sensory cortical areas.

Leaving the question, as to our ability to localize motor centres, in the lower animals experimentally, I strongly dispute whether, this being accomplished, we would be justified in transferring the results obtained to the human brain without further ceremony.

Attempts of this kind have been made on the strength of the homology in convolutional types among the Mammalia, which has been established by Huxley, Flower, Wernicke, Leuret, and other comparative anatomists. While this general morphological correspondence is unquestionable, it is not so decided, that anatomically homologous regions are necessarily physiologically homologous. If the development of the convolutions bears any constant relation to their motor function, we should expect that particular convolutions will rise and sink in their relative dimensions and complexity, with the variations in bulk and importance of the corresponding motor periphery. (In the kangaroo, for example, we would expect to find the centre for the hinder extremities to preponderate over the homologous area in the mole.)

This expectation is not fulfilled,* and with this, the transferribility of motor centres from one species of animals to another falls to the ground.

I could multiply examples, which all would show that the convolutional type does not depend so much on the habits and functions of a species, as on the powerful hereditary influence which has been transmitted to all descendants of a common stock, by an ancient ancestral race in bygone geological periods. The convolutional peculiarities depend on genetic and evolutional expressions of mechanical laws of growth, not on subsequent specific functional modifications.

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*There is a mammal, inhabiting the rivers of Africa and South America, which furnishes a test case; namely: the Manatus. This creature corresponds closely to the porpoise, both in its muscular periphery and general outline, for both animals are constructed with reference to an aquatic residence. On analyzing those structural affinities, however, which are alone conclusive in classification, we find that the manatus belongs to the pachydermata, and is consequently related to the elephant, than which a more dissimilar animal, as to shape and muscular contrivances, cannot well be imagined. Now on comparing the cerebral hemispheres of the manatus with those of the elephant on the one hand, and those of the porpoise on the other, we find that the resemblance is greatest in the former instance. In short, the convolutions are most similar in two animals, whose motor periphery is most unlike, in contradistinction to the dissimilarity existing between the convolutional distribution in two animals, whose motor periphery is the same. This bears out what is stated above.

We can easily afford to pass by the English writer's views without further discussion. Aside from the fact that he has merely adopted Hitzig's method and idea without appropriate acknowledgement, his only claim to originality is to be found in the fact that by increasing the strength of the currents employed, he has increased his sources of error. There is one startling exposition to be found in his first monograph. In the sixth experiment, on irritating a certain area of the second frontal gyrus, the dog experimented on, got up, looked fixedly in one direction, wagged his tail and fawned; Ferrier leaves the reader to form the conclusion that he has set an intellectual centre into action by characterizing these actions as an acted dream, and, in fact, he registers this mental centre with label No. 14.

He is evidently not aware that many animals perform the same and similar movements under chloroform, even when the brain is not touched, and every surgical operator is familiar with the co-ordinated actions, nay whole sentences spoken by human beings during the progress of an anæsthesia.

As to the more exact and reliable localization of functional areas, by anatomical methods, it is the only one which promises a satisfactory solution, of the difficult problem of normal and morbid cerebration. Its teachings should form the basis of every attempt, like the present one; to refer mental and motor symptoms to cerebral lesions.

Each area of the cerebral cortex is connected with the outer world, through medullated nerve fibres, which by their mediate connection with the peripheral nerves, terminate in the muscles and the peripheral " end-organs " of sense. It is consequently evident, that the fibres of the corona radiata represent both centripetal sensory and centrifugal motor tracts, and that, if it will ever be possible to trace the fasciculi of white substance from the nuclei of the nerves, to the cerebral cortex, we shall undoubtedly be able to map out the convolutions into functional centres, if these exist in the strict sense of the term. Such a continuity can not be traced directly, for ganglionic masses are interposed, interrupting the course of the fibres at various topographical altitudes. Our future exacter knowledge of cerebral physiology, depends on

the unravelling of this Gordian knot of associating commissural and decussating fasciculi. The first step in the right direction was taken by Luys and Stilling, and Meynert and Flechsig have more recently and more successfully undertaken the same task.

Meynert found that the great medullary tract through which each hemisphere is connected with the lower centres and the peripheral nerves, the crus, is naturally divisible into an upper tegmental and a lower basal portion. the pes pedunculi. Through each of these the prosencephalon is connected with the periphery, by the intervention of different ganglionic systems. The upper tract contains those fibres which have passed through or been interrupted in their course by the corpora quadrigemina and the thalamus opticus, he therefore terms these ganglia, tegmental ganglia, in contradistinction to the corpus striatum and nucleus lenticularis, which because they are connected with the basal tract, he distinguishes as ganglia of the pes pedunculi. It is through the ganglia of the tegmentum. that, besides other functions, the higher automatic actions are carried on; by the corpora quadrigemina, for instance, the regulation of locomotion, in reference to our visual impressions (through the lemniscus); by the thalamus, its regulation with regard to the tactile impressions. Through the ganglia of the pes pedunculi on the other hand, voluntary impulses are transmitted to the periphery. Sensory impressions are forwarded by both in a different degree.

If we proceed to trace the two medullary systems downward, through the pons, we find that the fibres of the pes pedunculi are directly continuous with the anterior pyramids. Now as you are aware, the anterior pyramids undergo a double decussation by the upper fibres, cross through the medulla to reach the opposite funiculus gracilis and funiculus cuneatus. These fibres are consequently continuous with the posterior columns of the cord, which proves the upper decussation to be of a sensory nature. By the lower, fibres are sent to the opposite lateral columns, while such fibres as do not cross at all, are continued downwards, as part of the anterior columns of the cord. (I must distinctly reiterate here, that while there are all possible variations in the decussation, this decussation is

never a complete one, as some recent compilers seem to believe, in ignorance of the researches of Lockhart Clarke, and Meynert). These fibres, together with those constituting the lower decussation are therefore motor.

On retracing these fasciculi to where they form an integral part of the pes pedunculi, we find that those which participate in the sensory decussation, are represented by the *outermost* fibres of the pes, the motor fibres by the *innermost* and middle portion.

Proceeding still higher to where the fibres of the pes pedunculi enter the ganglia and the internal capsule, we perceive that the outermost sensory ones run through the corona radiata to terminate in the occipital lobe, by a bold sweep backwards. I have been able to trace this fasciculus, which loses itself in a fan-shaped scattering of fibres to the posterior part of the gyrus fornicatus, the cuneus, and the praecuneus. From this fact there is but one conclusion to be drawn, namely, that the occipital lobe is subservient to the sense of touch, for no other sense can be projected through a tract which is continuous with the posterior columns of the spinal medulla. As Gratiolet had already shown, those ganglia into which the optic tract enters, which are mainly the corpora quadrigemina and geniculata, are connected with the occipital and posterior part of the parietal lobes by a powerful fasciculus, the "*radiations optiques.*" We will consequently locate the conscious centres of visual impressions in this area, also. Burdach, Gratiolet, and Meynert have further shown, that the white anterior commissure which is in reality an *olfactory chiasm*, is also connected with these cortical areas by a slender fasciculus. It is a natural conclusion then to attribute to the occipital lobe, a definite relation with the sense of smell. We thus see that the occipito-parietal lobe possesses three decided sensory connections. The frontal lobe with the island of Reil and operculum, is connected through the anterior part of the internal capsule, and the motor ganglia with the *innermost* area of the pes pedunculi; the old experimental and pathological conclusion that these cortical areas are motor territories, is thus anatomically confirmed.

The lenticular nucleus receives its fibres from the island of

Reil and operculum chiefly. the fasciculus which leaves its lower
border passes close to the median line, forming *the most inter-
nal fibres of the voluntary motor tract*. Meynert concludes
from this that these fibres form those nearest the median line
in order to be the first to cross the raphe and place the motor
nuclei of the cranial nerves under the crossed influence of the
opposite hemisphere. His surmise is. that as this bundle rep-
resents the motor cranial nerves, the cortical area, above
mentioned. from which it originates, is the centre for the nerves
of expression, both phonetic and symbolic.

But while it is quite clear that the frontal lobe is eminently
motor in its connections. we cannot yet assert that it is exclu-
sively so: as little can it be said that the occipito-parietal lobe
is solely sensory. The tegmental ganglia on the one hand
receive fibres even from the most anterior part of the frontal
lobes. while the caudex of the corpus striatum is in a connec-
tion. however slight, with the extreme occipital end. I should
consequently be inclined to characterize the functional topog-
raphy of the convolutions, not so much as a sharp, abrupt
demarcation of various centres, but rather as a blending, a
dovetailing. in which there are marked foci of functional con-
centration.

On proceeding to examine more intimately, the distribution
of the fibres to the cortex, we find that two kinds of fibres
terminate in the latter. Those which formed a part of the
corona radiata enter the gray substance in bundles, which
spread out fountain-like, the individual axis cylinders can fre-
quently be traced to be directly continuous with the basal
processes of the ganglionic cells. The other class consists of
fasciculi. which connect the apex of each convolution, with the
apex of its neighbor, by arching around the lowermost layer
of cortical substance at the base of the sulci. It is an infer-
ence, in complete accordance with physical laws, to suppose
these as well as similar fasciculi which join more distant areas
to be associating tracts. That, furthermore, such associating
tracts must play an important part in the mechanism of
coherent thought and associated voluntary action need not be
demonstrated.

It would lead me too far, to-night, to recount the philosoph-

ical explanation of thought and action, on a purely physical
basis, which Meynert and Wundt have given, and which is
quietly but surely gaining adherents on the continent, and
which will, no doubt, revolutionize the thus far unsatisfactory
and unscientific methods of explaining the phenomena of in-
sanity.   Suffice it to say, that our highest intellectual pro-
cesses, according to these philosophers, depend on molecular
changes in the nerve cell and axis cylinder, which are com-
parable to electro-negative oscillations; and that the more
numerous and powerful the impressions, and motor innerva-
tions stored in the six hundred million of protoplasmic
organisms, which people our cortex, and the more extensive
their protoplasmic connections and the arched associating
fasciculi, the more perfect will be that voluntary control over
our impulses, which distinguishes the cultured man from the
savage, the sane man from the lunatic!

And here we are approaching our subject.   How are we to
picture to ourselves the manner in which a morbid process
causes aberrations from the normal standard of mental action?
More particularly how does it produce the mental and motor
phenomena, characteristic of progressive paresis?

The principles which should guide us in such an inquiry can
be laid down in the following propositions:

I. Every manifestation of typical progressive paresis may be
formulated as consisting in an altered activity of some one of
the higher functions of the nervous system; the sole seat of
these functions is in the prosencephalon, and it is obvious that
in searching for the somatic basis of this alienation, our atten-
tion should be primarily directed to the region specified.

II. Whenever an anatomical or chemical change is discovered
in the higher centres of an individual, dying with paresis,
which can be shown to be independent of the manipulations
by the histologist, as well as of the results of post mortem de-
composition, we are justified in assuming such change to be
the proximal cause of the symptoms manifested during life, on
the part of the mind.   Such an assumption becomes almost a
demonstrated certainty, when a number of individuals, affected
with the same or similar group of symptoms, exhibit the same
or similar cerebral changes.

III. It is equally evident, since the prosencephalon is not a single organ, but an agglomeration of numerous, variously associated, and functionally, more or less differentiated centres, that it will be well to determine not alone the histological character of the morbid process, but also the precise topography of the region involved.

IV. Inasmuch as other portions of the cerebro-spinal axis, beside those concerned in conscious action and sensation, are occasionally involved in subjects of progressive paresis, and the resulting symptoms serve to mask or modify the clinical aspect of the disease, the automatic and reflex tracts extending from the third ventricle down to the filum terminale, should be carefully examined in order that their disorders may be differentiated from those of higher centres.

The first proposition is verified by the testimony of every competent observer. As to the second, it remains to be seen, since the essential lesion is undoubtedly cortical, whether the pathological change in progressive paresis is constant, and in what it differs from the pathological changes of other forms of insanity, and other hemispheric affections.

Our answer to this question depends on, what is all included under the term progressive paresis. Recent investigation has shown that the combination of mental and motor symptoms comprised under this head, does not present a uniform type, but that there are several varieties of the disease, independent of the fact that progressive paresis is occasionally a complication of chronic melancholia and chronic mania. If all these various forms be promiscuously thrown together, then I must confess that there is no particular morbid process characteristic of the disease. But if paralytic insanity be considered a generic type, under which several distinct species are admitted to exist, then I am fully prepared to maintain a unity of the pathological character for each such species.

(a) The first variety constitutes the classical picture of paresis, in which the mental symptoms appear first, and are soon followed by disturbances of the delicate muscular contrivances of the eyeball, face, tongue and pharynx, the extremities being involved last. Topographically speaking, this is a descending affection.

(*b*) The second variety is founded on numerous cases observed by Westphal, to which I can contribute several instances, in which the first symptoms are spinal, and the muscular co-ordinations presided over by the cranial nerves, are destroyed in the order in which their nuclei lie in the floor of the fourth ventricle and the aqueduct of Sylvius; the intellectual symptoms are the last to manifest themselves. This, in contradistinction to the former, is an ascending affection. I am inclined to consider the mental symptoms in this case as independent of the original affection, and altogether regard these cases as instances of locomotor ataxia, complicated by progressive paresis, although my investigations on this point are not yet concluded.

(*c*) We also see, though less frequently, a third group of cases in which the cerebral and spinal symptoms nearly coincide as to their time of appearance, the former more in the way of a progressive dementia, than of delusions of grandeur, the latter presenting itself as a progressive paralysis and ataxia of individual muscular groups. If it were clinically possible to separate this form from the others, it would be, pathologically speaking, desirable, for the lesion is a multiple cerebrospinal sclerosis, although the symptoms supposed to be characteristic of this process are ill-marked.

(*d*) Corresponding to the third form, as regards the synchronicity of the encephalic and the spinal symptoms, but materially different from it in that the mental aspect of the case is so like that of the typical affection as to be scarcely distinguishable from it, is one of the manifestations of the syphilitic dyscrasia. There are here, primary changes in the vessels, a subsequent infection of the adventitia, and, finally, destructive changes in the nervous parenchyma proper.

While these four varieties will have claimed the attention of every physician who has seen a large number of cases, there are others which are rare, if not unique. I include here more particularly a case, which exhibited all the symptoms of progressive paresis, but in the autopsy, presented multiple syphilomas scattered through the encephalon. As such a case, which first staggers one, by its exceptional character, furnishes an admirable argument in favor of the dictum, that symptoms

referable to the cerebro-spinal centres are less attributable to the microscopic character of a lesion, than to its topography and extent, I subjoin the necropsy, preceded by a brief history, for which latter, I am indebted to Drs. Kiernan and De Hart.

" The patient, a French laborer, was admitted to the City Asylum for insane males, at the age of twenty-six; he was luetic and intemperate.

" On reception, he exhibited delusions of grandeur of a rather stupid character; he maintained that he possessed vast amounts of movable and landed property, also that he was about to grow immensely in stature. At this time, he began to manifest a slight difficulty of articulation, and his tongue was tremulous. For the first two weeks, he was very excitable, so as to require large doses of sedatives. After this period, he became progressively more incoherent and irrelevant in conversation; he shouted at the top of his voice, without being moved thereto by any apparent cause, and refusing to take food for two days, as he imagined it to contain poison, was transferred to the hospital ward. The motor paresis had meantime increased to almost complete paraplegia of the lower extremities, while the labial tremor was extreme. He still retained his old delusions, more stupidly expressed than previously, to which he added that he was able to lift the building on his little finger, but he could hardly enunciate one continuous sentence uninterruptedly. A well-marked convulsive attack was noticed, and the difficulty of articulation having increased to well-nigh complete aphasia, death took place from exhaustion, following a maniacal attack.

" The most marked mental symptom manifested by this patient was a complete loss of certain recollections, a greater part of his existence while in France had become a complete blank to him, he had forgotten altogether that he had been a laborer. My attention was called to this case by Dr. Kiernan, more on account of a peculiar motor symptom than for any other reason, this symptom being the only one in which it varied from the ordinary course of progressive paresis. It was a passive contracture of the muscles of the neck and left arm, the chin being drawn to the left and backwards so as to

touch the left clavicle, the arm being partly flexed and pron-
ated, and carried behind the back.   On rousing him, he could
abandon this constrained position, but gradually relapsed into
it afterwards.   From this I made the diagnosis of a left
thalamus affection, locating the lesion in the posterior part of
that ganglion in accordance with Meynert's diagnosis of the
same symptom in an epileptic imbecile, in whose case the
autopsy confirmed the diagnosis.

   " In this case, I made the autopsy twelve hours after death.

   " The cerebral dura was adherent to the cranium, and pre-
sented a greenish discoloration over the left lobulus tuberis,
otherwise it showed nothing abnormal.   The dura mater
spinalis, however, was extremely thickened from the exit of
the first to that of the fourth cervical pair, exhibiting other
appearances of pachymeningitis.   The leptomeninges of the
encephalon were thickened and infiltrated with young cells,
but no pus, coagulable lymph, or increase of the arachnoidal
fluid could be determined.   A similar, more intense process
had affected the spinal membranes, and a diffuse, gummy in-
filtration, cheesy in its centre, involved the parts around the
anterior fissure of the spinal medulla, opposite the roots of the
third cervical pair, and had destroyed the anterior commissure
of that district.

   " The cortex of the hemispheres was everywhere the seat of
various superficial and nodular infiltrations.   The nodules
were of all dimensions, from microscopic to half an inch or more
in dimensions; the smaller were spherical and peri-adventitial,
with respect to some vascular trunk; the larger were ovoid,
their long axis represented by the stem of a large vessel, being
vertical to the cortical superficies.   The diffuse infiltration in-
volved the uppermost layer of the cortex, and was chiefly
located at the floor of the sulci, and especially over the island
of Reil, as well as the corresponding inner surfaces of the
operculum and temporal lobes.

   " Where the nodular growths had become larger, they fused
with the superficial infiltration, giving to the latter an appear-
ance as if it had processes dipping into the deeper layers.
Frequently several nodules situated on different branches of
the same main vessel, fused with each other around the latter,

thus resembling a papilloma. The white substance was also affected, the nodules not being, however, so well marked, and the transition from the neoplasm to the normal tissue was gradual. Their color varied, as did also their consistency; while the smallest were undistinguishable by the naked eye from the surrounding tissue, medium sized nodules exhibited a greyish-reddish, or yellowish-white centre, with a markedly red zone at the periphery, these were quite firm. Larger ones showed the same peripheral zone, with a softening of the centre, and the last stage of the breaking-down process, which terminated the existence of these masses was represented by cavities of varying dimensions, some without any sharp boundary and softened walls, others provided with a more or less firm connective tissue capsule. The distribution of these masses was symmetrical in both hemispheres, but the softening process had proceeded further on the left than on the right side. The right thalamus was entirely free, while the left which was intact in its anterior half, was converted into one mass of anastomosing nodular and tubular infiltrations in its posterior tubercle. The focus of the change was in the lenticular nucleus and the island of Reil, as you can see in the specimen, which I hand around, by the naked eye. It represents a section through the territories mentioned, made by a microtome; the largest cavity, or rather gap, which you see, involves the claustrum, external capsule and the outer articulus of the nucleus lenticularis, the latter is actually riddled with cysts, the internal capsule is relatively free. Other regions which were affected in a high degree were the central extremities of both praecentral gyri; the lobulus tuberis and the cornu ammonis. The left tegmental tract was completely broken down; one large cyst occupied the place of the left olivary body, two smaller ones were situated in its fellow. There were besides several small, diffuse, and for the most part softening nodules in the cerebellar hemispheres likewise quite symmetrical.

"The microscopic examination showed the youngest tumors to consist of an accumulation of round mononucleated cells, varying in size from the dimensions of a red corpuscle to those of a white one. These were situated in the adventitial sheaths of the vessels, and gradually encroaching on the perivascular

space, obliterated the latter and penetrated into the neighboring neuroglia. Now the neoplasm began to assume distinctive characters; the main body was composed of older elements which did not imbibe carmine well, and stiff, coarse fibres, pyramidal nerve cells, with intact contours, were still to be seen in the very midst of the mass; the peripheral zone was composed of young elements of the nature described above, as well as free nuclei, which were rapidly and deeply stained by carmine. Later on, the centre of the nodule underwent a partly mucoid, partly granular disintegration; in the resulting detritus mutilated nerve cells were yet visible. The neoplasm, whose periphery exhibited a rich vascularity, was determined to be a miliary syphiloma.

" It is unquestionable, that if the patient had not died from exhaustion, we should have attributed his death to the process which destroyed the important centres of the medulla and tegmentum.

" We here had a neoplasm, by which its multilocular occurrence produced those symptoms usually supposed to be associated with the chronic inflammatory process characteristic of typical progressive paresis. As we shall show further on, the elements in this inflammation which are productive of the morbid symptoms, are due, on the one hand, to anomalies of the circulation, and on the other to localized destructive processes. Notwithstanding its widely different histological character, this neoplasm acted in the same way; by its presence it kept up an irritative hyperæmia, and through its proper necrotic processes caused localized destructions in the cortex, the ganglia, and medullary tracts.

" As to that symptom, noticed as a permanent change in the patient's position, and which had led me to diagnosticate a thalamus affection during his life, it could not be so clearly referred to that ganglion after the thorough post mortem examination had been made. It is true the left thalamus was diseased, and that portion which had been surmised to be affected. But there was also a lesion of the left tegmentum, as well as of the spinal cord; there are thus three lesions to choose from, either one of which might have produced the symptom in question.

" The aphasia, in like manner, could not, as far as its ataxic element was concerned, be referred to the island of Reil or operculum, for the left hypoglossal nerve was totally destroyed by the large area of softening in the left olive."

In subjects of typical progressive paresis who die through intercurrent affections in the first stage of the disease, the only pathological change I could discover was a certain degree of leptomeningitis, and a dilatation of the perivascular lymph-spaces in the cortex. Neither of these changes is in any way pathognomonic of progressive paresis, for meningitis is found indifferently in other forms of alienation, as well as in sane persons, while dilatation of the perivascular spaces is much better marked in chronic alcoholism, melancholia, and epilepsy of long standing. These two changes have an etiological bearing, however, especially the latter, which, by weakening the lateral support of the vascular walls, throws an increased strain on the centres which regulate the vascular tonus, so that these breaking down, cause the hyperæmias which we shall refer to, and by recruiting their powers, determine the return of the vascular current to approximately normal conditions, and with this an amelioration for the time being of the symptoms.

Where, however, the patient has died in consequence of exhaustion from a maniacal outbreak, or has died from other causes shortly after such a one, we find a high degree of engorgement in the cerebral capillaries; at the same time proteinaceous bodies are found in the neuroglia, which, it seems, can be rapidly removed by the lymph channels. These bodies are identical with the so-called colloid bodies of authors, and are to be regarded as a true exudation. It is true the nuclei of the neuroglia are frequently concerned in their formation, but this occurs secondarily to the production of the material in an amorphous condition, the neuroglia cell takes it up as a morbid pabulum, and thus the round bodies familiar to cerebral pathologists seem to be produced. In many early fatal cases, such as die in the first and second stages of the malady, and which, on account of the violent motor excitement, rapid flight of ideas, and the fact that the paresis is not well developed, are frequently confounded with acute mania, in some of our asylums, these bodies are found even down in the commencement of the spinal medulla. Smaller ones join to form

larger accumulations, forming opaque spots visible even to the
naked eye. Such changes, on account of their obvious fatality,
have no psychological interest for us.

I do not hesitate to attribute the rapid flight of ideas to this
hyperæmia, further its incoherent character to the fact that
in certain cortical areas the engorgement reaches a higher
degree than in others, producing an actual stasis, which suffices
to render certain recollections, forming part of the patient's
"ego," by which he appreciates his proper relations to his past
and present, nugatory. I have frequently found partial stasis
in the cortex of patients who had died in a stupid condition,
combined with excitability. As the disease progresses, it seems
that the vessels, badly supported in the dilated perivascular
spaces, and subjected to repeated overstrain by repeated en-
gorgements, suffer in resisting power, so as to become unable
to discharge themselves of their contents, and forward the ac-
cumulating blood-column; when the next hyperæmia occurs,
we will thus have a genuine stasis, and this stasis is of a pecu-
liar character; we find the blood corpuscles no longer distin-
guishable, but replaced by, or rather fused into, an opalescent
hyaline cylinder. When resolution takes place, this cylinder
breaks up into spherical or oval fragments, which are carried
onwards in the vascular current, becoming further subdivided
at each bifurcation, and finally are represented by a granular
material having the same optical appearance as the larger
masses. Meynert, who first describes this condition, brings it
into relation with the stupor, and observes that, as the stasis
follows the hyperæmia, so the stupor follows the excitement.
This explanation is exceedingly plausible, and we may carry it
still further, and say that the resolution of the stasis corre-
sponds to the so-called lucid interval following the stupor: the
possibility of a well-nigh perfect* re-establishment of the
patient's normal mental condition, is readily comprehensible,
when we regard the pathological changes as mainly nutritive,
not destructive, in their character.

*I do not intend to convey the idea that the lucid interval is ever com-
plete, and consequently give my fullest assent to Westphal's exceptions to
Lubimoff's statement, but I must insist that the return to the approxi-
mately normal state, may be so nearly complete as to render the mental
defect undiscoverable, save to the experienced physician.

There is no region of the brain which is so vulnerable to vascular determinations as the lenticular nucleus, for here the vessels are less supported than in any other portion of the encephalon. We consequently find that dilated and contorted vessels are nowhere so frequent as here, and it is a *locus præ-dilectionis* for the stasis described. Accordingly no symptom is so constant in progressive paresis as a disturbance of those delicate co-ordinations which are transferred through this ganglion, namely, the articulation and expression.

With the repetition of this vascular stasis, destructive and irreparable change takes place. This occurs both in those cases in whom frequent exacerbations and remissions point to rapid changes in the blood-supply, and in those where the more continuous course of the symptoms would lead us to picture to ourselves a more permanent, slowly-increasing and slowly-produced condition of engorgement. It is these permanent destructive changes which determine the peculiarly progressive character of the malady. With each engorgement clinically marked by an excited stage, the encephalon becomes more or less crippled. While in the earliest attacks the patient speaks with a rapidity which a sane man can hardly equal, and there is a truly wonderful flight of ideas, marked by a decided degree of energy, and even of creative fancy, we find that in subsequent attacks his flight of ideas and speech both become slower, and the patient is able to follow his ideas in writing, and finally contents himself with penning endless quires of disconnected and melancholy evidences of his dementia.

Meschede, in this connection, reports a case of a paretic who, when first brought into the asylum, suffered from a maniacal attack of thirty-six hours duration, in which the flight of ideas and rapidity of speech, amounted almost to a delirium. He did not interrupt the torrent of sentences which issued from him but once or twice, to moisten his parched lips with a drink of water, and all this time was engaged in measuring the orbs of the planets, the distance of the dog-star, squaring the circle, and he finally gave a feast to the whole world of truly Arabian Night's profusion. It was in a case of this kind, which died from exhaustion after such an attack, that I found the engorgement and colloid filtration alluded to.

The final destructive changes are of two kinds, those which affect the nerve elements directly, and those of whose damaging effect on the latter, we have no direct optical evidence but can judge inferentially.

The characteristic of these processes in progressive paresis is, that they are rarely general, but that they affect certain provinces of the cortex, and leave others entirely or nearly intact, as far as we are able to determine. Not only this, but there is the greatest difference in the affected localities. While some are so utterly disorganized as to leave only one inference open, that their function was completely cut out of the mental life of the individual, others present such slighter changes, as would lead us to infer that the corresponding functions were merely weakened. We should thus have a second parallel to establish between lesions and symptoms; the totally destructive process would correspond to complete blanks in the patient's memory, and the entire loss of control over definite muscular groups: the partially destructive ones to a dimness of recollection, and diminished control over the same motor periphery.

It is a significant fact, that those movements which depend on the voluntary association of smaller muscular territories, suffer before those which depend on the combination of coarser contrivances. It would seem to point to the fact that the smaller muscular peripheries were represented by smaller hemispheric areas than larger ones, and that lesions of slight extent could consequently involve the lesser more entirely, and with proportionately greater functional damage, than the larger ones. This is partly also to be referred to another fact, that the layer of the cerebral cortex, adjacent to the associating fasciculi, is the first to suffer.

Before proceeding to analyze the intimate changes of the cortex and their probable relation to the mental symptoms, I would summarize the extent of my observations on the localization of certain motor disturbances.

In patients whose disturbance of articulation was extreme, and had been the preponderating element during life, I found the changes concentrated about the island of Reil, the operculum, Sylvian aspect of the temporal lobe, and the lenticular

nucleus. In this extensive area, I have not been able to local-
ize more narrowly. I drew this conclusion from three cases
in which the medulla and peduncular tracts were intact.

In four cases (one, however, not an example of the typical
affection), the paresis of the extremities, more particularly of
the lower, was most marked. Here a territory corresponding
to the upper extremities of the præcentral and postcentral
gyri, as well as the contiguous convolutions, exhibited the
most decided destructive lesions, with striking uniformity. I
do not on the strength of a few observations form the conclu-
sion that the centre for the extremities is here located, without
some reservation, but simply record this, to me, striking rela-
tion, in order that it may be compared or contrasted with the
observations of others; it is suggestive, however, when taken
in connection with the fact that the innermost fasciculus of
the middle portion of the internal capsule is connected with
the short gyrus connecting the two border convolutions of
Rolando's fissure, and the observation of Frey, and others, that
softening and other lesions, in the track of this bundle of
fibres, are associated with paralysis of the opposite arm or leg,
or both. It is also this fasciculus which is the first to have
its axis clylinders provided with a medullary sheath in the em-
bryo, (Flechsig). We believe that those tracts develope first
which are related to the first active periphery, and that the
movements of the extremities are the first performed by the
fœtus *in utero* to which a volitional element can be attributed,
may not be insignificant in this connection.

Two cases which manifested no marked motor paresis, but
well marked losses of certain recollections, exhibited de-
structive lesions of the second and third frontal gyri (first and
second of Ecker), in one of these, however, there were also
considerably affected areas in the occipital region. A third
interesting example was afforded by one patient, who, from
having been surgeon-dentist to Queen Victoria, was dismissed
the court because of some ill-timed interference in the Lady
Flora Hastings court scandal, became a general under Cavaig-
nac in 1848, thence went to Australia, to become incarcerated
for participating in a riot, and, coming to New York, was
once or twice a candidate for the coroner's office, actually re-

ceiving votes.   While he recollected many details of his
strange career, both of his early life, and the most recent
events, he had forgotten completely that he had been dismissed
from the English court.   He was a disciple of Mr. Henry
Bergh, and declaimed violently against the cruelty of Ma-
gendie's experiments, showing a good recollection of their
details which he had studied when a student at the University
of Edinburgh.   His brain exhibited lesions visible to the
naked eye, in the third frontal gyrus and the adjoining part of
the gyrus fornicatus.   Pathologically, this case was not a pure
one, for the whole peduncular tracts and spinal cord were the
seat of a change, which corresponds partly to Lockhart Clarke's
granular disintegration, partly to Arndt's observation of ex-
treme luetic affection of the cerebro-spinal vessels.   The
whole tract of the hypoglossal and facial nerves was the seat
of this disorganization, and his disturbance of articulation
was referred to this lesion, not to the hemispheric affection,
for neither his stock of words nor the construction of his sen-
tences had suffered.   It was merely the mechanical element
which was interfered with.

As to the closing scene of progressive paresis—if the pa-
tient lives long enough—the whole convexity of the hemispheres
becomes affected, mainly by a connective tissue hyperplasia,
and subsequent atrophy of the convolutions.   This corresponds
to the general and complete character of the terminal de-
mentia.   It should not be forgotten in this connection, that
some cases present from the very beginning, not special symp-
toms of obliviousness, or of motor paresis, but rather a general
dementia and general paresis, its progress being one of degree.
This is often the case with those whose symptoms are first
spinal, as for instance where progressive paresis follows loco-
motor ataxia, and in some examples of the luetic form.   Here
we find a general affection of the convexity from the very be-
ginning, when such cases reach the autopsy table through in-
tercurrent affections in early stages.

It will require accumulated observations of this kind to de-
termine the localization of marked and special symptoms, in
this and other forms of cerebral disease, and I have offered the
few deductions of localization of lesions in reference to dis-

tinct symptoms, not as so many unimpeachable facts, but be-
cause I think it better to hazard one rational conjecture, than
to chronicle a score of disconnected details.

I should add that in progressive paresis, the lesions are strik-
ingly symmetrical, with reference to both hemispheres, a fact
in complete accordance with their anatomical and functional
symmetry, and perhaps also with the observation of Arnold
and Oellacher, that the corpus callosum connects correspond-
ing convolutions of the opposite halves of the cerebrum.

There is a second group of symptoms, which without being
either mental or volitionally motor, may be referred to the
hemispheres. I refer to the so-called apoplectiform and epi-
leptiform attacks, as well as to peripheral lesions.

Apoplectiform attacks, occurring in those cases, accompanied
by that frequent complication, pachymeningitis, are most fre-
quently due to a meningeal hemorrhage, and are in no manner
different from other meningeal apoplexies. In a second class,
particularly older patients, or such as are luetic, true parenchy-
matous hemorrhage into the oval centre of Vieussens, or the
ganglia are found; occasionally softening is the lesion noticed.
But a third category presents neither meningeal nor cerebral
hemorrhage, nor softening, but merely an extreme condition
of the stasis, previously described; in some instances this
reaches almost the degree of a capillary apoplexy. Such a
condition suffices to produce all the symptoms of an apoplectic
attack, and I believe that it frequently precedes the true cere-
bral hemorrhages, acting as their exciting cause, by overstrain-
ing the weakened vascular walls.

In the last case, where I made an autopsy, a patient who
two weeks before death became rapidly, but gradually paraly-
tic on both sides, as well as comatose, shortly before death
was suddenly seized with a deeper coma, his face purple, and
breathing stertorous. I found a combination of hemorrhagic
and necrotic conditions, which is rather unique. An ex-
tensive bloody suffusion of the whole pia mater was the chief
morbid appearance of the convexity; at the base, an old organ-
ized extravasation, which had partly penetrated between the
pia and the medulla, partly into the arachnoid space, in which
it extended as far forward as the olfactory lobes. An enor-

mous extravasation was found in the left hemisphere, extending from between the thalamus and corpus striatum, upward and to both extremities of the hemisphere, so as to leave but a thin shell of cerebral substance, on the parietal and frontal lobes. The blood was mostly fluid, and the few clots present were soft and maroon colored; it was evidently a recent extravasation, and had distended the basal ganglia in the left lateral ventricle so as to obliterate the furrow which normally separates them. In addition three considerable depôts of softening were found, one in the apex of the left temporal, and one in each occipital lobe. Microscopical examination showed, besides the usual degenerative appearances, stasis, undergoing resolution, of all the cerebral capillaries.

On the whole, we may say, that many of the apoplectiform attacks, do not differ from the true apoplexies occurring in the sane, and that they take place more frequently, because vascular changes and frequent vascular overstrain, the predisposing and exciting causes of cerebral hemorrhage, more often co-exist in progressive paresis, than among the other classes. Of the relation of vascular stasis pure and simple to pseudo-apoplectic attacks, frequently occurring in rapid succession, we have already spoken.

Before proceeding to the analysis of the epileptiform attacks, I would insist that these have nothing in common with the convulsive attacks of true epilepsy, except as regards the clonic convulsions, and even these do not correspond to the outbursts of typical epilepsy. In their reaction to nitrite of amyl, the two exhibit a fundamental difference. Crichton Browne first found that paretics are less susceptible than normal subjects to the influence of this "vascular neurotic," while epileptics are exceedingly rapidly affected by much smaller doses than will influence a healthy person. Correspondingly we find that if given under proper conditions, nitrite of amyl will abort a threatened epileptic onset, more surely than any other drug, while over the convulsions it exerts no good influence, on the contrary I should suspect it of having a bad one. This is to be explained by the opposite vascular condition obtaining in the two, in epilepsy the attack begins with a cerebral anæmia,

which is counteracted by the amyl nitrite, which causes a decided hyperæmia; in the paretic's convulsive attack we have a hyperæmia to start with and this, nitrite of amyl could only increase. If any medicine could be tried with an expectation of some success here, I should judge that drug to be ergotine. Meynert and Wedl have recently found the same arterial stasis affecting the cornu ammonis of epileptics which is found over the whole cortex of paretics, as well as in the cornu ammonis, and leads to degenerative changes in this territory. It is curious that the stasis of the cornu ammonis, should be arterial in epilepsy, especially when we find the remainder of the encephalon in an intense condition of venous congestion after the epileptic coma, and I think the clew to the nature of these attacks of true epilepsy and progressive paresis is here given.

In the *haut mal* we have a general cerebral anæmia resulting from the tonic spasm of the cerebral arteries, due to a hyperæsthesia of the vascular centres. Now there is one part of the cerebral cortex which is very favorably situated and constructed as it were for a collateral hyperæmia, this is just the cornu ammonis, which unlike any other cortical territory lies at the floor of the lateral ventricle, and possesses an anastomotic network of vessels in the stratum lacunosum. In contradistinction to the functional abolition of the remainder of the cortex, we here have a functional stimulus set up, which travelling down the fornix to the thalamus, determines the partly co-ordinated character of the convulsions. That the cornu ammonis is a motor territory I am satisfied on grounds of comparative anatomy and experiment. I will only mention here in passing, that this S-shaped involution of the cortex is proportionately better developed in the bat, an animal whose natural motions have the rapidity of epileptic convulsions, and in smaller Rodentia than in other classes.

In progressive paresis, this same hyperæmia in a degenerating territory, produces much feebler convulsions, while it is not impossible that in other cortical territories it may cause convulsions of individual muscular groups. That we have no total loss of consciousness as in epilepsy, has been recorded by every good observer, and I can, from a less extended experience, only confirm their statement. The condition of con-

sciousness, or even of excitability, which precedes the
convulsion, passes uninterruptedly and progressively into
stupor, this increasing with the progress of the convulsive
attack, and extending beyond it; while in the epileptic attack
we have: first, complete consciousness and aura; second, com-
plete unconsciousness with convulsions; thirdly, complete
coma, with occasional re-awakenings.   With a radical differ-
ence between the vascular conditions in the two affections, the
one being a vaso-motor spasm, the other a vaso-motor paralysis,
we have an agreement as to the locality affected, in both it is
as we have said the cornu ammonis.   My observations in this
respect are quite in accord with those of Meynert, and I can
add the further observation that, in three cases presenting no
convulsive attacks at all, the cornu ammonis was decidedly
healthy, as compared with the same region in paretics who had
convulsions.

Of peripheral organs affected, one of the most interesting is
the retina.

The optic papilla is most constantly affected in that form of
paralytic insanity, which complicates locomotor ataxia; in a
case of this kind remarkable for the youth of the patient, who
was only twenty-three, I found beginning atrophy of both
discs.   In an old standing case of genuine paresis, whose luetic
nature could not, however, be excluded with certainty, there
was sclerosis of the temporal side of the optic papilla, and a
corresponding defect of vision in the right eye, while the left
appeared healthy.   A third case of advanced paresis, the dis-
tant result of an injury received at the battle of Bull Run,
exhibited no retinal change whatsoever, while a fourth who
began as a case of melancholia, with delusions of dread and
persecution, and now presents decided paresis, general hyper-
algia, tinnitus aurium and photopsia, had a markedly hyperæmic
optic papilla.   These four are selected cases, chosen to exhibit
any possible relation between the retinal lesion and the symp-
toms; as could be anticipated, no such relation could be found, and
where, as in the last case, delusions were founded on the pho-
topsia, which, in its turn, was the result of a retinal hyper-
æmia—this hyperæmia had no more essential a relation to the
delusions than circumstances originating outside of the patient's

body have, which equally serve to tinge his morbid ideas. To those who are accustomed to think physiologically it would be unnecessary to insist on this. I dwell on this point, rather because I have been informed that a few years ago an ophthalmologist went to one of our insane asylums to look for hallucinations and delusions in the retina! Which side of the mirror the hallucinations were on, in this case, I need not say.

My examinations of the optic nerve itself have been few, and consequently are not conclusive; but on looking over my records of autopsies, I am struck by the frequent observation of unusually firm and small optic tracts, chiasm and nerve. In one case I found numerous corpora amylacea scattered through these, and in a second, a decided and extreme hyperplasia of the perineurium and neuroglia, with atrophy of the axis cylinders and sheaths. I would not always refer the retinal affection to a descending neuritis or secondary atrophy, but hold it possible that the similarity of the retinal to the cerebral elements predisposes them to the same changes, the same engorgements and sclerosis. Another element in their causation may be the increased intracranial pressure, marked by the increase of the arachnoid fluid, and the chronic meningitis found in ninety-nine paretics out of a hundred. Perhaps the frequent co-existence of the luetic dyscrasia is not without some influence here.

One reason why the retina of paretics will never receive the attention it deserves, is that the fatality of the original affection renders it a matter of slight importance to diagnosticate an incurable affection. I will merely state that I have never met with complete amblyopia in a paretic, and that I consider Clifford Albutt's assertion, that where graver changes are absent, at least a hyperæmic blush of the papilla exists, as inaccurate.

Much more important, in a prognostic direction, are trophic disturbances of various distant organs. There is a great frequency of gangrene of the lungs, and of fragility of the bones, particularly the ribs, among paretics. These, as well as numerous affections of the skin, several of which are quite different from those usually encountered by dermatologists, and, I think, can be referred to that group of affections which Brown-

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Séquard, Ollivier and Dupuy have described as secondary to hemispheric lesions. Eulenburg has recently shown that trophic disturbances of various kinds may be induced by destroying various cortical areas in animals, and we certainly have a complete parallel to these experiments in the destructive lesions of the cortex in paresis. It is not necessary to refer these peripheral complications to the sympathetic ganglia! Poincaré and Bonnet have endeavored to locate the original lesion of the disease here, but they have not been confirmed. I have found pigmentation of the inferior cervical ganglion in one case, but not in others, and I question whether this change was pathological.

And now as to the disturbance of the delicate associations on which the incoherence and the delusions of grandeur *must* depend.

The cerebral cortex contains, besides the granular elements, whose nervous nature is disputed, pyramidal and spindle-shaped nerve cells. We can partly draw a parallel between the ganglion cells of the retina and those of the cortex. As in the former, a number of individual elements act synchronously as receptive units, when subjected to a luminous impression, so we may suppose the cortical nerve cell to be an impressible organism, and to be associated with its fellows in the reception of appropriate impressions. But there is this difference between the two cases, while the retinal cell is subjected to repeated impressions which successively blot each other, and no permanent registration takes place, we have in the six hundred million ganglion cells of the hemispheres a sufficient number of registering groups to store away all the impressions an individual receives in his life time, and as one group of cells is subservient to one impression alone, the permanency of our recollections is explained. That the registration of impressions can lie in the ganglion cell, we can readily prove by the phenomenon of the retinal after-image. This is a rudimentary remembrance, as it were; it is true that it lasts but a short period, but how unfavorably is the retina organized in this respect as compared with the cortex?

Meynert draws the beautiful parallel that a sensory surface like the retina, receives successive impressions, and transmits

them to the hemispheric cortex for recollection, just as a camera obscura receives successive transitory images, and these are transferred to the different leaves of a sketch-book for permanent preservation by the artist's pencil.

Now let us suppose that we have received an impression by means of the retina, and another by the ear, from the same source, let us suppose the retinal impression to be a cat, the auditory perception, the peculiar cry of the animal. The two impressions are associated, and the shape and voice of the animal constitute a combined conception of the simplest kind. On the next occasion, when we hear a cat without seeing it at all, we have a mental image of its shape, because the auditory perception centre, being in an organic connection with the visual, by an associating fasciculus recalled the latter into action.

We may carry this example further, and substitute more complicated ideas for each of these simpler impressions, then we will be able to make out our case for progressive paresis.

The chief intellectual phenomenon of the paretic (during the quiet interval, in which the real character of the disease can be best studied) is a loss of his proper sense of self-consciousness,*—his sense of self-consciousness depends on his proper appreciation of his surroundings,—the appreciation of his surroundings depends on the association of numerous impressions which have acted on him during his life-time as the teachings of experience and instructors,—and these impressions have become either destroyed, or if they exist, are not associated. To this there is a pathological parallel. At the apex of the convolutions the vascular channels are more free, and the venous, as well as the lymphatic outflows are less apt to suffer obstruction than at the depth of the sulci, for here, as the vessels have to dip deeply between the convolutions, and dense meningeal exudations chiefly occur here, the compressible emunctories suffer. This serves to intensify the atrophic

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*This involves an apparent, but only an apparent, contradiction, some might suppose the delusions of grandeur to correspond to an increase of self-consciousness. A minute's reflection will serve to show that a loss of the relations to himself and surroundings, which alone permit the existence of such delusions, are deductions from, not additions to, the paretic's mental life.

and exudative processes in that district whose emunctories are blocked up. Now, it is precisely at the base of the sulci that the associating fasciculi curve around to join neighboring convolutions; this connection being interfered with, the different individual impressions may co-exist, but they are not associated; they are disconnected factors. In short, in the destructive lesions of associating apparatus, we perceive the stamp of incoherence.

It is not always this coarser association that is affected; usually the fifth (eighth in the occipital lobe) layer of gray matter is the first to suffer, and it is here that the sclerotic patches described by Meschede, the miliary sclerosis of Tuke and a curious lesion observed by myself, are located. This layer is made up of spindle cells, which are not connected with the projection fibres, but with the associating band at their poles; it is reasonable to suppose that they represent the aggregate compound of two impressions, and are the seat of ideas abstracted from the impressions on which these ideas were originally built up. We can here perceive why the more intricate mental processes (mental co-ordinations), like the more delicate motor co-ordinations, should become impossible, while the simple registration of impressions, and the recollection of isolated facts, should still be well-nigh perfect. Changes occur in the spindle cells, both in this disease and in chronic mania, much earlier than is usually supposed. Atrophies of their slender bodies cannot be determined very accurately, but on counting the number of these bodies in a paretic's cortex, and comparing the result with the healthy average, they will be found diminished.

It is on the loss of proper self-consciousness that many of the so-called erratic actions of paretics depend; they lose the power of comparison, and consequently create vast projects (never very ingenious), without calculating their means of carrying them out. They commit a hundred actions which might well be characterized as infantile or boyish. Their tendency to lie, for some of their apparent delusions, are nothing but lies with which they finally deceive themselves, is due to that deficient judgment which a loss of associating power always entails.

Beside loss of associating mechanisms, deficiencies of whole recollections take place, with which cortical areas are found destructively involved in their whole thickness. This explains their forgetfulness of certain events, which we have noticed, and also the fact that such oblivion may involve both long past and very recent recollections. In one case, where an area of the cortex was so much destroyed, that the destruction became visible even to the naked eye, as you see in the plate I have caused to be passed around, the association and projection fasciculi were the seat of a molecular degeneration, which I take to be a secondary process, analogous to the changes described by Tuerck. It seems that the association fibres decay when the centres which they serve to connect are destroyed.

Here is a field for speculation which I shall content myself with having touched on, for fear of encroaching on the evening.

One word as to the general results of our observations, as compared with the observations of others, and from which the following deductions can be drawn:

1. Derangements of voluntary motion, and derangements of the intellect, both depend on changes of the cerebral cortex, and they should always be considered together, pathologically and clinically, just as in the combination, for example, of ataxic and amnesic aphasia.

2. Morbid conditions of the cerebral cortex are of two kinds—the first depends merely on acceleration or retardation of biochemical processes in the nerve cell and nerve fibre, dependant on anomalies of the vascular current; as a clinical result we have maniacal excitement or melancholic atonicity It is not yet definitely settled under what concrete conditions we will have one or the other of these manifestations, but from their dependence on essentially evanescent conditions it is readily seen that mania and melancholia are only symptomatic terms. The symptoms of the first stage of progressive paresis belong, in great part, to this group. The second class of morbid conditions depends on processes which are destructive, permanent and irreparable. It is indifferent what the character of the change may be, its results depend on the region affected.

3. It is possible for marked paresis, even to complete paraparesis, to occur without any spinal lesion whatever. A spinal lesion is neither essential to, nor characteristic of the typical disease. Where it does occur in the typical affection, it is a secondary process, not a pronounced lesion, it is an atrophy dependant partly on the long continued disuse of the motor tracts, partly on the increased lateral pressure, exerted by the accumulating cerebro-spinal fluid. We know since Magendie's time that the spinal arachnoid space, being capacious, and surrounded by compressible walls (venous plexus), acts as a safety-valve to encephalic pressure. But with the frequent repetition of the fluxions causing the latter, the safety-valve itself suffers, and with it the nutrition of the cord. Where the spinal lesion is in the posterior columns, we have a complication to deal with, as has been specified. The fact that in the earlier stages of progressive paresis no changes in the electro-muscular contractility, and no anaesthesia occur, proves that the paresis is a true paresis of the will-power, and is not of a spinal nature. An affection of the medulla oblongata and the mesencephalon, may take place quite early. Oculomotor ataxia, facial and hypoglossal paresis, must occasionally be referred to the central tubular gray matter, not to the cortex.

4. The incoherence in articulation and in the construction of sentences, their stopping in the middle of one sentence to pick up another at the wrong end, must be referred to the most elaborate associating system we possess, namely the *claustrum.*

I have refrained from detailing the lesions affecting these localities, histologically, since I may have to refer to them in connection with the specimens exhibited. I would simply state, that many authors have described as lesions, appearances found in every individual who has passed his twentieth year, and others have registered and even photographed the results of the reaction of absolute alcohol, (leucin and cholestearin precipitates) as lesions characteristic of progressive paresis. My results, with the exceptions to be detailed, are mainly in accordance with those obtained by Rokitansky, Westphal, Meynert, Adler, Lubimoff, Schuele, Magnan, Mierzejewski, and Herbert Major.

a. The so-called colloid bodies are not strictly to be so termed; they are evidently a fusion of several organic compounds, among which lecithin is probably one.

b. Fat or oil globules, I have never found.

c. Amyloid degeneration of the vessels occurs in the luetic form alone, as far as my experience goes.

d. There is no correspondence between the preponderance of melancholic symptoms and the development of protagon spheres. (Schuele.)

e. Pigmentation of the cortical nerve cell, where diffuse, can not be unquestionably interpreted as abnormal, since the cells of the substantia nigra and locus coeruleus are always pigmented. But pigment clumps in a nerve cell are decidedly pathological. These do not constantly occur.

f. Miliary aneurisms, fusiform and dissecting, must be shown to have a definite relation to the symptoms of the disease, before they can be considered of any importance. I am convinced that where the investigator has found them very frequently, he has caused their artificial production by pursuing improper methods. I have not found a single clear appearance of this kind which would stand all tests. Besides they occur in those who never have been insane, as a predisposing cause of apoplexy.

g. There is often an apparent disproportion between the gravity of the mental symptoms and the changes in the ganglion cells, which appear to be intact. In such cases it is the rank growth of the connective tissue, which with Rindfleisch, we may consider as having rendered the delicate associating and projecting machinery of thought useless.

With regard to the frequency of the perivascular dilatation, it, with several affections of the tutamina cerebri, is an important etiological factor. We know that the existing causes of progressive paresis are all such as will cause vascular determination and retarded lymph-outflow. The ill-supported vessels can no longer be held in tone by the vascular centre, this finally breaks down by sheer exhaustion. We correspondingly find that, in accordance with the resulting engorgement, the disease begins with a condition of excitement, not the melancholia, which Guislain claimed for it more in accordance with

the formulas which he followed, than with the clinical facts.

We have thus traced clinical groups of symptoms, which are constantly associated with complicated central changes to these as their origin, I have been more or less theoretical in my deductions, it must be recollected that the nature of the subject forbids any other method of treatment.

Although psychological pathology, the highest branch of neurological inquiry, is still in its earliest infancy, and all statements as to its possible future degree of perfection, are therefore premature, we may make one prediction with tolerable confidence: That a palpable proof of the exact manner in which disorder of the higher elements of the nervous system determines aberrations of thought and of action, will forever lie beyond the pale of human demonstrative power!

Still it would be an excessive conservatism, which resting satisfied with this discouraging conviction, would forbid further investigation in our field because it is partly speculative.

By logically combining the results of anatomical, experimental and embryological research, we are enabled to offer an explanation for the phenomena of the mind, which without being comparable to a mathematical solution, is yet something more than a new theory. For an explanation has passed out of the realm of the wholly hypothetical as soon as the contingent probabilities in its favor, become as numerous and powerful as they have become in the case of Meynert and Wundt's propositions.

Finally as an apology, for having approached the vast and problematical subject of the mind, vast and problematical even in its crippled state, I may venture the suggestion, that if it is worth while studying insanity at all, it is worth while studying it in accordance with those branches without which no specialty is a scientific study, namely, Anatomy and Physology.

Art. IV.—THE PATHOLOGY AND MORBID HIS-TOLOGY OF CHRONIC INSANITY.

By Edward C. Mann, M. D.,

Late Medical Superintendent State Emigrant Island Asylum, Ward's Island, New York

(Read before the New York Neurological Society, March 5th 1877.)

The morbid histological changes occurring in insanity are, at the present day, undergoing microscopical investigation at the hands of many very skillful observers, both in our own country and in Europe, and these assume great importance when we reflect upon the fact that the pathological phenomena discovered in the brains of persons dying insane, all have for their basis, interference with the due nutrition, growth and renovation of the brain-cell, which, by interrupting the nutrition, stimulation, and repose of the brain, essential to mental health, results in the impress of a pathological state in the brain and disordered mental function. The investigation of both the normal and the morbid histology of the brain is a work requiring great labor, patience and perseverance, and also great judgment in the recording of observations, and, even with the most careful microscopists, mistakes may be made as to the nature and value of appearance met with in histological research. The naked-eye appearances which may be met with in the bodies of those dying insane are chiefly peculiarities in the form of the cranium, of which the most frequent is want of symmetry between the two sides; the shrunken and shriveled ear in chronic insanity, consequent upon hæmatoma auris; variations from the normal standard in the thickness or thinness of the cranium; changes in the membranes as to appearance and structure, and finally changes in the cerebral substance itself. In chronic insanity, the changes chiefly met with in the brain, have been, atrophy of the convolutions and brain itself; induration of both white

and grey matter; thickening and opacity of the membranes; chronic hydrocephalus; effusions into the sub-arachnoid space; pigmentation of the cortical substance, and extended and profound sclerosis of the brain. The pia mater is found to be thickened and adhesive to the brain, and its vessels tortuous and thickened in their walls. Atheromatous and fatty degeneration of the walls of the cerebral capillaries have also been noticed.

The pathology of epileptic insanity, which in cases under my care has generally been associated with chronic insanity or dementia, is a very interesting subject; one deserving of far more room and attention than is here devoted to it. I propose to merely briefly allude to it, reserving a more lengthy discussion of this very important subject for a future time. The pathology of the production of epilepsy occurring or associated with chronic insanity or dementia, consists, I think, primarily, in interference with the proper nutrition of the cerebral tissue of the fœtus, so that even during embryonic life, the brain of the infant undergoes pathological changes which induce deficient moral power, mental weakness and epilepsy, there being an ill-balanced and defective state of the whole central nervous system, predisposed to take on diseased action. The portion of the the nervous system mainly affected by, or more strictly speaking, the portion which is the primary seat of epilepsy associated with chronic insanity or dementia, I believe to be the medulla oblongata, the corpus striatum, and the motor tract of the cervical region of the spinal cord and the cerebellum. From the fact that I have by means of the fluid extr. ergot, continued for some time, decreased the paroxysms of epileptic mania both in frequency and intensity, so that the maniacal excitement has entirely disappeared in many instances, the pulse and temperature becoming normal, I have inferred that the cause of the epilepsy in these cases was cerebral hyperæmia of the nervous centres, and that the disturbance was partly at least functional in character. On the other hand, in epilepsy, associated with chronic mania and dementia, I have never had anything but negative results from the use of ergot, and I have therefore inferred that the state of the nervous centres was one of organic degeneration, induration and anæmia. Examina-

tion of the urine in the former cases revealed an excessive elimination of the phosphates, while in the latter cases associated with chronic insanity the elimination of phosphates was below the average. The reaction of the urine in these latter cases was acid. In speaking of the epilepsy of chronic insanity as dependent upon anæmia, I of course recognize the fact that the clonic spasms of the second stage of the epileptic paroxysm, during which the alternate powerful muscular contractions occur, the pupils oscillating, the pulse being full, and palpitation of the heart occurring, are accompanied by notable cerebral congestion. I have only had the opportunity of making one post mortem examination in a case of chronic insanity associated with epilepsy. The case was one of chronic mania in a German, in the course of which epileptic paroxysms occurred. At times the maniacal excitement preceding the paroxysm would be such as to require mechanical restraint. The patient had fits very frequently and after a short time had them daily, and they increased both in frequency and intensity in spite of all medication. The patient finally died in a paroxysm after a succession of fits, lasting thirty-six hours. The post mortem examination revealed thickening of the membranes of the brain; the arachnoid opaque, and the pia mater much thickened. The brain was atrophied and indurated, and the lateral ventricles were filled with fluid. The cord was normal in appearance. The brain presented an anæmic appearance. Upon hardening the brain and medulla oblongata, and examining microscopically, there was a thickened and enlarged condition of the capillary vessels of the medulla, and some vascularity in the fourth ventricle extending through the medulla. In the larger nerve cells of the deeper layers of the convolutions of the brain, which I consider analogous in their functional activities to the ganglion cells of the anterior cornua of the cord, was seen pigmentation, atrophy, and degeneration. On examination of the cells of the grey matter of the corpora striata, there was found degeneration and atrophy.

Having devoted considerable time to the investigation of both the normal and morbid histology of the brain, I desire to call attention to an appearance which I have

noticed in the brains of those dying insane, and to which my attention has been drawn from the interest it assumes when viewed in the light of a possible ultimate cause of the nutritive defect which results in chronic insanity. We know, that for the proper nutrition and healthy functional activity of the brain-cell, the proper nutrient supply is required, and that we cannot have healthy mental function without a due supply of healthy blood to normal and healthy brain substance. We also know, that if any agent operates in the influencing of the circulation unfavorably, so that a morbid condition of the cerebral capillaries be induced, we shall inevitably have resulting morbid changes, set up and maintained in the cerebral cells.

In my writings on insanity, I have called attention to the fact, that a microscopical examination of blood from insane patients, as compared with an examination of blood from the same number of healthy individuals, revealed in the blood of the insane a marked increase in the number of white corpuscles. In making microscopical examinations of brain tissue from chronic insanity, I have noticed repeatedly in different cases lymphoid cells, or white blood corpuscles, and also red corpuscles in small numbers, in the substance of the brain tissue, evidently having emigrated from the blood-vessels. From what I have observed, I think that under conditions of inflammatory irritation of the brain, an emigration of lymphoid cells takes place on a large scale, the cells, or corpuscles, by virtue of their vital contractility passing through the walls of the vessels and penetrating into the brain tissue. It will be remembered that both Dr. Bastian and Dr. Blandford have noticed a plugging up of the blood vessels by small embolic masses composed of aggregations of white corpuscles in insanity. Ecker found that the vessels of the gray matter were generally dilated in insanity, and Raman also noticed the same thing in the vessels of the pia mater, while Dr. Major has described a dilatation of the arteries in "brain wasting," a condition which appertains to chronic insanity.

anything b here two factors, which operate, I think, in the therefore inf the appearance in the brain of the lymphoid of organ in some cases of the red corpuscles. First, the undue

predominance and accumulation in the blood vessels of the white corpuscles, which obstruct the capillaries, as they move so much more slowly than the red corpuscles, giving us as a result an impeded circulation and an increased pressure on the coats of the vessels; and second, the dilatation of the vessels before alluded to. These two conditions are favorable to the rapid emigration of the white, and also the red corpuscles through the walls of the vessels, and also, perhaps, the same condition may be produced at times by the obstruction in the capillary vessels becoming great enough to rupture them, permitting in this way the escape of a few blood globules into the brain tissue. (Such a condition would be analogous to what, I think, often occurs in the lungs, as I endeavored to explain in a paper on consumption, published in the New York Medical Record of September 18, 1875.) Such lymphoid cells would act probably as foreign bodies, and a slow course of inflammation would be likely to be set up to get rid of the intruders. Such an inflammatory process would naturally be of slight intensity and long duration, and these collections of lymphoid cells would tend to become developed into a fibroid structure, resulting in induration of the brain, such as we find in chronic insanity. I am also forcibly impressed with the idea that, if I am correct in my conclusions, we have here the solution of the problem as to the relation which exists between tuberculosis and insanity. Dr. Clouston, in the Journal of Mental Science, for April, 1863, showed that of 282 patients who died with tubercular disease at the Royal Edinburgh Asylum, 153 passed rapidly into the state of chronic insanity, the acute stage being of very short duration, the patients all manifesting a decided tendency towards chronicity. He also noticed that the prognosis relating to mental recovery was eminently unfavorable, and that apparent recoveries proved to be only remissions. In these cases, where the development of the two diseases seemed to be nearly contemporaneous, there was probably, owing to a want of proper hygienic and sanitary surroundings; want of sunlight, fresh air and exercise, this undue development of the white blood corpuscles and the tuberculosis was very likely induced by the escape of some of these lymphoid cells into the meshes of the connective tissue

of the lungs where they became the nucleus of tubercular de-
posit.   Again, these cases may have been the result of too
little blood being sent to the lungs as a result of want of proper
exercise, and close confinement at business or trades; the upper
part or apices of the lungs being allowed gradually to collapse
and tubercles appearing as the circulation ceased to affect
them.   I think this occurs more often than we are aware of,
especially in persons who inherit the predisposing, neurotic
element.   That there exists such an hereditary, neurotic or
morbific element or force, present in both insanity and phthisis,
I most firmly believe, and I also believe that there is a correla-
tion of morbific force which renders these diseases mutually
convertible.   I have repeatedly seen this borne out by unde-
niable facts, children of one family being affected with both
insanity and phthisis in many different instances.   I have no
doubt other observers have also witnessed the same thing fre-
quently.   I have also come to believe that skin disease, in
many of its forms, may be included in this same class of
mutually convertible diseases, as I have had occasion to notice
several times, psoriasis, herpes and eczema alternating with
attacks of insanity.   To return more immediately to our sub-
ject.—respecting the dilatation of the vessels which I before
alluded to, it appears to be probable that the general obstruc-
tion in the capillaries of the brain, causes primarily a compen-
satory hyperæmia, and as this gradually becomes permanent,
the small arteries would naturally become enlarged, as they
have been found to be by Ecker and Dr. Major, and their
walls would become thickened, as we find them to be, *post
mortem*, in chronic insanity.   Such long continued mechanical
hyperæmia causes an impairment of vitality and function, and
this we find exemplified by the retrogressive changes which
occur in the substance of the brain in chronic insanity, viz.:
atrophy, induration and degeneration of the nervous elements
of the brain, with a resulting dynamical state of loss of psych-
ical activity and profound physical and mental weakness, with
the exception of cases of apoplexy in which large clots have been
discovered, *post mortem*. I am not aware that any observer
has described any such lymphoid deposit in the brain, which
may, or may not, have undergone fibroid metamorphosis or

degeneration. I think, therefore, that from both a physiological and pathological standpoint, these observations (if confirmed by further researches) become of the highest clinical significance.

I desire not to be misapprehended as regarding the presence of the lymphoid deposits in the brain as the ultimate cause of insanity. I *do* however, think, that by their presence we are enabled to explain many of the changes incident upon chronic insanity and think their presence must affect, very materially, the ultimate molecular changes in the brain, upon which, functional activity depends, and regard it as a very strong probability that such foreign deposits in the brain, may, by interfering with the molecular changes just alluded to, destroy both functional excitability and activity. It would appear very probable the prominent alterations taking place in chronic insanity; viz., atrophy of the convolutions and of the brain itself and induration of the two substances, with degeneration and atrophy of nerve cells may be considered, fairly, to depend upon this abnormal state in the mutual relationship between the blood and the tissues, which becomes the ultimate cause of the nutritive defect, which results in chronic insanity.*

Clinical Cases illustrating the Pathology and morbid Histology of Chronic Insanity. Case I.—*Dementia and Paresis.* Death resulting from pulmonary hemorrhage. T. A., male, aged 22, single; occupation, wagon maker. Admitted to asylum June 24th, 1874. Upon admission was demented with symptoms of paresis; laughed vacantly when addressed and stared unmeaningly about him. No appreciation of condition or surroundings. His gait was staggering, and lips and tongue were affected with muscular tremors. He never spoke but once and that was upon the occasion of a visit from his brother. His speech at that time was hesitating and trembling. He had an attack of sub-acute meningitis in October, and died in January from an exhausting hemorrhage

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*The best results in microscopical examinations of the brain tissue, can be obtained by immersing the brain immediately after removal in the following:

R.—Bichromate Ammonia,	160 grs.	
Methyl Alcohol,	10 oz.	
Distilled Water,	30 oz.	

M. To remain until hardened.

from the lungs. *Post-mortem*: Upon removing the calvarium the membranes were adherent to the skull; subarachnoid effusion existed; also large effusion between the pia mater and the brain; the pia mater was thickened in patches. There was effusion at the base of the brain, fluid in the spinal canal and the spinal cord was atrophied. There was miliary tuberculosis throughout the brain. Upon making an examination of the chest, the left lung was found to be partially destroyed by the breaking down of the caseous products of pneumonia as a result of which, large cavities were formed. The heart gave evidence of recent endocarditis. The surface of the heart and endocardium were overed with miliary tubercles. The walls of the heart were atrophied and exhibited traces of fatty degeneration. The kidney, spleen and liver normal. Upon hardening the spinal cord, making sections, and employing carmine-staining, there was found to be, upon microscopical examination, atrophy and degeneration of the nerve elements of the posterior columns, with increase of connective-tissue. Sections of hardened brain-tissue being made, there was observable in the cerebral cells of the frontal convolutions a diffused granular degeneration. No change could be discovered in the cells of the cervical sympathetic, which was carefully examined. Lymphoid cells and white corpuscles were found in aggregated masses in the brain having become developed into fibroid structure causing shrinking and induration.

Case II.—M. A. R., female, aged 29, single, occupation, servant. Admitted to asylum Dec. 29th, 1873. Form of insanity, dementia ending in paresis. The speech was slurring and hesitating; the gait was staggering and the mental faculties very much enfeebled; would become very angry at trifling incidents and then would relapse into silence, which lasted sometimes for weeks. She suffered from gradually progressing paralysis which involved the sphincters of the rectum and bladder. The cutaneous and muscular sensibility was impaired and there was likewise loss of electro-muscular contractility, so that disease of the antero-lateral and posterior columns of the spinal cord was diagnosticated before death. The paresis was attributed to spinal injury when quite young. She died from exhaustion, March 24th, 1874. *Post-mortem*: The dura

mater was firmly adherent to the cranium; the pia mater was thickened and infiltrated and the arachnoid thickened and opaque. The convolutions of the brain were atrophied and the brain-substance indurated, and throughout the brain were collections of altered white corpuscles. There was fluid in the spinal canal, and the cord was atrophied and softened in patches; the heart was small and flabby; spleen atrophied; stomach, liver and kidneys, normal; the uterus was in a rudimentary condition, apparently never having been developed properly. The spinal cord after being hardened and sections being made, revealed upon microscopical examination, loss of neuroglia and connective tissue and degeneration of posterior columns and loss of nerve tubules of white substance; the ganglion cells of both anterior and posterior cornua were disintegrated and atrophied, and granular and fatty matter occupied their place.

Case III.—M. P., a female, aged 23 years, was admitted to the asylum Sept. 30th, 1873. Upon admission was depressed and melancholy and in a delicate state of health. She became gradually demented and paretic. Physical exploration of the chest revealed pulmonary tuberculosis, with cavities at the apices of both lungs. Patient died from exhaustion from paresis and tuberculosis July 18th, 1875. *Post mortem*: Brain anæmic, atrophied and indurated. The spinal cord was about of a normal size, its membranes were thickened and the pia mater thickened and opaque. The lung presented extensive disease; the heart was small and flabby and the kidneys atrophied and anæmic. Upon hardening the cord, the posterior columns together with the posterior section of the lateral column were found to be affected. The posterior columns presented atrophy and disintegration of nerve elements and plates of connective tissue in different places. In the posterior lateral columns were granular and fatty corpuscles and new connective tissue.

Case IV.—J. W., male, aged 27; occupation, student; admitted to asylum, April 30th, 1873 with acute mania. Upon admission was violent, requiring the restraint of a camisole. As soon as the mania subsided, dementia supervened, and he became gradually paralyzed. His mental faculties seemed entirely lost. He did not speak; required to be dressed and un-

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dressed, and put to bed like a child, and led to the table for his meals, which he took from a spoon which had to be put in his mouth by an attendant. On the morning of March 27, 1875, he became suddenly comatose and died in a short time. *Post mortem*: The dura mater adherent to skull; arachnoid opaque and thickened; pia mater thickened and infiltrated and the blood-vessels enlarged and varicose. A varicose vessel had ruptured, giving rise to extensive hemorrhage which pressed upon both hemispheres causing death. The brain was anæmic, atrophied and indurated. There was effusion at the of the brain and in the lateral ventricle. Upon examination the lungs were found diseased, a large cavity being found at the apex of the left lung; the stomach, liver, heart and spleen were normal. The kidneys were hypertrophied and undergoing fatty degeneration. Many more cases might be cited but at the expense, I am afraid, of your patience, and those which have been inserted are typical of chronic insanity.

Art. V.—CASES OF INJURY OF THE BRAIN, INVOLVING SPEECH.

By P. R. Hoy, M. D., Racine, Wis.

IN October, 1842, I was called to see James Lawson, aged 18, a resident of New Haven, Ohio.

I found him comatose. There was a fracture of the skull, occupying the anterior superior angle of the left parietal bone, caused by the kick of a horse. He was insensible during the operation of trephining and removing fragments of bone, which left an opening as large as a half dollar. Soon after the operation he fully recovered his senses.

On the morning of the third day I found the patient comatose. I cut the stitches and opened the wound, when there escaped a clot of blood and a small quantity of bloody serum.

As soon as this cause of pressure was removed he became not only conscious, but was able to converse freely. I exposed the brain in order to ascertain the source of the hæmorrhage. As the dura mater was not injured, it occurred to me that the case afforded a good opportunity for experiment.

When the patient was asked a question that involved a considerable degree of mental effort, there was an increased motion of the brain, accompanied by a decided increase of the flow of blood in the small vessels of the brain, which congestion gradually subsided as soon as the mind came to rest. I next asked him a question, and immediately made firm pressure, with my thumb, on the exposed brain. As long as the pressure was continued he remained silent, but the instant I lifted my finger, he would reply, never suspecting that he had not answered at once. These experiments were repeated again and again with precisely the same results.

For fear that I might be censured for exposing the patient's life in my young zeal for science, I will state that he recovered without another bad symptom, and is still living, a healthy man.

Dr. Thos. Johnson and Dr. Orlando Steward, of New Haven, Ohio, were present at the operation.

Dr. Johnson remarked that he had always supposed the mind was the immortal part of man—the soul, but these experiments showed the brain to be a machine for the manufacture of thoughts, for in this case you could control the boy's thoughts, stop and start the brain, as an engineer his locomotive.

August, 1856, John Loftus, an English boy, age 19, was working in Mount Pleasant, Racine county, Wisconsin. He was ordered to ride the mare, Dolly, to town, and leave her colt in the stable. While struggling with the colt the mare kicked him on the head.

Those who witnessed the accident supposed him to be killed, but finding that he soon began to breathe, sent for a surgeon.

I found a fracture and depression at the superior border of the os frontis, a little to the left of the mesial line. Dr. S. W. Wilson assisted in the operation. As soon as the depressed

portion of the bone was removed, the patient sang out,
"Whoa, Dolly," with great energy, and then stared about him
in amazement, and demanded, "where is the mare, where am
I, what has happened?" Three hours had elapsed since the
accident, and yet the word that he was just about to utter re-
mained locked up by the pressure, to be articulated the mo-
ment that pressure was removed. Loftus had a good recovery.
He was not conscious that Dolly had kicked, but recollected
that she wheeled around with her heels towards him, and laid
back her ears. It may be interesting to state that the locality
of the fracture in these two cases was nearly at the same point.

The following remarkable case was related by Prof. R. D.
Mussey, of the Ohio Medical College at Cincinnati, to the
class of 1840, in a lecture on the physiology of the brain. I do
not know whether it has ever been published.

A man living in Vermont was standing near his mill, banter-
ing with his son about shooting a kingfisher that was perched
on a dry snag that projected from the water in the pond. The
son fired and the rifle ball, a small one, missed the bird,
ricochetted and struck the father near the middle of the fore-
head. He dropped instantly, and for a long time it was thought
impossible for him to recover; but time wore on, and he still
lived, a mere animal, incapable of speech, for 15 years, at
which time there appeared a slight elevation of the skull at the
crown of the head.

Dr. Mussey was called and trephined the spot, when he was
enabled to remove the flattened ball that had remained so long
within the skull. In a few moments the old man called out,
"Zeke, you dog, you missed it!" "Missed what?" asked the
doctor. "Why, the kingfisher!" This was the first word
spoken since the accident, and he could not understand that
the report of the rifle was not still reverberating over the
water at that moment. Zeke was married, had a family, and
was living in the West. The father had grown gray, and all
was changed. A Rip Van Winkle in reality.

Art. VI.—CEREBRAL HYPERÆMIA.

By I. L. Teed, M. D., Kansas City, Mo.

THE brain, like the other organs, is composed or built up of certain elementary parts; viz., the cell element, or the proper structure of the organ; the vascular element which includes the fluids conveyed to and from it; the residuary or intercellular fluid and the channels of conveyance to and from the organ; and thirdly, the nervous element by which it is brought into co-ordinate relation with the rest of the body; only, as in the brain the cell element includes the nervous element, the brain being its own nerve, this element has to be examined from two separate standpoints.

In the other organs the result of their function is expressed in a material form, and this expression is therefore capable of substantive analysis; in the brain, the result of its function has only a dynamic form, and is therefore incapable of any such analytical inquiries.

By way of comparison, the disorders of the liver may be classified as first, disorders originating in intrinsic disturbance of its cell element, whether of the proper hepatic cells or of its connective tissue matrix. Secondly, disorders originating in disturbance of its vascular element, whether of the blood, of the intercellular fluid, of the lymph, of the blood vessels, or of the lymphatics. Thirdly, disorders originating in disturbance of its nervous element, whether this arise within itself or be reflected upon it from some other organ. Fourthly, accidental disorders, whether produced by foreign bodies, pressure, traumatisms, and the like.

In this arrangement the results of disorder of its function, such as of the bile, sugar, etc., therein produced, are lost sight of primarily, as such disorder is only a consequence of some disorder in one or other of its primary elements, because their very existence is a secondary result of the previous existence

of these other parts; while as a similar disorder of function may be the result of primary disorder in either one of its elementary parts, attention becomes directed to actual pathological states, and not to mere symptomatic appearances. Thus the hepatic cells may of their own intrinsic action secrete an abnormal bile or sugar, abnormal either in quality or quantity; or they may be necessitated to such abnormal action by some disorder in the vascular element, such as quantity or quality of blood, conditions of the channels of conveyance, or some other; or they may be necessitated to such abnormal action by some disorder in the nervous element, whether this originate in the organ itself, or be reflected from some distant organ; or they may be necessitated to such abnormal action by accidental causes, such as mechanical or traumatic violence, producing pressure, injury, or the like, under which may be included foreign bodies, gall stones, enlarged glands in the vicinity, visceral distentions, etc., etc. Now it is evident that to restore permanently the normality of function to the hepatic cells, the cause of disorder must be removed; and it is also evident that in these different conditions very different medication is necessary, while under a failure to discover the pathological condition, the medication might lead to disastrous results, if not to death itself.

Let us transfer this plan of classification of morbid states to the brain. We have: First, disorders originating in its cell element, which include all degenerations of vesicular cells, of nerve fibres, and of neuroglial cells, commencing as original processes. Secondly, disorders originating in its vescular element, which include miliary and other aneurisms, thrombosis, embolism, hyperæmia, anæmia, overflow of lymph channels or serous appoplexy, all forms of toxæmia, and all abnormal conditions of the blood itself. Thirdly, disorders originating in its nervous element, which include all the disturbances in the functions of the nerve cells and fibres, whether central or reflex, that are not produced as secondary results from disorder originating in the other elements. Fourthly, troubles originating in consequence of mechanical or traumatic injury, as from disorders of the envelopes hard and soft, concussion, contusion, cysticerci, etc., etc. In this arrange-

ment the results of disorder of its function, such as insanity or disorders of ideation, spasm, convulsion, paralysis, or disorders of motion, and disorders of sensation, are lost sight of as primary morbid conditions, because such disorders are only a consequence of some disorder in one or other of its primary elements; ideation, motion, and sensation being secondary results of the previous existence of those parts; and it is only by such disordered manifestations that we become cognizant of trouble existing in these primary elements; they being the symptoms of such disorder, just as palpitation, cough, or vomiting are indications of derangement of the various organs manifesting such symptoms. No one would say that palpitation, cough, or vomiting are substantive realities; they are merely modes of action, abnormal in their manifestation; in the same way insanity and disorders of ideation, spasm, convulsion, and paralysis, and disorders of motion, and disorders of sensation are not substantive realities, they are merely modes of action, abnormal in *their* manifestation; and, if the abnormal condition of ideation is not a substantive reality, the normal condition of ideation cannot be a substantive reality, it also is only a mode of action, but is normal in its manifestation. Thus it is high time that the word " mind," (which is the same as ideation), be no longer employed to express a substantive reality, any more than we should express a substantive reality by the term " rhythm of the heart."

We may speak of mental disorder as we say pulmonary disorder or cardiac disorder; only in the first we expressly designate the particular mode in which the cerebral disease is manifesting itself, as in the two latter we may speak of cough or palpitation. We speak of them, it is true, as morbid conditions, but really we mean they are symptoms of morbid conditions; so when we say a person has a cough, or is troubled with palpitation, we mean exactly the same as when we say a person is insane, we express the prominent symptom in each case, but not the true morbid condition.

The function of the brain (nerve centers) consists in the correlation of the force of the external world into that particular mode of force known as nerve-force, which may be manifested either as sensation, ideation, or motion, according

to the particular cells involved in the correlation. To perform
this function properly the cells, (neurine) require a certain nu-
tritive condition, the result of definite relations between them
and the blood. In a former paper it was attempted to explain
why a lack of blood, and consequently a disturbance of these
relations gives rise to phenomena, apparently identical with
those produced by an excess of blood. In the present paper
some of the phenomena of the latter, that is, of hyperæmia,
will be examined, especially those lying in the borderland be-
tween the physiological and the pathological states.

The first point to which allusion will be made, is dilatation
of the arterioles from paresis of their vaso-motor nerves.
These nerves are derived from the sympathetic; and as for a
very considerable period in medical history, the sympathetic
nervous system has been considered by some as an entirely
different system from the cerebro-spinal, we find a constant
tendency to attribute some morbid conditions to especial mor-
bid changes in this system as an independent organ; while
some have considered this system as the dominant source of
nervous power in the body, and therefore attribute nearly all
diseases to disturbances of its condition.

To complete the electric circuit, we need, first, the battery in
which the electricity is generated; secondly, the body into
which the electricity is to be discharged or stored up; thirdly,
an efferent wire from the first to the second; fourthly, an
efferent wire from the second to the first. If only one wire
be extended from the first to the second, so no substitute for
the second wire be employed, there will not be any passage of
the electric current, however freely electricity may be de-
veloped. In the body we have the same conditions—the
electricity is developed throughout its whole periphery—the
efferent nerves convey it to the brain; then it is correlated,
equivalent to discharged, efferent nerves to the periphery com-
plete the circuit. Efferent nerves pass to the brain (and cord),
and they are various in kind. Efferent nerves pass from the
brain (and cord), and they also differ in kind; the sympathetic
nerves forming one of these varieties.

Paresis of vaso-motor nerves may therefore arise from various
cerebral conditions; such as forms of emotion, overwork, cen-

tral diseased conditions, toxic agents, reflected diseased conditions.

The second point to be examined is physiological hyperæmia from increased action of the brain itself. One person commences the pursuit of blacksmithing, another ballet dancing— by the influence of the will they put certain series of muscles into unwonted activity; their tissue waste is increased, their blood circulation is increased, their nutrition is increased, their functional power is increased. A third person commences to study, by the influence of his will he puts certain series of cortical brain cells into unwonted activity; their tissue waste is increased, their blood circulation is increased, their nutrition is increased, their functional power is increased.

It should ever be borne in mind that the brain is only a part of the body, and is subject to the same laws as the rest of the body, both in its healthy and in its morbid conditions. If the blacksmith overworks, if the ballet dancer over-dances, paralysis of their overworked muscles and subsequent atrophy may be the result; if the student overstudy the same effect may be produced. But from the peculiar histological structure of the brain, in another that complex condition known as inflammation may be produced; while in another the excessive discharge of nerve force in study may so reduce the activity of the nerve centres, that the arterioles do not contract from actual deficiency of nerve force sufficient to enable their muscular coats to overcome the dilating influence of the blood current passing through them.

The next point is, hyperæmia resulting from alcoholic drink in small quantity. Increase of oxidation may be produced by increasing the supply of oxygen, or by increasing the supply of easily oxidized material. Under the usual somatic conditions when alcohol is absorbed into the blood, it rapidly becomes oxidized, and thus saves the tissues from oxidation or waste. This rapid oxidation and liberation of force, accelerates the somatic processes, and part of the force thus largely liberated becomes used up at once in increased ideation. Another part is used up as increased motion, and this produces an increase in the heart's action, and a corresponding degree of increase in the circulation throughout the body; in which the

brain, receiving a supply equal to one-fifth of the total volume of blood circulated, has its circulation much more largely increased than is the circulation of any other organ; this increase distends the arteries, closes up to a greater degree than normal, the perivascular spaces, and thus increases the results of oxidation in the brain, at the same time that it diminishes the channels for their removal. Under rest, or so-called sleep, the result of the pressure, and therefore very slight coma rather, the circulation becomes re-established in the arteries and the lymphatics, and the alcohol being eliminated, proper tissue waste again occurs, and the functions of the brain cells return to their normal condition.

The balance of the cerebral circulation being maintained by the lymphatics as well as the veins, if the former do not convey away the surplus intercellular fluid (the cerebro-spinal in this organ), which results from the oxidation of hydrogen, whether of the tissues or of the contents of the blood, this fluid accumulates; and as the cranium is a closed cavity with non-distensible walls, and therefore having a speedy limit to its containing capacity, the fluid produces pressure, and thus hinders the arrival of the arterial blood, retards the circulation, and allows the intercellular fluid to pass by osmose into the veins, until the arteries have so contracted as again to leave the perivascular spaces open.

The next form is hyperæmia occurring in the earlier stages of pyrexia, or fever in the exanthemata, before the appearance of the eruption; with it, or just after it, we often have symptoms of cerebral disturbance, delirium, sleeplessness, disturbed dreams, and the like. What is the cause of such symptoms? The disorder is accompanied by a diffusion of inflammatory foci in the skin; in these spots there is an increased tissue change, an increased liberation of force; this is conveyed by efferent nerves to the brain, increased activity of nerve cells is the result; with this comes increase in the blood supply. Then again the increased liberation of force causes acceleration of the circulation and dilatation of the arterioles by the increased volume of the blood current. An increase in the urinary secretion, if it can be obtained, will in such cases assist materially; hydragogue catharsis is the next substitute—in some

few cases blood-letting, an operation which nature frequently accomplishes by epistaxis. This condition must not be confounded with that form of hyperæmia sometimes met with in advanced stages of fever, in the adynamic or typhoid condition; whether bilious, remittent, typhoid, typhus, or continued (Synochus); as in almost all these the congestion is passive, or venous.

The last point to be adverted to is that form of hyperæmia found in general paralysis of the insane, in which the arterioles are most probably in a state of moderate or slight spasm; they thus at all times contain a larger amount of blood than natural. As this form of disease has generally proved fatal, it has been very fully investigated, and is still being investigated by numerous physicians of lunatic asylums, to whose reports the reader is referred.

The general subject of hyperæmia is sufficiently discussed in the text-books, and if the results of hyperæmia in other organs be transferred, *mutatis mutandis*, to the brain, the results of hyperæmia of this organ will be at once intelligible—vide Hammond and Niemeyer.

Art. VII.—A CASE OF TRAUMATIC TETANUS TREATED WITH LARGE DOSES OF TINCTURE OF ACONITE. RECOVERY.

By J. C. THORPE, M.D., Lemont, Ill.

I was called September 8th, 1876, to see the son of Thos. Stein, a German, aged 14 years, who had received a wound in the ball of the left foot, made by a manure fork.

The history of the case as obtained from his parents, was that the wound had been made one week previous to the time I was sent for; they had paid but little attention to the child as they did not think the wound serious. On the 6th, two days

prior to my visit, he was unable to open his mouth, and his neck was stiff.

Upon examination. I found well-marked rigidity of the muscles of the neck and jaws, and the risus sardonicus was characteristic. He was unable to turn his head or to separate his jaws. The muscles of the abdomen were very tense; he complained of great pain in the neck and back; had not slept any for two nights. His only comfort was when placed in an easy chair. His bowels were constipated; tongue furred; pulse, 120; temperature of the body 104° Fahrenheit; difficult respiration; twitching of the thighs, and on raising him up he had a severe spasm, with well-marked opisthotonos. The condition of the eyes indicated that the third cerebral nerve was involved, shown by the fact that the muscles which are supplied by this nerve became tetanic and caused retraction of the globe so deeply in the orbit, that the eye was almost lost to view. The wound in the foot presented an unhealthy appearance, resembling hospital gangrene, and was not unlike it in smell. To this I applied dilute carbolic acid. As I never had treated successfully a case of tetanus, I was quite anxious in regards to this one. The plan of treatment which I determined to pursue in this case, was the administration of aconite, a tincture of which was prepared by my friend, Mr. Jacob, who is a thoroughly educated druggist, and therefore the drug could be relied on as being good. The doses given at first was *eight minims* every two hours, this was continued for two days without any perceptible impression from the medicine or any mitigation of the symptoms.

10th. I found the bowels constipated. I ordered castor oil and turpentine, and tinct. belladonna was applied along the spine. The tinct. aconite was increased to *twelve minims* every two hours. The diet ordered was the most nourishing that could be taken. Strong beef-tea, brandy, etc.

11th. The bowels had been opened by the oil and turpentine. Patient slept some during the night; the pulse 100; temperature of the body 102. At times he complained of great pain in the abdomen. Upon raising him up his breathing would become hurried and short. This was owing to the imperfect action of the muscles of the chest, producing the

most painful dyspnœa, which was relieved by laying him back in his chair. Continued the aconite.

12th. Passed a restless night; tongue covered with a dirty fur; pulse 110. I gave him, at this visit, sub. mur. hyd., gr. viij; pulv. opii., gr. ii; pulv. ipecac, gr. iii; misce, divid. tr., 3 powders; one given once in 3 hours, followed by castor oil and turpentine. The aconite was continued.

13th. He had rested more quietly after the action of the oil; relished his beef-tea; the wound in the foot more healthy. I directed that they should continue the aconite.

14th. General improvement in all the symptoms. He could be moved without producing much pain. Had slept quietly during the most of the night; pulse, 90; temperature of the body, 98. I ordered that the aconite should be continued, twelve minims, once in two hours. Discontinued the use of the belladonna to the spine.

15th. At this visit it was evident that the aconite had began to take effect. He rested well during the night; had no return of the spasms; the expression of the face improved; he could move the head a very little; the jaws could be opened about half an inch.

16th. Feels more comfortable; no pain; the expression of the face more natural. From this time forward there was marked improvement in all the symptoms. In the course of five days the spasms and opisthotonos ceased; there was no twitching in the lower extremities. I continued the aconite in the same doses once in four hours, except when he was asleep, up to Oct. 1st, after which time the doses were gradually decreased to October 12, at which time the face had almost regained its natural expression; he could open his mouth about half an inch, and could sit up and take his meals at the table. His convalescence, though slow, has been gradually progressing, and he now is entirely recovered.

Conclusions.

1. Is not tetanus a zymotic disease?

2. Can tetanic spasms be reproduced by the secretions from the wound if applied to other wounds?

3. Why did not the aconite given in the large doses, and repeated as they were, produce toxic symptoms?

4.   Will this *materies morbi* arrest the physiological influence of the remedy on the system?

5.   Can we fix the limit to the use of medicine in disease by the amount given to a healthy person?

This case was made so important from the large and continued doses of the aconite that I have thought proper to call the attention of some of my medical friends to it.

Neurological Correspondence.

New York, March 20th, 1877.

Messrs. Editors: I take pleasure in offering you as the result of neurological discussions, not only papers by Drs. Eugene Dupuy, Edward C. Spitzka, and E. C. Mann, and others, but also discussions and abstracts of papers by many other physicians, which I hope will be of interest to your readers.

NEW YORK COUNTY MEDICAL SOCIETY.

On the 8th of January last, Dr. Eugene Dupuy, of this city, read a paper entitled "The Pathology and Treatment of Reflex Motor Symptoms. Paralysis, Contractions, etc.," which will be found in another portion of this Journal.

After he had finished, a discussion followed, of which the following is the principal portion:

Dr. Jacobi said, that his remarks would be rather in the nature of historical allusions than otherwise. He would mention one fact which is, that from the very first time that the subject of reflex paralysis was brought to the light, he doubted very much its reality; he was not inclined to accept the theory which was and is, that the paralysis is the result of an external irritation. That, he continued, goes against me. I can readily believe with Dr. Dupuy, that convulsions may result from external irritation, but I could not very well imagine that the paralysis should be the immediate result of the external irritation. At all events I think that we will be obliged to look out for some central change, which, in the shape of an irritation, will be the cause of the subsequent paralysis. I must admit that I have not such an immense array of facts as Dr. Dupuy; neither have I such a large experience as other gentlemen, but so far as we know, reflex movements result from some central irritation, and these convulsive movements are the result of some external irritation.

I think that the explanation should be looked for in the fact that when peripheral irritation takes place, not only are the sensitive nerves irritated, but also the accompanying vaso-motor nerves and the accompanying trophic nerves; yet I say that I do not mean to decide the question whether vaso-motor nerves are trophic nerves. I do not believe that vaso-motor and trophic nerves are identical.

Dr. Dupuy has alluded to the fact that Claude Bernard, twenty years ago, and also some other physiologists after him, had found, or had believed that they had found, that dilatation of the blood vessels resulting from external irritation, was the result, not of paralysis of the sympathetic, but an irritation of some central blood vessels, and the dilatation was supposed to be the result of the paralysis. Now it is assumed, and very probably correctly, that there is irritation. Now I can imagine what formerly I could not, that a dilatation of the central blood vessels will result from vaso-motor irritation.

We know that changes in nutrition will frequently occur just as well with dilatation as with contraction of blood vessels. We know that if the blood vessels in the dura mater of the spinal cord and in the spinal cord itself are dilated, there will be pressure on the spinal cord as well as change in nutrition. The result is an immediate stoppage of the nerve action as well as paralysis.

I am inclined to believe in paralysis due to an irritation caused primarily by a central irritation and secondarily by a peripheral irritation—the immediate cause of the paralysis being the result of an external irritation.

You will excuse me for going so far into the matter. I am not at all prepared to discuss the subject.

Dr. MARY PUTNAM-JACOBI spoke as follows:

Mr. President—I do not feel competent to discuss Dr. Dupuy's paper polemically, for I have not as yet had an opportunity to perform the experiment upon which he mainly bases his theory—namely, observation of the exposed spinal cord during artificial irritations of distant parts, as the kidney, sciatic nerve, etc. But I should like to ask a few questions, it

only to bring out into better relief, several points that seem to me at present somewhat indistinct.

And in the first place, is Dr. Dupuy justified in classing together such diverse phenomena as cramps, spasms, perversions of sensibility, and finally paralysis,—and assuming that they must all receive an identical explanation on the ground that they are all "reflex?" The application of this term to pure paralysis, presumed to be induced by irritation of parts remote from the nerve centres, is indeed in common acceptation, yet in regard to it I think the remark of Vulpian is extremely apropos. He observes that by the very terms this kind of paralysis cannot be a "reflex action," because the impression transmitted to the cord is not reflected from it, but terminates in it, and in such manner that even the ordinary centrifugal, *i. e.*, the motor impulses are *arrested*. Hence the paralysis. When the irritation causes an increased secretion of a gland, there is indeed a centrifugal as well as centripetal impression; and when, as in the case of tetanie, developed during the irritation of intestinal worms, spasmodic contraction occurs, we may say that there is really a reflex action. But in paralysis, there is "reflexly" *no* action,—in other words, there is an inhibition of action. The distinction is so marked, that it seems to me stronger proof is required than has been brought forward this evening, to show that the condition of the spinal cord is identical in the two cases.

Dr. Dupuy does not merely assume a lesion of the spinal cord as the necessary intermediate link in the production of "reflex" phenomena, but claims to describe it from actual observation. He thus not only differs from the earlier writers, as Stanley, who made absence of spinal cord lesion an essential feature of reflex paralysis; but also from those later authors, who admit either the theory of exhaustion, or the theory of inhibition of the action of nerve cells, a state evidently not demonstrable to the eye. Dr. Dupuy's theory of permanent dilatation of blood vessels, seems designed to meet the special objections which have been so often raised against Brown-Séquard's theory of vascular cramp. It has been said that spasmodic constriction of blood vessels can never last so long as do the paralyses supposed to be explained by them. It is

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certainly easier to imagine a prolonged relaxation of blood
vessels. But during what time has this been observed on the
spinal cord? How has the Doctor eliminated the effects of
prolonged exposure to the air, which necessarily exists in his
experiment, and is necessarily absent in the clinical cases,
when the spinal hyperæmia is supposed to be induced entirely
by irritation of distant nerves?

The rapidity with which such irritation may induce paraly-
sis, is most elegantly illustrated by the experiment of Lewis-
son, which I have myself repeated. If a rubber band be passed
tightly around the neck of a frog so as to constrict it, the frog
instantly jerks over on his back, with a sudden convulsive
movement, and then lies perfectly motionless. When turned
prone, however, he remains so, and offers no resistance to any
irritation. Lewisson states that such a frog will cease to
breathe, and die within 24 hours, while, if a ligature be passed
around the root of the lungs, thus avoiding injury of the
cutaneous nerves, the animal will live eight days. This I have
not myself tested. But I have tried the effect of passing a
ribbon tightly around the thigh of a frog, excluding the blood
vessels and sciatic nerve. The leg was immediately partially
paralyzed, the thigh remaining in semi-extension, and moved
sluggishly as compared with the other limb. In two hours,
however, although the ligature remained in place, the motility
was fully restored.

This rapid disappearance of the paralysis is observed in
nearly all cases of experimental irritation of remote nerves,
and constitutes one of the greatest difficulties of applying the
results of experiment to the clinical cases—where the phe-
nomena are of so much longer duration. Dr. Dupuy meets this
difficulty by supposing that, after the hyperæmia of the cord
has lasted a certain time, permanent alterations of nutrition
occur in the nerve elements. He does not suppose these to be
compressed by the enlarged blood vessels, but to suffer from an
arrest in the molecular nutritive exchanges, supposed to
coincide with the slackening of the blood current. This
theory, somewhat recalling Brown-Séquard's theory of syncope
by general arrest of molecular nutrition throughout the body,
is, I believe, the most original and ingenious point in Dr

Dupuy's paper, but also, it seems to me, one that is by no means sufficiently demonstrated. It is closely interwoven with the doctor's special views as set forward on various other occasions, according to which alterations of nutrition in nerve tissues are always preceded by alterations in their circulation, and *depend* upon these. It is quite in accordance with this view, that Dr. Dupuy—although not saying so with sufficient precision on this evening—returns to the theory of Whytt and Prochaska, which regarded the sympathetic nerve as the agent of communication in reflex paralysis. If I understand the doctor aright, he considers that in the peripheric lesion—cutaneous or visceral, which has preceded the paralysis—the essential circumstance is the irritation of the sympathetic nerves of the part. This irritation is at first transmitted to other branches of the sympathetic system—hence to the vaso-motor nerves furnishing the blood vessels of the spinal pia mater; and these blood vessels at first contract, to dilate afterwards from exhaustion.

It seems to me a radical presumption against this theory, that it supposes a precision of relationship between the vascular territories of the injured part and the spinal cord, which really only exists for the spinal nerves distributed to the part.

Because irritation of a segment of the cord will produce vaso-motor phenomena in a distant part, we surely are not to infer that irritation of that part must equally produce vaso-motor phenomena in the cord. I would like to inquire, also, whether Dr. Dupuy considers that the irritation is distributed over all the filaments of the sympathetic nerve which intervene between the irritated viscus and the cord; whether, for instance, in a case of paraplegia consequent upon uterine retroflexion, the entire hypogastric and lumbar plexuses experience the initial irritation and secondary exhaustion attributed to the filaments supplying the spinal blood vessels? If so, where are the symptoms of such widespread disorder? But if not, how explain such precise transmission of impressions across this intricately ramifying plexus? How, indeed, can we prove it?

In the case of paralysis from irritation of cutaneous nerves, Dr. Dupuy lays stress upon the pain, and hence seems to re-

gard the sensory nerve filaments as the channel conveying the
reflex impression. But in many visceral lesions, especially
those of the kidneys, this is absent, and the argument must
rest on other facts. Unfortunately so many of these facts are
susceptible of double interpretations, and none more than the
condition of the cord as described by Dr. Dupuy in his experi-
ments. I will only allude to the fact that Gull, repeating
these experiments for the examination of the theory of vascu-
lar cramp, insists that he could, with the naked eye, observe
no contractile vessels in the spinal pia, but only veins, and in
those he observed no alterations of calibre whatever during
irritations of the kidney, etc.

There is here a question of fact that I am not at present in
a position to discuss. But, admitting for the moment that an
initial pallor of the cord, followed by secondary hyperæmia,
be distinctly observed we must still ask: 1st. Has the hype-
ræmia supposed to depend upon distant irritation, been dis-
tinguished with sufficient care from that due to exposure of
the cord? 2d. If electrical irritation of a sciatic nerve, (as in
some of Dr. Dupuy's experiments), produced "reflex para-
plegia" when the cord is exposed, why does it generally fail to
do so when the vertebral column remains intact? 3d. Where
is the proof that this hyperæmia precedes and determines
alteration of function in the nerve cells, instead of being
determined by them? If "reflex" impressions be really con-
veyed by vaso-motor nerves, and if these really, according to
the old opinion, constitute an independent system, instead of
one anatomically derived from the spinal cord, (the generally
accepted modern view), then it is evident that the nerve
elements of the cord *can* only be affected by means of altera-
tions in the blood supply. But if impressions affecting the
functions of the cord are conveyed to it by the nerves especially
appropriated for the purpose of conveying impressions to it,
then the morbid phenomenon falls into the same frame work
as the physiological; the impression first makes itself felt
upon the central elements in which the nerve filaments termi-
nate, and the alterations of the circulation are consequent
upon alterations of the functions of these elements. If, as
maintained by Eulenburg and Handfield Jones, the functions

of the motor elements be directly inhibited by the afferent irritation, we should expect the circulation to be slackened by diminution in the nutritive demand, hence relative stasis of blood in the vessels of the pia mater.

Dr. Dupuy has not made it perfectly clear whether he supposes that the vaso-motor paralysis leads to inflammation of the cord, and hence to permanent paralysis. If so, his theory would offer a different explanation of the facts explained by Leyden, Gull, Hayem, Feinberg and others, as the results of ascending neuritis. On the one hand, however, our views of inflammation have not yet so far retrograded from the doctrine of Virchow to that of Hunter, as to admit that congestion, independent of cellular irritation is able to set up inflammation. On the other hand, the cases of neuritis positively demonstrated, are, according to Leyden, only four; namely, the one by Gull, with microscopical alterations of the cord; one by Kussmaul, with atheroma of pelvic arteries, and fatty degeneration of the ischiatic nerves, and two cases of Leyden himself.

There is one question more, Mr. President, I should like to ask, and that is relative to the diagnosis. I think Dr. Dupuy has not brought out the points of diagnosis as clearly, as at all events we should like to have them; on the one hand, even of idiopathic disease of the spinal cord—on the other, in regard to the share of reflex irritations in cases where other causes for paralysis exist. Thus I have at present under observation two cases; the first a woman, who, seven years ago, sustained a severe hemorrhage at confinement, and upon first getting up out of bed, two weeks later, was found to have a retroflexed uterus, and also a marked paresis of the lower extremities. The retroflexion has disappeared, but the paresis persists, making walking somewhat difficult, and certain movements, as rising into a sitting position from an extended one, quite impossible. Which is the cause here—permanent exhaustion of the cord by the hemorrhage, or reflex irritation from the uterus? Again, a second case, of an extremely chlorotic, but not hysterical girl, affected with a slight retroversion of the uterus, seized suddenly with paresis of lower limbs, which in twenty-four hours was so complete that she became

unable even to stand for a few seconds—a condition which has persisted unchanged for six months. Which is to blame here —the chloro-anæmia, or the moderate retroversion?

Dr. Dupuy does not tell us whether the existence of hyper-æsthetic points along the spinal column can be relied upon as indications that spinal hyperæmia exists in these cases. He lays stress upon them in other cases to which he applies the revulsive method of treatment. Nor does he explain the absence of the symptoms generally considered characteristic of spinal congestion, notably numbness, prickling, etc. But especially he does not attempt to explain how a lesion, that confessedly would be considerably diffused throughout the substance of the cord, should produce such extremely localized effects. This difficulty has always seemed to me of great weight in the question of infantile paralysis, so often referred to a congestion of the cord, and which really may often depend on a myelitis, not only anterior, but diffused. I confess —should I permit myself to express in this place an independent opinion, it would be that, even when an active or passive hyperæmia of the cord were proved, either experimentally or clinically, it must be a lesion secondary to that of elements directly involved in the morbid process, namely, the ganglionic cells constituting the central terminus of the nerves whose peripheric expansion has been involved in the irritation of a distant part.

Prof. Lewis A. Sayre said he did not pretend to offer any finely spun or incomprehensible theories in explanation of cases of so-called reflex paralysis which have come under his notice. He called them cases of reflex paralysis in his books for want of a better term, and because he did not know what else to call them. He can give only the results of personal observation, and the effects which he has seen. He will leave the more profound manifestations for physiologists to explain. He then related several cases of reflex paralysis depending on irritation at the end of the penis. He removed the irritation and the paralysis was almost immediately relieved. It seemed to him that if the paralysis had been due to a central lesion, recovery would not have been so rapid. And the proof that there was no central lesion was that the paralysis was relieved

on removal of the external cause. One case from Westchester County was brought to his office, where the foreskin was adherent to the glans penis. The boy was a most pitiable object. He was apparently idiotic, and could not pronounce his mother's name. The attachments were broken up, and in ten minutes he could call his mother by name. He lacked power to co-ordinate the muscles of speech, but as soon as the cause of such want of power was removed, he could without difficulty call his mother by name.

The Doctor is in receipt of letters from different parts of the United States showing that many cases of this class have been treated successfully. He has noted the histories of some sixty or seventy, and the result of such treatment has been most happy.

Dr. Fessenden N. Otis mentioned a case which he had seen where paralysis was due to the presence of several strictures. The paralysis disappeared on the removal of the strictures.

In commenting upon Dr. Sayre's remarks, Dr. Jacobi said that it seemed to him a misnomer to call such cases as were related by Dr. Sayre, illustrations of reflex paralysis. The phenomena were those of choreiform spasm.

Dr. Jacobi also deprecated the declaration made by a person of so much influence as Dr. Sayre, that "he was quite indifferent to the theory of the facts observed,—all he wanted was the facts." In reality, the theory was of so much importance that *one* case, carefully observed and analyzed and explained, would be worth much more than the sixty quoted by Dr. Sayre, merely as repetitions of one another.

The Neurological Society.

At a meeting held January 9, 1877, Prof. A. E. Macdonald read a paper on General Paresis. The following is a pretty full abstract:

The disease concerning which he desired to speak derives special importance from the comparatively recent date at which its identity has been established, and from the remarkable increase within that period of the number of cases of insanity assuming its characteristics, although in the light of our present knowledge of its evidences, we can recognize them

as existing in individual cases long since described; yet so rare were these cases that they were regarded as simply presenting unusual symptoms added to those of some one or other of the three accepted varieties of mental aberration, and it is within the century that the increasing frequency of the occurrence of such cases has led to their closer study, and to their separation into a distinct type of mental alienation.

For a long time the theory of such separate identity was vigorously combatted, and even yet there are alienists and pathologists who, differing from the great majority of their confreres, hold that the association of the mental with the physical alteration, is only fortuitous, and does not constitute a distinct diseased condition. But this view is losing ground, and can scarcely be deemed tenable by any one whose investigations of the morbid manifestations have been practical rather than theoretical. If general paresis is simply insanity plus paralysis, the two conditions having no inter-dependence one upon the other, we might expect to find a similar paralysis attacking patients whose minds remain unaffected, but we do not; or to find patients manifesting similar evidences of mental disturbances, but we do not; and again when there *is* insanity and paralysis of this nature, we should expect to find the association, if it be indeed accidental, at one time with one form of mental alienation at another with another; but *this* we never find. Other paralyses, indeed, may attack the sane, or the insane, or if attacking the insane, may be found in different cases associated with mental disturbances of different types; but the paralysis of the character under consideration is no more plainly distinguished from other paralyses by physical peculiarities than is the insanity with which it is always conjoined distinguished from sanity, or from other forms of insanity, by certain definite mental peculiarities in the few cases wherein the disease attacks those who are at the time of its invasion suffering from insanity of another form.

Since its recognition and exaltation to the dignity of a distinct disease, much has been written; but the contribution of the professor was intended to be rather in the way of record than conjecture, to present an abstract of the present state of knowledge upon the subject, and to apply the theories ad-

vanced and the observations detailed by others to the cases which have come under his care in the institution with which he is connected. In this institution, out of a total of 1,600 cases of insanity, 305 were cases of general paresis.

The diseased condition to which we apply the term " general paresis " is characterized by an association of psychic and somatic evidences of which progression is a prominent feature. A case fulfilling the requirements of its designation will be marked psychically by a progressive incomplete paralysis of the muscles, extending gradually over those of the entire body; and by a mental failure marked by extravagant delusions progressive also both in the degree of the grandeur and in the variety of the subjects which they embrace.

The following is about the course which the disease pursues:

A man of middle age and robust frame, of active and vigorous habits, given very probably to high living, with the various dissipations which the term has come to imply, but preserving among his associates the reputation of a keen and shrewd business man, is observed to undergo a change. Generally there is a period of melancholy and depression, with irritability, lasting but a few days, and very possibly passing unnoticed. Then he is seen to be flighty. In his business he shows a speculative tendency and a recklessness that are new to him. He makes useless purchases and contracts which must prove disadvantageous to him. In his personal life, if he has been correct before he now becomes dissipated and " fast." If he has been self-indulgent, he seeks new and grosser dissipation. With it all, there is an evident air of self-esteem, of physical and mental well-being, a tendency to loudness of manner and dress, to ostentatious display coupled with unusual and unnecessary generosity towards others. The prevailing feeling now is one of good humor; the desire is to have a good time, and share it with as many as possible.

A little further progress in this direction and there is found open indecencies of conduct, petty thefts very likely, violence, towards others who have refused to join in with their proposed good time, and a decided failure of memory concerning especially actions and events of very recent occurrences.

In the meantime, physical symptoms have become apparent,

detected at first, if at all, by those familiar with the disease, but gradually by others. The prominent feature is a loss of co-ordinating power in the voluntary muscles, progressing steadily towards their complete paralysis. Recognized first as affecting the most delicate function, attention will be called to a slight hesitancy in speech, and looking at the lips or protruded tongue, there will be found a slight tremor in the former, with inability to keep them firmly closed, and in the latter, trembling and a disposition to return by an involuntary jerk into the mouth. The articulation is manifestly abnormal; there is a thickness of speech like that of a drunken man, a halting at certain words and a slurring over of others. It is evident that the patient feels that there is something wrong in this respect and endeavors to right it, enunciating his words slowly and carefully, and for a time succeeding, but soon lapsing again and making especially bad work of it, where vowel sounds are few and lip sounds frequent.

The appearance of the whole face changes also; it has a flabby, greasy look, and the facial folds are relaxed perhaps unequally, and in time obliterated. But more distinctive still is the appearance of the eyes, showing changes so constant, and so uniform, that they furnish one of the most reliable aids to diagnosis. At first there may be contraction of the pupils, but later there is dilatation, *unequal* dilatation, and in this inequality lies the great value of the evidence. Often there may be added to the inequality an irregularity of the pupil caused by the folding in of the edge of the iris; and there may be ptosis also.

The want of co-ordination is now found in the muscles of the extremities. If the elbow is bent and the fingers extended, they cannot be held steady, but their tips will tremble and jerk convulsively. Hence the hand-writing is altered and becomes scratchy and irregular, some letters are larger than others, and the lines upon the paper are not followed. The mental changes make themselves apparent in the composition of letters, so that their perusal will doubly indicate the condition of the writer. The change will be similar to that noticed in the speech—a tendency to omit words or parts of sentences, to mingle ideas together, and especially to repeat phrases and to reassert some prominent idea.

In the lower extremities the want of co-ordinating power is shown in changes of gait. At first there is a slight difficulty in starting promptly and easily; a little tendency to make a wider circle around corners, and a decided difficulty in changing direction quickly, and in avoiding collisions. Presently there is a tremulous, unsteady step, the patient separates his feet and stretches out his hands as if afraid of falling, and he either, drags his feet after him as in the ordinary form of paralysis, or brings his heels down with a rap as in locomotor ataxia. As these troubles increase, it becomes altogether too much of a tax upon his attention to keep his equilibrium, and to make progress, to permit of his turning it at the same time to anything else, and if he be addressed, he will stop and steady himself upon his legs before answering, even if the person asking the question is walking with him.

But long before this degree of muscular failure is reached, mental aberrations will have progressed until the stage of delusion has been reached, and these delusions are in themselves distinctive of general paresis, differing somewhat from the delusions of "ambitious mania," so-called. The one characteristic in which these delusions agree in their exaltation. But the delusions of general paresis primarily connect themselves with the patient himself; if extended to include others, the paretic is apt to recognize an inferior rank in those around him. Naturally these exalted delusions have reference, first, to the patient's person—his health, power of endurance and actions; second, to mental capacity and intellectual brilliancy, and the legitimate rewards of such perfection of body and mind have not failed to follow him—wealth, renown, honors, are his without measure. He is not king of a single dominion; he is emperor of the whole world. He is not *as* rich as anybody else; he is richer than everybody else put together. He is not simply president; he is "a bigger man than Grant." They are more tolerant of the delusions of others, even amounting to belief in them, than the insane of other classes. Ordinarily an insane man will recognize that his neighbor is deficient in sense while strenuously maintaining his own soundness; but a paretic's delusions are extensive enough to embrace all mankind. When one of them has given a sum-

mary of his wealth, the others, if appealed to, will endorse his statement, always adding that wealthy as he is, they are still more so.

A curious point in the history of the disease, is its gradual extension from one country or locality to another, and its gradual increase in localities where it has once appeared. Of its history in any one locality, it may be said that statistics show an uniform order of progression which may be recognized by certain salient characteristics, which, taken in order may be stated as follows:

1. The appearance and recognition of the disease in male patients.

2. Increased frequency of occurrence in males, and appearance in female patients.

3. Increased frequency of appearance in both sexes (in greater proportion than of ordinary forms of insanity), and increase in the proportion of females to males attacked.

4. Departures from the ordinary rule in various points, such as duration of the disease, age of the patients attacked, etc.

At the present date general paresis is the form of disease assumed by a large number of the insane in institutions in the Eastern and Middle States, while those in the Southern and Western, it is comparatively rare.

The following table, being a condensation of statistics of American asylums for the past three years. (covering a total of nearly 16,000 cases) will illustrate the comparative immunity from general paresis enjoyed by certain territories:

	STATES.			
	Eastern.	Middle.	Western.	Southern.
Percentage of paretics to whole ⎰ Males.	4.	8.6	1.7	1.1
number of admissions. ⎱ Females.	0.9	.4	.2	.1
Percentage of deaths from general paresis to those from all ⎰ Males.	21.2	23.4	9.3	7.3
other causes. ⎱ Females.	4.8	2.9	1.1	2.6

Regarding the causes of general paresis, only one thing can be regarded as settled, namely, that heredity must be looked upon as the great predisposing cause in this, as in the other

varities of insanity. It attacks persons of all grades of society, the dissolute as well as those whose habits are correct. However, it is impossible to deny that influence of dissipated habits when present and too long indulged in. Some ascribe the cause to alcoholism, to over-intellectual efforts, to syphilis, to a suddenly assumed and inordinate degree of dissipation; but the settlement of the vexed question, in the opinion of the speaker, is to come through the progress of pathological investigation; and all that it is safe to say in the present state of our pathological knowledge, is that it finds its subjects mainly among those in whom there is an hereditary tendency toward nervous disease; and that so far as immediate causes are concerned, while the disease undoubtedly attacks a few who have no such vices, and many in whom such vices do exist, an examination of the assigned causes of paresis in cases admitted to Wards Island leads the attention to two other influences—sunstroke and injuries to the head. Of 83 cases, the former was alleged to be the cause in 12, and the latter in 12; but sexual indulgence was the assigned cause in the largest number of cases, being 45 in all; while the remaining 14 were attributed to various causes, such as opium-eating, lead-poisoning, and otorrhœa among the number.

General paresis more frequently selects the male sex than the female; it occurs generally in middle life; and the prognosis is unfavorable—no other ending of the disease than death need be looked for. The average length of the disease is three years; death occurring from paresis uncomplicated, comes in one of two ways; either gradually through paralysis of the respiratory muscles and the exhaustion attending the disease, or suddenly from one of the specific forms of convulsions. The former is much the more common mode of death. Sometimes death occurs from complications with other diseases.

The doctor directed attention to the fact that that peculiar disease first described by Prof. W. A. Hammond, and called by him athetosis, which is "mainly characterized by an inability to retain the fingers and toes in any position in which they may be placed, and by their continued motion," has not so far as he was aware, been before spoken of as a frequent

concomitant of general paresis. He has observed it in quite a number of cases, the toes being affected more frequently than the fingers.

The doctor closed his paper by briefly alluding to the pathological anatomy of the disease, kindly furnished him by the special pathologist of the state asylum at Utica.

There is no pathological condition hitherto observed in the nervous centres which has not occasionally been found in general paresis; and the names applied to the disease by different authors—chronic arachnitis, diffuse periencephalitis, cerebral atrophy and the like—show what different opinions have prevailed as to the essential lesion. The most characteristic anatomical feature of the disease in its last stages is an extensive atrophied condition of the nervous elements of the cerebral organs, which closely resembles the condition observed in higher grades of senile dementia. In a large number of cases, probably one-half of all examined, there will be found upon the inner surface of the dura mater an inflammatory organized exudation to which the name "hæmatoma of the dura mater" was formerly given—an exudation sometimes reaching a thickness of half an inch, and consisting of vascular layers from two to twenty in number. Of the causes of these exudations, little is known. The majority of cases will be found in connection with chronic alcoholism, while some specific forms are of syphilitic origin.

The effect of these exudations upon the brain is that of gradually increasing pressure, causing interrupted circulation and atrophy of the convolutions beneath them. The pia mater in nearly all cases shows some abnormal conditions, varying from pigmentary and fatty deposits in the lymph spaces to purulent exudation.

The most extensive and constant lesions in the earliest stages of the disease are presented by the vascular system of the brain. In the beginning, abnormal dilatation of the vessels is a constant occurrence, followed by an atonic condition, obliteration of the capillaries, and distension of the lymphatic sheaths surrounding the veins, by the extravasation of blood corpuscles, which may give rise to new formations and degenerations.

Coincident with the alteration of the vascular arrangements and in proportion to their extent, the neuroglia and the nervous elements are invariably involved. The changes are various, and in the estimate of one or another author, each one is held to play the most important role in the determination of the disease. No one of them, however, has proven to be a primary morbid affection, or to be peculiar to, or invariably connected with, paresis. In fact, it may be said they all represent only different stages of atrophy and necrosis of the tissue, or, in other words, that they are the result of a *diffuse but slowly progressive atrophy and necrosis.*

REMARKS UPON DR. MACDONALD'S PAPER.

Dr. Thomas R. Pooley said that he had listened with a great deal of interest to Dr. Macdonald's paper, and thought it was especially valuable on account of the statistics which he gave. He regretted that the Doctor had not included in the cases which he enumerated, the results of opthalmoscopic examinations. He had had an opportunity to examine the eyes of a few cases of general paralysis of the insane in the general hospital for the insane at Middletown, Conn., and had always found atrophy of the optic nerves. Albutt, who had examined fifty-three cases of general paresis, found five cases in which there was no change in the optic nerve and retina, of the remaining forty-eight, atrophy of the discs in its different stages in forty-one cases, and in seven cases he was doubtful as to whether there was any change or not. He notes the following points in regard to the eye affection.

1. That atrophy in the optic nerves takes place in almost every case of general paralysis.

2. That it does not travel downwards from the optic centres and along the tracts, but appears to occur as an independent tract of sclerosis.

3. It often becomes apparent at first as hyperæmia with slight exudation—red softening. If thin, it whitens—generally from the outer edge inwards—the nerve becoming white and starring, and its edges sharply defined.

4. The atrophy seems to bear no constant relation to the ataxy of the orbital muscles seen in general paralysis.

5.   The stage of hyperæmia corresponds to the contraction of the pupil and dilatation ensues as atrophy succeeds.

6.   As atrophy of the nerves can hardly be surely ascertained in the incipient stage of general paralysis, it is of but little importance as a diagnostic symptom.   Its value lies rather in its important pathological significance.

These observations of Albutt make it desirable that the eyes of all such cases should be examined in order to control or disapprove his statements.   Coming as they do from so accurate an observer, they are, however, of great value, and the few observations which opportunity has favored him in making, which were, however, confined to the more advanced stage of the disease, are quite in accord with his statements.   It is to be hoped that further examinations with the mirror and pathological examinations of the eyes will soon be given us by those who have opportunity for the study of such cases.

DR. EDWARD C. MANN said that he had made several post mortem examinations during the past three years upon persons dying in his institution (the New York State Emigrant Insane Asylum), and after hardening the brain and spinal cord, had made microscopical examinations of the tissues.   He had found pigmentary and granular degenerative changes in the cells of the cortical gray matter of the frontal convolutions and other changes in the brain common to other morbid conditions, but that he had *invariably* met with an atrophied condition of the nerve element, of the posterior columns of the cord, together with a new formation of connective tissue.   He, therefore, regarded the morbid changes in the cord as peculiar to this disease—general paralysis—and he regarded this change in the cord a *constant*, and in many instances the *primary* one in this disease, as there was nothing in the changes that he had met with in the brain to distinguish them from the changes that he had met with in other forms of insanity.

DR. E. C. SPITZKA said that he most decidedly dissented from the conclusions at which Dr. Mann had arrived.   He had constantly found changes in the cerebral cortex in every case of progressive paresis whose brain he had examined.   He also considered the cortical lesion as primary, essential and characteristic in typical cases of the disease, while those of the

pedunculi, pons and medulla are secondary, either through a direct extension of the original process which began in the hemispheres, or by consecutive degeneration, in the manner described by Tuerck.

That well marked changes of the spinal cord do occur, there cannot be the slightest doubt; but they bear no relation to the characteristic symptoms of the first stages of progressive paresis. Their advent is marked by the loss of electro-muscular contractility, and sometimes by the muscular atrophy often noticed in the last stage, and in cases which die early enough, the cord may be found quite intact. It is in syphilitic cases chiefly that the spinal cord has been found markedly degenerated by himself, and here the change was of a peculiar character, here it is reasonable to suppose that the hemiplegic and spinal disorders kept step, since they were due to a constitutional process.

In a third series of cases, the spinal change is primary, and Dr. Mann's assertion might seem to apply to them at first sight; but on examination into their histories, together with the account of the microscopic appearances, as given by Westphal, we find that they did not represent a pure form of progressive paresis, but were in reality cases of locomotor ataxia complicated by progressive paresis.

He was very glad to hear Dr. Pooley refer to the changes of the retina and the optic nerve. Unfortunately, Dr. S. had not been able to make opthalmoscopic examination of the patients whose brains were afterwards submitted to his examination. But he had found certain inflammatory changes around and in the optic tracts and chiasms which, together with the degenerative processes in the thalami and higher centres, might serve to explain the causation of the optic trouble by the same or similar lesions which determined the existence of the mental and motor symptoms.

In this connection he would refer to some interesting observations made at the same English asylum, in which the author referred to by Dr. Pooley, Clifford Albutt, had made his opthalmoscopic investigations, on the marked prevalence of color blindness among paretics as compared with those suffering from other forms of alienation. That this is due to the

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same changes of the optic nerve and retina which Dr. Pooley has observed and described, he had no doubt whatever.

In his opinion the pathological summary presented by the author of the paper exhibits the contradictory nature of pathological evidence in progressive paresis. To no particular kind of lesion, but rather to its locality, extent and progression are the complex symptoms of this disease adducible.

Dr. Pooley wished to know whether Dr. Spitzka had examined the vaginal sheath of the optic nerve.

Dr. Spitzka replied that the nature of his post mortems did not permit the examination of the intra-orbital part of the nerve, or of the eye itself, much as he regretted it, for he expected important data from a thorough investigation of these parts.

Dr. Peters asked whether any deductions with regard to the localization of motor centres had been made in his analysis of such cases.

Dr. Spitzka said that he was well aware that in the light of certain modern experiments, it is but logical to expect some confirmatory pathological results. He, himself, began his examinations with a certain amount of bias in favor of Hitzig and Fritsch's deductions, but cannot refer to any confirmatory facts among his results. As a rule, the chief intensity of the pathological process was concentrated on the frontal and parietal lobes, it is true, but that this is a motor region we knew, anatomically, long before Hitzig and Fritsch experimented. Besides, the results of these physiologists are being questioned; and as to Ferrier's researches, he must be excused from referring to the fanciful theories of an observer whose experiments show so many sources of error, and whose conjectures are as wild as they are incompatible with well-founded facts in comparative anatomy.

Dr. Peters referred to the confirmation of Hitzig's results by Dalton and Arnold, and said that if any disease was needed to demonstrate the pathological value of these results, that disease was paresis.

Dr. Spitzka replied that if patients died with paretic symptoms confined to one group of muscles, we might be able to point to destructive lesions in certain convolutions as causing

them, but this is not the case; we find diffuse and multiplied changes in the cortex, a result quite in conformity with the fact that in the last stages the paresis has involved nearly all the limbs in succession. Occasionally, where marked disturbance of articulation was noticed, the operculum and island of Reil were chiefly involved. How uncertain the localization of a lesion with reference to the symptoms manifested during life is, one of his cases illustrated in a striking manner. Here deviation of the head, neck and arm to the left side had been observed *intra vitam*. In short, a series of symptoms analogous to these noticed in lower animals, in whom the posterior third of the thalamus has been divided, or in human beings in whom the thalamus was extensively diseased, (Meynert). At the autopsy the thalamus was found extensively degenerated, but the tegmental tract and the anterior commissure of the spinal cord were the seat of a similar process. Now either one of these three lesions would have served to explain the symptoms in question, and it can be definitely referred to none of them. The localization of motor centres has much that is seductive about it, as something which promises to reduce matters to an exact basis; but in its present condition it is not far removed from the theoretical condition.

At a meeting of the Neurological Society held March 5th, 1877, Dr. Mann read a paper on the "Pathology and Morbid Histology of Chronic Insanity," after which remarks were made about as follows:

PROF. HAMMOND asked the reader of the paper whether he had ever observed any derangement of the optic thalamus or parts adjoining in autopsies when in life hallucinations were present?

Dr. Mann answered that he had not.

Prof. Hammond had recently had a case under observation where it was subsequently proved that the patient had had a hemorrhage in the optic thalamus, and in which during life hallucinations of sight and hearing, with loss of sensibility on one side of the body were present. To-day he made a *post mortem* examination of the body of a patient in whom there were also hallucinations during life, but who had two or three

days previous to death a second hemorrhage in the corpus striatum.

Recent observations would go to show that there is some connection between the optic thalami and the special senses, and where there is hemorrhage in the optic thalamus there are hallucinations.

Dr. Peters remarked that in connection with a remark made by Prof. Hammond, he would like to refer to a case that interested him very much. He was unable to procure a post-mortem examination. It was in the person of a lady who had been blind for 15 or 16 years previous to the beginning of Dr. Peter's acquaintance with her. She could not distinguish light from darkness, although the pupils were widely dilated. She had frequently attacks of hallucination of the most intense and extraordinary character—that is, hallucinations of sight; she saw, at times, an immense number of figures— men and horses in the room—men who were busy building stables for horses in her bed-room, and these characters would disappear finally through the floor. At other times she would see a large number of washerwomen engaged at their vocation in her parlors. Then, again, there would be a large number of children playing about. These figures disturbed her very much. She was thus affected—deranged—two or three different times. The last attack was produced by some defect in the plumbing in the house, on which occasion the plumbing had to be overhauled from the top to the bottom of the house. There were a large number of men in the house who remained at work three or four weeks, and she was greatly disturbed thereby.

With all her blindness she was excessively neat, and a most perfect house-keeper. She would grope about the house, feel every chair and table, and detect the slightest particle of dust. She knew just how the tables and chairs were placed, and where and how everything was placed upon them.

When the plumbers had finished, the last attack came upon her. She never had hallucinations of hearing. All these things transpired without sound. She could not make the women and children speak.

Every one of these attacks was preceded by pain about the

eyes, so that he rather inferred that the irritation from the eyes extended backwards along the optic tracks.

He thought the seat of trouble was in the corpora quadrigemina, rather than in the optic thalamus.

She recovered at one time under the use of ergot; at another time by the use of santonin. While under the influence of chloral her illusions attacked her more severely, and he was obliged to resort to the use of hypodermic use of morphia. She was 72 or 73 years of age, a delicate, fragile, anæmic person, very bright, pleasant, and witty, and kindly when she was well, and of course, as she never went out except to ride, she had very little or no fresh air or exercise; her house was badly ventilated, (although it was a very large house) because she would never permit the windows to be opened for fear dust would enter. Altogether, she was an exceedingly interesting case.

Dr. James C. Kiernan said that one point in Dr. Mann's paper is taken up by nearly every writer on insanity, its importance being altogether out of proportion to the prominence given to it. He referred to the so-called hæmatoma auris on the ear of the insane. Whither it is ever found in the sane he could not state; at the same time, if it be a symptom of insanity, it is very rarely found in the hospital into which he is connected. In that institution, out of 1,600 patients, it had been observed in but 25 instances. He had observed it in those not classed as insane, yet not of sound mind—as the idiotic and the imbecile. In these cases he had seen it certainly as well developed as in cases of true insanity. In the year 1874 there was one case in the above-mentioned New York City asylum for the insane—a case whose ear presented the true hæmatoma auris, which the assistant mistook for erysipelas. In one work on insanity the writer speaks of this appearance of the ear as an element in prognosis. He speaks of its being of evil augury when it appears on the left side. The speaker had seen three recoveries where this appearance is present—certainly not a very large proportion. The average recovery of insane in the institution being 6 per cent. of those treated.

Prof. Hammond thought that most physicians residing in malarious districts have seen forms of mental aberration in persons subject to malarious influences. In several cases examined by him he has taken blood from the spleen by means of an instrument used for that purpose, and he has found pigmentation to a great extent. He has also, in several cases, detected pigmentation of the retina in conjunction with pigmentation of the spleen. He thought his observation would go far to confirm what Dr. Mann has said relative to pigmentation of the brain. There are numerous cases on record to show that pigmentation of the brain occurs in connection with a malarial condition.

Dr. E. C. Spitzka remarked that with regard to the majority of the points presented in Dr. Mann's interesting paper, he could agree to the fullest, as every one will who has worked in the same field. However, he could not regard some of the appearances which the author of the paper has described as bearing a relation to insanity, as at all concerned in the production of mental symptom. He had already called attention to the necessity of carefully discriminating between such changes as are observed in the sane brain, and such as are solely found with insanity, in a paper read before the International Congress. He was very glad to find since then that Obersteiner, of Vienna, had arrived at the same conclusions, as a result of the examination of over 150 brains of sane and insane subjects. He says that even in *healthy persons* who have passed the 20th year, we find a certain degree of vascular tortuosity, and even accumulations of pigment and granules in the adventitia and perivascular spaces.

These same changes have been described as connected with insanity by Dr. Mann, in conjunction with most authors. After the observations quoted, however, he could attach only a very doubtful value to them.

With regard to the pigmentation of the cortical nerve-cell, Dr. Mann, in accordance with every author who has written within the last twenty years, speaks of it as one of the characteristic changes of insanity.

Now, diffuse pigmentation of these bodies has, by Herbert Major, been found in the case of a sane man who had died

from traumatic injury. Dr. Spitzka sees no reason why this diffuse pigment should effect the functional integrity of a cell; for there are regions in the human brain which are *constantly and normally* pigmented, such as the *substantia nigra* of Sœmmering, and *locus cæruleus.* In the former, the degree of pigmentation is more intense than in any pathological pigmentation with which the speaker was acquainted, and to be consistent, it will be necessary to abandon pigmentation as being of any demonstrable value in insanity.

The excess of white blood corpuscles which Dr. Mann claims to have found in his patients, and to which he attaches an etiological value, will be found due to the same causes which provoked the insanity, or to the circumstances attending the malnutrition and confinement of the patients, and he (Dr. S.) failed to see any direct connection between the two. The symptoms which attend the closing scene of leucocythæmia, are sluggishness, and a general phlegmatic disposition, and have nothing in common with the *essential* phenomena of insanity.

It would lead one too far to refer to many artificial appearances due to decomposition, or the action of reagents, which have been quoted by many observers as changes accompanying insanity. He was happy to see that Dr. Mann had not mentioned them.

In bringing the subject of hallucinations before us to-night, Dr. Hammond had touched upon a very interesting and difficult theme. In order to refer this problematical symptom to its somatic basis, it is necessary to first endeavor to form a plausible idea of the normal signification of impressions.

Take, for instance, an impression of the retina. It is obvious that no registration takes place in the eye. The fibres of the optic nerve go, as Dr. Peters remarked, to the corpora quadrigemina, although a small portion goes to the thalamus, and, as Adamuek's experiments have proven, the complicated co-ordination of the ocular muscles takes place in reference to the object which is seen. Not only this, but that higher automatism by which our whole locomotor apparatus is made subservient to vision, in following a moving object without exerting the will, for instance, must depend upon the auto-

matic impulses which traverse the lemniscus from these ganglia. This is, therefore, also a most unfavorable locality for the reception of impressions for purposes of conscious registration.

There is a powerful fasciculus of the *corona radiata,* known as the *radiations optiques,* which is directly and indirectly connected by its peripheral end with the optic tract, and by its central end with the occipital lobe.

When we consider that these complex registrations which imply a higher consciousness can only have their seat in the higher centres, namely, in the cortex cerebri, we will be led to conclude that it is through this fasciculus that registrations are projected on the cortical convoluted screen. We may call it a screen because it acts like one in receiving impressions, and differs from it only in that its impressions are never blotted out except by destructive lesions, or by death.

The true explanation of an hallucination would therefore seem to be that in an intact cortical territory, through anomolies in its vascular supply, an old impression is re-awakened with life-like vigor, that an electro-negative oscillation takes place analogous to the one occurring when the actual impression was first registered.

He could explain every higher hallucination by this theory and by no other, and consequently he is at variance both with Dr. Hammond, when he would attribute this symptom to disorder of the thalamus opticus, and with our respected president, when he would attribute it to the corpora quadrigemina.

Dr. Hammond asked Dr. Spitzka whether he had not attached a fuller signification to the word hallucination, a higher sense to the term than he had? We know that all the special senses may exist without a cerebrum at all. If the normal sense of sight can exist under these circumstances, why not the abnormal?

Dr. Spitzka said that it is not the question whether the object seen or supposed to be seen, is believed in or not. It is the same as to the nature of the object seen.

Whatever of apparently intelligent action is performed by animals after the hemispheres are removed, can be safely referred to automatism. An automatism, however highly de-

veloped, can never include a registration of complex impres-
sions; and consequently the ganglia subservient to automat-
ism can never have any direct relation to an hallucination.
All the movements described by Dr. Hammond, he thought,
could be explained without resorting to consciousness as their
motive.

The interesting case of Dr. Peters, which he recounted
above, proves that the *retina* is altogether unnecessary to the
production of an hallucination; and the fact that those whose
eyes have been completely extirpated, have as vivid dreams
(and what, he asked, are dreams but hallucinations) as other
people, bears out the same conclusion. Reproduction of
images can have their seat only where a complete registration
of images is possible, and for the thousands of impressions
which a human being is called upon to register in his lifetime,
there is only one ganglionic region which presents individual
registration cells sufficient, and that is the cortex cerebri. The
interesting observations of Trevitanus, that the brain surface
is richer in blood when the sleep is interrupted by dreams,
than in quiet sleep—in the case of those whose hemispheres
had been exposed by cranial injury, throws a good deal of
light on the subject.

The Medico-Legal Society.

At a meeting held February 7th, 1877, Dr. A. L. Carroll, of
New Brighton, Staten Island, N. Y., read a paper entitled
"The Plea of Insanity," of which I give you the following
abstract:

It was not the purpose of his paper to attempt a definition
of insanity in the abstract. Jurisconsults and physicians
alike have ever failed and ever will fail to establish an unvary-
ing "norm" of mental function which shall be applicable to
all cases; and this for the reason that each individual problem
involves the consideration of such shifting factors as congenital
temperament, training, habit and other personal circumstances;
John Doe's insanity consisting in his departure from the men-
tal state, which is natural to himself, not in his difference from
the mental state which is natural to Richard Roe. The
nearest approach to a verbal description of insanity is to call

it *a departure from the individual's natural mode of thought and action arising from subjective change, i. e., not to be accounted for by changed external conditions;* or to put it in the form of the algebraic formula on which the modern scientific method insists, if a man's original character is represented by *a*, his training and habits by *b*, and his present peculiarities by *c*, then in any given case.

$$\text{INSANITY} = a\,b + c.$$

The aspect of insanity most interesting to the public is that which relates to criminal actions, especially to homicide, since it has latterly become the fashion with counsel for the defense to impress upon the juryman of the period that killing is *prima facie* evidence of mental unsoundness, and to retain medical experts as special pleaders, not to give an impartial scientific opinion, but to concoct the most injudicious case for the defendant, until many people are inclined to think that the proposed " mad-doctor " looks upon everyone except himself as insane.

It is beyond question that there are numerous phases of insanity clearly distinguishable by a professional observer, but not easily to be perceived by an unpracticed eye; and the dispute between law and medicine over these obscure forms of mental disease, arises from the fact that the legal notion of insanity *remains unaltered from the past age*, where nothing short of total dethronement of reason was recognized as lunacy, whilst modern medicine has learned to detect earlier stages and less marked varieties of affections of the brain as well as of the organs. Even in the most advanced legal opinions two characteristics insisted on as essential to insanity are, inability to distinguish right from wrong, and the existence of delusion; notwithstanding that almost every asylum affords abundant examples of lunacy—and dangerous lunacy —*without* delusion, and that every attendant on the insane can testify to the knowledge of right and wrong manifested by many of his most mischievous wards.

But setting aside all these legal doubts—the question arises, ought all these less-defined cases of lunacy to be excluded from the operation of criminal law; and the question would

be unhesitatingly answered in the negative by a few, at all events, of the ablest alienists, who, when they certify to a patient's insanity, do not necessarily mean to imply that he is unaccountable for any and all his actions. In other words there is a distinction to be drawn between insane criminals and the criminally insane, and it is quite possible that a person whose insanity would absolve him from responsibility in one direction may commit crime from a sanely criminal motive. In any given case, the first question to be considered is whether the criminal act was the direct outcome of the actor's insanity; the second, whether, if so, the person was incapable of resisting the insane promptings.

The doctor then considered the different conditions of which homicide may be the outcome. Little consideration may be given to the acts done by idiots or demented persons out of unreasoning mischief or mere irritation. If the imbecility be evident, of course legal responsibility cannot exist. Neither can there be any question of the irresponsibility of persons who commit violence under the influence of general mania, in which all the faculties are so manifestly deranged that the least practiced observer can readily detect the craziness. But aside from cases of acute or chronic general mania, in which incoherence of thought and perversion of reason proclaim the disease unmistakably, we may have to deal with partial mania, wherein the mind is clear in most of its operations, aberrant in but a few directions; and this it is which oftenest furnishes food for medico-legal discussions. It is beyond dispute that such a condition exists.

Cases wherein homicidal acts unmistakably spring from delusion, are also divisible into two categories: as to whether the delusion, if true, would or would not justify the deed. If a living parent kill his child in obedience to an imagined divine command, we cannot impute criminality to him more than to the patriarchal Abraham; if a man be insanely convinced that the person whom he attacks in supposed self-defense was about to take his life, we must judge him as we would a person of sound mind under actual similar circumstances. There are, however, on the other hand, instances where hatred, revenge, and other vicious motives are gratified

on provocations which would be trivial even if it were real. A monomaniac whose insanity is so partial as not to incapacitate him for ordinary business or social intercourse, is possessed by the delusion that some casual acquaintance has blackened his character, or supplanted him in his mistress's affections, and thereupon maliciously murders his fancied traducer or rival; or he broods over and executes mortal vengeance for a still less grievous imaginary affront, showing that he is wicked as well as insane. Whether here the insanity should cover the wickedness, is a query which medical experts show a growing inclination to answer in the negative. Hammond says: "If I entertain the delusion that a certain person has injured me, I may be insane; but, even if I am, I ought to be punished if I kill the individual who I imagine has done me a wrong," and he draws a broad distinction between persons who are " at the same time insane and responsible for an infraction of the law," and those "whose intellectual faculties are so perverted or destroyed as to render them absolutely unaccountable for their actions," advocating the infliction of the extreme legal penalty upon " the insane person, whose delusions are not such as would, if true, justify a homicide." Harsh as this view may seem at first, it becomes scarcely more than equitable when we consider how often homicides of such sort are perpetrated after evident premeditation and followed by carefully devised measures to avoid detection.

The writer then passed to the consideration of a series of sudden outbursts grouped under the heads " affective," " emotional," "volitional" or "impulsive insanity," characterized not by delusions or other marked disturbance of intellect, but by the predominance of instinctive impulse over volitional control. As regards these, a broad distinction from the juryman's point of view, may be made between acts of purposeless violence, done in a state of actual unconsciousness analogous to acute mania and those done with purposive vindictiveness.
* * * Of the condition known as "transitory mania," little need be said, save that most examples of it probably belong to epilepsy, and that in the few which remain, there are usually antecedent and present indications of mental disease or of very manifest tendency thereto.

Somewhat analogous is the state of mind—or, rather, absence of mind—which has passed into a by-word under the name of "morbid impulse," where a person, acting without passion, without delusion, without motive, "is irresistably impelled by an instinct" like the craving thirst for drink, " to commit murder or other criminal deed," which " his reason disapproves and his moral nature abhors, aghast at himself the while." The essential feature of this alleged form of disease, is that without apparent derangement of reason or loss of consciousness, the patient is possessed by "a violent impulse, which, swallowing up reflection and will, irresistibly utters itself in convulsive action." But when we come to look at the matter more closely, we find that even the strongest advocates of the doctrine of irresponsibility entertain doubts as to the existence of such an uncomplicated, single-symptomed malady. A very fair general statement of the phenomena of this particular phase of lunacy is that acts of violence may be done with *intention* but without rational *motive*. The affected person has an intense inclination to kill some one, he does not care whom; his victim may be his dearest friend or an entire stranger—but when his impulse vents itself by selection upon an enemy in revenge of an old standing grudge, the coincidence should rouse strong suspicion of the genuineness of his disease.

When this plea of "volitional insanity" is brought into court, the point to be decided is, as Maudsley has said, " whether the impulse was really irresistible or whether it was only unresisted;" or, as the author of the paper expressed it three years earlier[*] " the question to be solved in such a case is not the *difficulty* but the *impossibility* of resisting an emotional impulse. * * * * Frequently the impulse is rendered 'irresistible' by the absence of a sufficiently strong motive to resist it. A well-bred man may preserve his coolness under an insult in the presence of ladies which he would be 'irresistibly' impelled to resist on the spot, is offered elsewhere; and in the case of undoubtedly insane impulse, it may be questioned whether the certainty of legal penalties would not often turn the wavering scale in favor of self-control."

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[*] New York Medical Gazette, May 27, 1871.

Finally, we have the juryman's puzzle invented by Pritchard forty years ago under the title of "moral insanity,"— a title which, as our knowledge of mental disease advances, is less and less frequently met with in medical literature. In such cases we are asked to declare criminals irresponsible, solely because their conduct "appears to be governed by immoral motives which are cherished and obeyed without any evident desire to resist them." Although "their mental resources seem to be greater sometimes than when they were well, and they reason most acutely, apparently, because all their intellectual faculties are applied to the justification and gratification of their selfish desires." But even granting, in any given case, the insanity in other respects of a culprit whose reasoning powers are for the time unimpaired, and who deliberately "gratifies his selfish desires" with capacity to understand the consequences of his acts, the question remains whether he should be exempt from punishment, and this question common sense must answer in the negative. Unless we are prepared— and with much better reason—also to exculpate all sane criminals whose moral sense has been blunted by early training and association.

From all that has been said, it is evident that acts of violence committed, by persons of unsound mind, are divisible into two general classes: those which are purposeless and automatic, or of which the purpose would be justified if the lunatic's premises were true; and those of which are prompted by a conscious intent to do wrong. In the former class of cases, there can be no doubt of the actor's irresponsibility; in the latter, neither ethics nor justice demand his absolution. We might, perhaps, stretch a point in sympathy with the concomitant mental unsoundness, and refrain from inflicting the death penalty; but we should see to it that the intentional criminal lunatic shall not escape punishment altogether, as he is now allowed to do under our statutes. Even when the murder is apparently motiveless, we should insist that the murderer be segregated from the community for life, since all authorities agree that "homicidal mania," from whatever cause arising, is very apt to recur. As it is, our laws (which accept our preposterous verdict that a man may be sane up to the

very moment of committing a purposeless crime and recover
permanent sanity immediately thereafter, being the victim of
brain disease only at the instant of the act) prescribe the farci-
cal formality of sending the alleged lunatic to an asylum
whence he must be speedily released as "recovered." The
difficulty of determining the motive of an action, or even the
existence of insanity, is in many cases very great—in nearly
all insuperable to the unprofessional observer, and there will
always be room for medical experts in medico-legal cases; but
there is no reason why these should confuse instead of simpli-
fying legal proceedings. The wisest plan would be to refer all
testimony concerning mental unsoundness to a permanently
organized commission in lunacy, on whose report an intelli-
gent decision might be reached. A step in the right direction
was made in the New York act of 1874, directing that the plea
of insanity be received only at the time of arraignment, and
empowering the court to appoint a commission of inquiry;
but the appointment of such commission is optional instead of
obligatory, and no guarantee is demanded as to the special
qualifications of the temporary commissioners. Much, there-
fore, still remains to be done to bring law and medicine into
equitable harmony. Meanwhile, jurists and jurymen will do
well to bear in mind that insanity, if genuine, is shown in
other ways than by a single action; that "hypothetical ques-
tions" are useless, each case requiring thorough and painstak-
ing examination on its own individual merits; and, lastly, that
a person whom no physician would hesitate to pronounce in-
sane may, nevertheless, commit crime with a reasonably culpa-
ble purpose.

JAMES APPLETON MORGAN, Esq., said that the speaker (Dr.
Carroll) had alluded to one point which is of great interest to
lawyers, and that is the question of experts in legal proceed-
ings. There is no difficulty which encounters the lawyer and
which can be overcome with greater ease than the difficulty of
procuring experts. It is an ordinary spectacle in court to find
present an equal number of "experts" engaged on both sides
of the same course. He did not think there was a better illus-
tration of this than in the celebrated Wharton case tried at
Baltimore not many years since. That case, however, he

thought was an exception to most cases of the kind, inasmuch as in that case the medical gentlemen engaged testified conscientiously to what they believed to be true. The case was a novel one, and the different experts testified according to the theories held by their respective schools.

He quite agreed with the reader of the paper in his suggestions regarding the appointment of a committee to inquire into the subject. They do those things better in France than they are done in this country. There they have what is called a permanent commission, authorized by the Government, to whom is referred questions involving expert testimony, and whose opinions have great legal weight.

He offered the following resolution, which was carried unanimously:

"That the Society appoint a committee to inquire into and ascertain concerning the system of medical and surgical experts appointed by law and attached to courts of justice, understood to be provided by the laws of France."

Such an inquiry, he thought, would be a credit to the Society; and if such a system were established, he looked forward to the time when it would attract the attention of legislators and others, and ultimately to an engrafting of such a system upon jurisprudence.

The Hon. Geo. H. Yeaman said that the paper opens up to the legal side of the house one of the most important subjects which can occupy the attention of the profession. When he first began to study insanity in its legal aspects, he was inclined to excuse everything done by a man either insane or partially insane. He did not "chip," as the saying is. He was very much astonished to discover that some of the leading alienists went just as far in the other direction, and argued for the punishment of the very people whom the legal profession had been inclined to excuse. He was compelled to say that the argument of the reader of the paper was well stated, and he was led to inquire whether there are not some cases where a little wholesome punishment would result in a benefit to the subject punished.

𝔕eviews and 𝔅ibliographical 𝔑otices.

I.—HERBERT SPENCER: PSYCHOLOGY.

(Continued from January No., 1877, p. 140.)

THE PRINCIPLES OF PSYCHOLOGY. BY HERBERT SPENCER. 2 Vols. 1876.

After the rather remarkable attempt to show the mode of " genesis" of a simple nervous system, which was discussed at some length in our preceding number, Mr. Spencer begins the simple, though not easy task of showing the mode of " genesis of compound nervous systems." After a short reference to the apparent fact, that the production of pigment in the dermal structures of various living beings, is at least partly due to the action of light, Mr. Spencer proceeds to show, how the organs of vision are produced. He says, " the rudimentary eye consists of a few pigment-grains, under the outermost dermal layer, and hence we may infer that rudimentary vision is constituted by the wave of disturbance which a sudden change in the states of these pigment-grains propagates through the body." (P. 532.) Now, what is there to justify such an inference?

There is absolutely no legitimate warrant for it. But this is only one from among hundreds of instances, in Mr. Spencer's works, where important steps are taken, or positions assumed, apparently without a consciousness of the illegitimacy of the procedure.

But to continue. " How such pigment-grains become concentrated in the particular place they may most advantageously occupy, we need not consider at length. Other things being equal, they will develop most where most light falls, and where, consequently, variations of light, caused by adjacent things, are strongest; and since a close cluster of pigment-grains, when affected, will send through the body a more efficient wave of disturbance, *natural selection will further the concentration*—there will be a survival of individuals, in which the approximation is greatest, ending in the formation of an integrated patch. The pre-existence of a simple nervous system

being *assumed*, let us consider what will happen when incipient vision is added." (P. 533).

Will the reader kindly follow us for a moment in a scrutiny of this statement?

"Incipient vision " is assumed, as well as a " simple nervous system," to which latter, visual impressions are to make their way, from the " pigment-grains," which constitute the rudimentary eye. Though it may be a fact that pigment-grains "will develop most where most light falls," yet that the necessary parallel fact, that, in consequence of there being more pigment in the site of the rudimentary eye than elsewhere, in the same proportion, more light must have fallen on that part than on others, is not only not established in any case, but wholly improbable, and, indeed, incapable of proof, must be remembered. This latter fact, so quietly assumed, stands in a causal relation to the former, and hence has precedence in order of time, and in that of a true logical sequence. The fact which is mentioned, depends on the fact that is assumed, and which is not only assumed but improbable. No one can be permitted, in behalf of Mr. Spencer, to evade this point, by replying that the accumulation of the pigment-grains at the seat of rudimentary vision depends on "other things," as well as the stimulus of light, in the absence of *proof* that anything else was concerned in the matter. Such a course involves a logical subterfuge unworthy of the name of sound reasoning.

But admitting these prior assumptions, what proof is there that when light falls upon the accumulation of pigment-grains, it will "send through the body " a " wave of disturbance," presumably along the track of future visual impulses? We have no hesitation in declaring that there is not a single fact which even gives strong indirect support to this assumption. Finally, having got over all the difficulties in the way of the original acquisition of the rudimentary eye, in the manner shown, what evidence is there to show that " natural selection *will further the concentration*" of granules, &c., which lead gradually, by a survival of the fittest, to the development of a perfect eye? There is none. But before passing on, we desire to offer a few remarks on the true office of " natural selection."

The word selection always signifies a choice, between alternatives. It implies always two or more things, one of which, for example, is, for some reason, *selected* in preference to another. If we speak of a selection from among plants or animals, it is indisputably implied that there are *two or more kinds*, from which aggregate a selection is made. The ordinary plain meaning of the term is not impaired by reason of any peculiarity of the agent or means for making the selection. For example, because it is called *natural*, it does not cease to be selection. By this it is meant, that those plants and animals, which are best fitted by their peculiarities of size, strength, endurance, &c., to live, under the common circumstances of their lives, will stand

the best chance, in the long run, to survive and propagate their kind, while others, less fortunate, in the particulars named, are placed at a disadvantage, and sooner or later perish. This so-called principle may enable us to explain why one kind has survived, while another has perished. But it seems to be overlooked too often by those who discuss the subject, that this case does not touch the vital point of inquiry. The real point is to explain how the same kind or class of beings came to be individually so different, that a selection is made possible? Selection did not *create* the differences or individual peculiarities, which alone render it possible. It can choose, as it were, between them, when they have been produced, but it would be absurd to declare that the differences *were produced* by natural, or any other kind of selection. Now, what is it that *produces*, —not *chooses*, or even *perpetuates*,—the differences between plants or animals, differences which render some more, others less, fitted to live under certain circumstances?

Before selection can appear on the scene, all the real difficulties of the case have been surmounted. But how? By a mere assumption. For we find on every page of the writings of certain authors, that "natural selection," "produces," "creates," "forms," etc., for example *an eye.* By all means let the reader, however familiar he may be with such topics, pause and reflect, and see whether he can be satisfied that natural or any other kind of *selection* has anything whatever to do in *producing* the differences between animals, or between plants, after the appearance of which alone selection can come into play. This whole passage, concerning the mode of development of the organs of vision, is simply a tissue of ingenious but almost valueless assumptions. And the same judgment must be passed on much that is contained in the chapters on the genesis of "compound" and "doubly-compound" nervous systems. It is true that they contain many ingenious speculations, rather than facts, in regard to the inner development of the central nervous system, but they are without either practical or scientific value, so far as we can see. Mr. Spencer himself appears to be fully aware of the character of that part of his work under discussion. He says, "in seeking to build up a general conception of the process of nervous evolution, in its higher stages, I have elaborated the argument quite far enough perhaps too far. Let me, indeed, disclaim the endeavor, which some may suppose I am making, to explain the process in full. My purpose has been rather to make the possibility of such a process conceivable, and I have taken specific cases and used concrete language, because so only could I make myself understood. The actual genesis has been much more involved than that which I have described—so involved that *a true delineation, even if it could be made,* would be scarcely comprehensible." (P. 557).

These latter admissions are certainly true. But if so, why

occupy so much space and consume so much time, in trying to do what cannot be done at least, in the present state of our knowledge? What we object to in general, in Mr. Spencer's work, from beginning to end, is the extraordinary preponderance of hypothesis, or assumption over fact, and cautious reasonings on the same. And for ourselves, we cannot be misled in our endeavor to estimate the true value of such work, by any plea, in behalf of the place and function of speculation in the course of science. Its true use is a different thing from its habitual abuse, and when comprehensively surveyed, it must be alleged against the writings of Mr. Spencer, as we have said before, that in his use of hypothesis he habitually trespasses on the grounds forbidden to legitimate inference.

The cerebellum is described in general terms, as an organ for "doubly-compound co-ordination" of space-relations, and the cerebrum as an organ for the "doubly compound co-ordination" of time-relations.

What is the meaning of these phrases? By co-ordination is meant, of course, in this case, the simultaneous and equal appreciation of various relations, not only those which happen in the same place and time, but in the past, taken in connection with the present. One object is recognized in its space-relations, or event in its time-relations, as regards other positions in space, and other periods in time past, or even to come.

According to Mr. Spencer, not only is each half of the brain, the seat of appreciation if we may so speak, of these compound relations, but each half has perfectly similar functions, and hence the action of the brain is not only "compound," but "doubly-compound." But can we by judicious analysis, subordinate all the functions of the cerebellum to the category of "space-relations," and those of the cerebrum to that of "time-relations?" For ourselves, we unhesitatingly say no. In this case we have conspicuously shown Mr. Spencer's loss of balance as between *quantitative* and *qualitative* relations, and on the side of the former. It is seen everywhere in his writings. There is a dominating tendency in his analytic procedure to reduce everything to terms of matter and motion, even within the most interior domains of biology and psychology. Certain it is that the synthetical summaries of the cerebellar and cerebral functions, made by Mr. Spencer, will not endure the test of even a superficial analysis, in the presence of the facts of the case. But we cannot in this place do more than challenge the adequacy of his generalizations in relation to the functions of the higher parts of the nervous system.

But in chapter VI. Mr. Spencer descends in detail, to a description of the mental functions of the nervous system. To this chapter we will now direct the attention of the reader.

Reflex action affords the starting point in the exposition. This is carried up from simple to complex reflexes. But as valuable a discovery as that of reflex action in nerve physiology

has been, its utility is habitually over-estimated. By its faith-
ful application, very many phenomena have been and may be
explained, that were formerly supposed to belong to a different
category. But it should be remembered always, that reflex
nervous actions, however complex they may be, are made up of
an aggregation of simple reflexes, which may be brought to light
by judicious analysis. The sphere or prerogatives of reflex
action, are not enlarged by mere numerical, or serial complica-
tions, for it does not cease by such procedures to be reflex action.
All there is in it, is seen essentially in its simplest manifestations.
These remarks are none the less true, when it is admitted,
that all parts of the nervous system may be the seat of reflex
action—the cortex cerebri, as well as the spinal cord. For it is
one thing to admit its general prevalence throughout the
nervous system, and it is quite another, to declare that it ex-
cludes other kinds of action, in a part, which is its seat.

These remarks are made, not so much in view of Mr. Spen-
cer's mode of treating this subject, as in view of a very general
tendency among physiologists, in discussing the functions of
the nervous system, to transcend the legitimate sphere of reflex
action, in their applications of it to the same.

But to pass on. In the endeavor to establish the true rela-
tions of mental faculties and structure, in the higher parts of
the nervous system, Mr. Spencer uses the following language:

" Every mental faculty, rightly understood, is an internal
plexus of nervous connexions corresponding to some plexus of
relations among external phenomena that are habitually expe-
rienced." (P. 574.) The former is developed in any particular
case, according to Mr. Spencer, wholly at the instance of the
latter, except in so far as they may have been acquired by heredity.
But if we should ask how the ancestors of any given being came
by the nervous mechanisms they have, and inquire far enough
back, along the line of descent, we will find that they were pro-
duced wholly by the play of external stimuli upon the original
"colloid" or protoplasm. All internal changes in structure, when
hunted down to their ultimate causes, were *produced* by external
physical agencies, and when once produced they have been
faithfully transmitted by heredity, if encouraged by favoring
external circumstances. But this matter of heredity, itself, is
nothing but another result of the action of external physical
agencies. Because if the peculiarity evoked in an organism, by
a certain set of external agencies, meets with adverse external
circumstances, then it simply is repressed or perishes, to give
way to another peculiarity to which the existing external
influences may be more friendly.

These may be said to be the two capital features of Mr.
Spencer's system of psychology, viz.: The original acquisition
of all nervous structure, by the play of outer physical agencies,
at first upon a structureless creature, and the subsequent per-
fection of the same in the course of untold periods in time, and

by numberless changes in external relations, aided by the principle of heredity. These two principles, which involve also, the law of the " survival of the fittest," furnish the clues to Mr. Spencer's system. We shall presently examine these generalizations briefly, and hence for the time dismiss them. They are called up at present, to enable us the more readily to comprehend the statement, which we are now to pass under review.

In psychology, one of the chief subjects for discussion, is that process, by means of which in its totality, we obtain our knowledge of what is called the " outer world." No subject within its domain has given rise to more discussion. Now what is perception, according to Mr. Spencer? says he:

"Suppose such an animal as we have been considering, sees approaching some small creature of the kind on which it preys. Then, while this small creature is coming nearer, but before it has reached the point at which its *visual image arouses the reflex action that effects its seizure*, a series of visual images, increasing in size and definiteness, must be yielded by it, and it must yield an accompanying series of stimuli to the eye-muscles. Though the reflex action takes place only when the retinal and muscular impressions become combined in a certain way, yet during approach to the required combination, the reflex action is tending to arise, there is a gradually-increasing excitement of the nervo-motor apparatus, which will presently perform reflex action. The effect does not stop here. Through the established connexions there is propagated a gradually-increasing excitement of the nervo-motor apparatus which catching the prey will bring into play—there are produced faint revivals of the tactual and gustatory states which capture of such prey has on past occasions yielded. Thus then results, what we call *peception*; [perception] for we have here a cluster of real feelings caused by the presented object, joined with a cluster of ideal feelings, representing certain other real feeling which the object has before produced, and can again produce." (P. 561.) Or again, "a perception is formed only when a cluster of real feelings excites a correlated cluster of ideal feelings." (P. 563.)

Here it will be noted by the reader, that the same remarkable fondness for assumptions is displayed that we have discussed at length, elsewhere. We do not forget that Mr. Spencer is introducing examples such as we have quoted, largely for purposes of illustration, nor that in this volume, he is dealing professedly with "objective psychology." But can it be shown, has it ever been shown, that the catching of a mouse, say by a cat, is *simply a reflex action*, only this and nothing more, as Mr. Spencer more than implies in the passage quoted? Does *perception*, imply anything beyond reflex action?

On all hands, it is admitted to imply feeling, which, in its ordinary sense, at least, is not involved in simple reflex action, though it may be, as it is in that class of reflexes, known to some as "sensori-motor." But does not perception imply more than *mere feeling*, or simple apprehension of some particular condition of the sensory nervous apparatus? According to Mr. Spencer's account, a perception is composed as follows: " We have," says he, speaking of perception, "a cluster of real feelings, caused by the presented object, joined with a cluster of ideal feelings, representing certain other real feelings, which the object has before produced, and can produce again."

Nothing but "feelings," are found in this catalogue of the component elements of perception.

To group feelings into clusters, does not make of them anything but feelings. It is hardly possible under the circumstances, to suppose a sort of chemical combination among them so as to obtain by their combination a compound, as we may in chemistry, having qualities or properties different from what the elements entering into it seemed to have before they were combined. Though a member of an assumed "cluster," each separate feeling retains its characteristics, much the same as if it existed alone, though there are some apparent, rather than real, exceptions to this statement. But by what means are these "clusters of feelings" formed? By "co-ordinating plexuses," according to Mr. Spencer. We will shortly consider the idea of "co-ordinating plexuses," which, we believe, on anatomical and physiological grounds to be well founded, but to be, to some extent, illegitimately used by our author.

We know, perhaps, quite as well as we need to know, of the great difficulty, not to say, impossibility, which lies in the way of drawing a satisfactory distinction between *feeling* and *knowing*, or knowledge. It may, perhaps, be truly said that it is not possible to know without feeling, though we should greatly hesitate to declare that it is impossible to feel without knowing. But it would hardly seem to be a question with Mr. Spencer. It is feeling from first to last, the differences being those of number and complexity of grouping. Knowing is simply a more refined and complex form of feeling. There is no real distinction, except for figurative purposes, between feeling and thought. At any rate, this is the impression very naturally gathered by a perusal of his writings.

For our own part, we maintain a distinction is to be made, as between thought and feeling, closely as they are related in experience, and that both these elements enter into perception, and hence that it is inadequately treated, and that the process is not faithfully delineated by Mr. Spencer, in any part of his writing. But we are not able to treat this subject in the present notice as it deserves to be, but hope to do so, in the near future, in a work on the "Philosophy of Perception."

But, says Mr. Spencer, "we may now pass from perceptions to ideas properly so called. Though every true perception, along with the presentative feelings, necessarily contains certain representative feelings, these do not, at first, become what are usually understood by ideas. *They have not the detachableness* which distinguishes ideas that are fully developed. *   *   *   *
When do ideas, rightly so-called, arise? *They arise when compound co-ordination, passes into doubly compound co-ordination,*
*   *   *   * They are the necessary concomitants of that process by which thorough intercalated psychical states, there is established a mediate relation between psychical states that can not be brought into immediate relation. And they have for

their seat those intercalated plexuses, which co-ordinate the co-ordinating plexuses previously existing." (P. 565.) We begin with simple sense impressions, and simple reflex action. Then we advance a step higher, to another class of centres, or " plexuses," which are capable of taking up into themselves the primary impressions made on the lowest class of plexuses. This second class of " plexuses" co-ordinate and combine simple sense impressions. The impressions made on this second order of plexuses is transmitted to a higher set of plexuses, where they, too, are co-ordinated. This is " compound co-ordination," as the former was " simple." This " compound co-ordination" it is, which yields " perception." Next in order, if not finally, we have a still higher class of " co-ordinating plexuses," which work up the result arrived at in " compound co-ordination," and this is " doubly-compound co-ordination," and the result is *ideas*.

But what is an *emotion?* " The plexuses which co-ordinate the visual impressions yielded by an apple on the table, with the motor act, required to grasp it, and with the ideas of tactual and gustatory sensations it will yield, are nearly the same as certain plexuses that have before worked together. * * * * Each plexus has been inherited in the form of a well-organized set of connexions, obscured by multitudinous feeble connexions, and the inherited central connexions of the plexus first excited, are definitely connected with the inherited central connexions of the similarly constructed plexus that is habitually excited after it. The accompanying subjective results are these: The consciousness of an approaching body, making sounds and motions of a certain kind, is followed by a consciousness of painful states, sensory and motor, having no definite localizations, The immediate perception, with the crowd of ideas, resulting from preceding similar perceptions, arouses not only ideas of particular pains that have followed such perceptions in the life of the individual, but through the inherited organization it arouses an indefinite sense of ill—a cloud of dim feelings of suffering that cannot be reduced to form, because they have not been experienced—the *emotion of fear*. And with the primitive form of fear, thus physically organized and psychically constituted, there are afterwards integrated the higher and more involved forms of fear; all of which have for their central element, ideal feelings of pain or discomfort that are unlocalizable, and therefore vague.

Respecting emotions, it has only to be added that they, like ideas, result from the co-ordinating actions of the cerebrum and cerebellum upon the *medulla oblongata and the structures it presides over.* * * * .* *The medulla being the seat of all feelings, whether aroused from within or without, etc.*" (P. 571-2.)

From the foregoing extracts and considerations, the reader may see what are Mr. Spencer's views as to the real nature of such mental states and products, as perceptions, ideas, emotions, etc.,—from a physiological standpoint.

But we will postpone remarks on the adequacy of Mr. Spen-

cer's interpretation of psychological phenomena from the phys-
iological point of view, until we have what are his interpreta-
tions of the same phenomena from psychological points of view,
to which we are soon to pass briefly,

Before leaving the physiological side of our subject, we desire
to offer a few remarks, on the notion that the medulla is the·com-
mon seat of feeling and emotion. A few eminent physiologists
have held to this view, and as we have seen, it is adopted by Mr.
Spencer. It is supported by the high authority of M. Vulpian.
This opinion was discussed, to some extent, in our last number,
in a review of Professor Ferrier's book, the "Functions of the
Brain." We cannot do more at present, than to express a cer-
tain degree of surprise, that it should be seriously maintained, in
the present state of our knowledge, that the *medulla oblongata*
is the seat of the emotions, or the highest forms of feeling. The
cerebral cortex, it seems to us, all the real evidence points to as
their seat. We predict that the time is not far distant when but
few, if any, will venture to maintain any other view. But we
cannot, in this place, enter into an adequate discussion of this
subject.

We desire, also, to call attention to a passage in regard to the
localization of function in the brain, which will serve to show,
as well as any other, the suggestiveness of many parts of Mr.
Spencer's writings. It is as follows:

" Whoever," says he, "calmly considers the question, cannot long re-
sist the conviction that different parts of the cerebrum must, *in some way
or other*, subserve different laws of mental action. Localization of func-
tion is the *law of all organization whatever*, and it would be marvellous,
were there here an exception. If it be admitted that the cerebral hemis-
pheres are the seats of the higher psychical activities, if it be admitted
that among these higher psychical activities there are distinctions of kind,
which, though not definite, are yet practically recognizable, it cannot be
denied, without going in direct opposition to established physiological
principles, that these more or less distinct kinds of psychical activity
must be carried on in more or less distinct parts of the cerebral hemis-
pheres. To question this is to ignore the truths of neuro-physiology, as
well as those of physiology in general. It is proved experimentally, that
every bundle of nerve-fibres and every ganglion has a special duty, and
that each part of every such bundle, and every such ganglion has a duty
still more special. Can it be, then, that in the great hemispherical gan-
lia alone, this specialization of duty does not hold? That there are no
conspicuous divisions here is true, but it is also true in other cases, where
there are undeniable differences of function—instance the spinal cord, or
one of the great nerve-bundles.

Just as there are aggregated together in a sciatic nerve, an immense
number of fibres, each of which has a particular office, referring to some
one part of the leg, but all of which have for their joint duty, the manage-
ment of the leg as a whole; so, in any one region of the cerebrum, each
fibre may be concluded to have some particular office, which, in common
with the particular offices of many neighboring fibres, is merged in some
general office fulfilled that region of the cerebrum.

Any other hypothesis seems to me, on the face of it, untenable. Either
there is some arrangement, some organization, in the cerebrum, or there
is none. If there is no organization, the cerebrum is a chaotic mass of
fibres, incapable of performing any orderly action. If there is some

organization, it must consist in that same "physiological division on labor," in which all organization consists; and there is no division of labor, physiological or other, but what involves the concentration of special kinds of activity in special places. (P. 573–4.)

These statements we believe to be strictly true, and to have anticipated in a remarkable manner the best results of subsequent research into the anatomy and physiology of the brain. It is in striking contrast with the singular and seemingly chaotic doctrines in respect to the same subject, enunciated during the past few years by Dr. Brown-Sequard. But the subject of localization of function in the brain will receive rather extended notice in our next number in the concluding portion of our review of the work of Dr. Ferrier. To what is there to be said, we would invite the attention of the reader.

It will be hardly possible for us in our brief notice, to enter at greater length into Mr. Spencer's mode of treating psychological themes from a physiological point of view. Before we close we shall recur once again to this subject.

What are Mr. Spencer's doctrines in regard to the *nature* of mind? Does it have, substantially, a separate existence from the nervous organization, with which, if it is a separate entity, it is intimately associated during the continuance of corporeal life, or is it simply a name for the aggregate of the higher functions of the nervous system, as contraction is the name for the function of muscle? In speaking of the "composition of mind," it is said "the proximate components of mind are of two broadly-contrasted kinds—Feelings, and the Relations between feelings."

But what is a feeling? It "is any portion of consciousness which *occupies a place sufficiently large to give it a perceivable individuality;* which has its individuality marked off from *adjacent portions of consciousness,* by qualitative contrasts, and which, when introspectively contemplated, appears to be homogeneous. These are the essentials." (P. 164, vol. I.) The "relations" spoken of as the other class of ultimate components of mind, are but a sort of feeling, for it is said that "it is true that, under an ultimate analysis, what we call a relation proves to be *itself a kind of feeling.*" In the final analysis, therefore, it appears that mind is "composed" solely of *feelings.* This is essentially the position of Hume, not to mention that of other members of the Lockean School.

One thing is quite noticeable in these and many similar statements,—the carrying of chemical and spatial conceptions into discussions of consciousness.

But to pass on from questions as to the "composition" of mind, what shall be said as to its "substance," its substantial independence of the nervous organism, which some regard as its instrument, during the corporeal life of the individual? Mr. Spencer devotes to this subject a rather remarkable chapter, and to some of the statements contained in it we invite the attention of the reader.

Mr. Spencer is a phenomenalist, apparently, of a rather pronounced type. In certain parts of the chapter on the "substance of mind," it is distinctly asserted that we know nothing of mind, and can know nothing of it. He says: "To write a chapter for the purpose of showing that nothing is known or can be known of the subject which the title of the chapter indicates, will be thought strange. It is, however, in this case needful," etc. (P. 145). It is the old story of the phenomenalists,—all we know is the phenomenon,—that which causes it, or gives it birth, we cannot know. Phenomena appear in the physical world, and they are referred to what we call matter, or within consciousness, and are referred to what we call mind, but it is the high office of certain forms of philosophy, overlooking the bases, and discrediting the value, of inference, to insist we do not, and can not know anything of either. They are radically inaccessible to our faculties. All we know, or can know, as Berkley declared in respect to matter, and Hume, for mind as well as matter, is our impressions and ideas,—only these and nothing more. And among the followers of Berkley and Hume, we may unhesitatingly range Mr. Spencer.

But here arises one of the many difficulties which stand in the way of a critical estimate of the value and tendencies of Mr. Spencer's labors.

It so often happens that what is said at one time is apparently conceded, or even contradicted, at another. In the chapter now under consideration—"The Substance of Mind"—he says: "*Mind* is, certainly in some cases, probably in all, *resolvable into nervous shocks*, and these nervous shocks answer to the waves of molecular motion that traverse nerves and nerve centres. Thus, not only is the substance of mind supposed to be knowable as having this universal character, but it is closely assimilated to, *if not identified with nervous shocks*." But after these and many other like declarations, Mr. Spencer writes in the same chapter as follows: "The foregoing reasoning brings us no nearer to a final solution of the question. Even could we succeed in proving that mind consists of homogenous units of feeling, of the nature specified, we should be unable to say what mind is. * * * * The reduction of all the more complex forms to the simplest form, leaves us *with nothing but this simplest form*, out of which to form thought; and thought cannot be framed out of one term only. Representation and re-representation of this ultimate unit of consciousness, *in terms of itself*, leaves us at last just where we were at first. * * * * When the two modes of being which we distinguish as Subject and Object, have been severally reduced to their lowest terms, any further comprehension must be an assimilation of these lowest terms to one another; and, as we have already seen, this is negatived by the very distinction of subject and object, which is itself the consciousness of a difference transcending all oher differences. * * * * Can

we think of the subjective and objective activities as the same?
Can the oscillation of a molecule be represented in conscious-
ness side by side with a nervous shock, and the two be recognized
as one? No effort enables us to assimilate them. That a unit of
feeling has nothing in common with a unit of motion, becomes
more than ever manifest when we bring the two into juxtaposi-
tion. And the immediate verdict of consciousness thus given,
might be analytically justified were this a fit place for the need-
ful analysis.   *   *   *   *   Here, indeed, we arrive at
the barrier which needs to be perpetually pointed out, alike to
those who seek materialistic explanations of mental phenomena,
and to those who are alarmed lest such explanations may be
found. The last class prove by their fear, almost as much as
the first prove by their hope, that they believe mind may be
possibly interpreted in terms of matter, whereas many whom
they vituperate as materialists, are profoundly convinced that
there is not the remotest possibility of so interpreting them.
   *   *   *   *   It may be as well to say here, once
for all, that were we compelled to choose between the alterna-
tives of translating mental phenomena into physical phenomena,
or of translating physical phenomena into mental phenomena,
the latter alternative would seem the more acceptable of the two.
Mind, as known to the possessor of it, is a circumscribed aggre-
gate of activities, and the cohesion of these activities, one with
another, throughout the aggregate, *compels the postulation of a
something of which they are the activities.*" (P. 158–9, vol. I.)

These admissions are certainly remarkable when taken in
connection with other deliberate statements of Mr. Spencer.
For our own part, we fully agree with them. If we had to
choose either of the alternatives mentioned to the exclusion of
the other, we would certainly choose with Mr. Spencer. But
we have never felt ourselves to be in the position which such a
choice would imply.

We hold mental and physical phenomena to be at bottom of
entirely different orders. They cannot by any legitimate, and
hence rational procedure, be analyzed the one into the other, or
both into some *tertium quid,* in which they may coalesce in
substantial unity. It has never been done, and so far as we can
see it never can be.

As to the proof of the existence of what has been called Mind,
there is no direct evidence outside of the phenomena of con-
sciousness if there is there. But when we consider in their
completeness mental phenomena, we are compelled, as Mr.
Spencer says, to the "postulation of a *something of which they
are the activities,*" at the point of a cogent inference, or series of
inferences. The phenomena imperatively require some such
agent or cause, as mind is said to be. And the kind of evidence
which leads to this conclusion is just as valid, or may be, as is
the so-called direct evidence. There is much to be learned yet,
as to the real function and value of *Inference,* in the domain of
the natural and physical sciences.

But though we know two groups of phenomena, " physical " and " mental," acknowledged to be radically distinct from each other, and though we seem to be compelled to postulate a pair of substantial entities, to which they respectively belong. yet it is held by Mr. Spencer in strict conformity to the dictates of phenomenalism, that we neither do nor can know anything of either. They are relegated to the "unknowable." This is exactly the position of Hume. Mr. Spencer not only feels " compelled " to admit an unknowable, substantial basis on the one hand for physical, and on the other for mental phenomena, but beyond these, a single form of " Unconditioned Being common to the two."

He says, "though of the two it seems easier to translate so-called matter into spirit, that to translate so-called spirit into matter, (which latter is indeed wholly impossible,) yet no translation can carry us beyond our symbols. Such vague conceptions as loom up before us are illusions conjured up by the wrong connotations of our words. The expression " substance of mind," if we use it in any other way than as the x of our equation, inevitably betrays us into errors, for we cannot think of substance save in terms that imply material properties. Our only course is to recognize our symbols as symbols only; and to rest content with *that duality of them which our constitution necessitates*. The unknowable, as manifested to us within the limits of consciousness in the shape of feeling, being no less inscrutable than the unknowable as manifested beyond the limits of consciousness in other shapes, we approach no nearer to understanding the last by rendering it into the first. The conditioned form under which being is presented in the subject, cannot, any more than the conditioned form under which being is presented in the object, be *the unconditioned being common to the two*." (P. 161-2, Vol. I.)

This not only puts the " substance of mind." and the " substance of matter," beyond the actual, or even the possible sphere, of human knowledge, but merges them into an unconditioned, and unknowable something that lies beyond, in the bottomless abyss of our absolute and everlasting ignorance. This is a stretch of philosophical humility, not to say candor, to which we have never yet been led, and we devoutly hope we may never be. Either there is some peculiar meaning given to the word " knowledge," which we do not fully comprehend, or we must, and we do contend, that we *know something* of matter, and *something* of mind. And we make this declaration in tolerably full view of the course and results of speculation concerning these subjects in the past. But we cannot, in this already long notice, do justice to such topics. But we expect to discuss them at length in a work which has occupied no small portion of our time and thought for many years past.

We have no time or space in this present notice, in which to examine in detail the remaining contents of these two goodly

volumes, and in which we have the nature and modes of action of the various mental faculties considered, often with wearisome minuteness of speculative detail, and in which the modes of acquiring our various forms of knowledge are exposed at length, in attempted conformity to the law of evolution, and by the aid of a highly refined and problematical *physics* of the nervous system, which latter point, however, we have considered at some length in the earlier part of this notice.

There are two capital features, amongst a few others, in Mr. Spencer's psychological system. They are,—first, that not only all changes toward complexity or perfection, but the original genesis of a nervous system, is dependent on *external physical agencies*, and their varying modes of action. The inner mechanism, and its action, are alike incidental to the action of such agents. The impulse to organization is originally from without. The forces set in play are from without. The "tendency to vary" is from without, since it depends in a final analysis of its conditions, on variations *in external causes*, as do the specific variations in structure and action of the nervous mechanism. Except in a secondary sense, nothing is from within. The internal structure is but an organized history of the action in space and time, of matters external and related to the animal organism.

The second feature to which we have alluded, is that of *heredity*. To this so-called principle is committed every change, actual or hypothetical, which rises into view in the course of a progressive evolution. And this in the face of the teachings of experience, that very many acquired peculiarities of structure, and hence of action, are not perceptibly transmissible. But neither of these features were originated by Mr. Spencer. They have been long familiar to those who have given themselves to the study of the phenomena of living beings.

Mr. Spencer has not established any important facts or laws in the province of nerve-physiology or psychology proper. But he has carried physical conceptions and quantitative relations, almost throughout the domain of both these sciences, as we must persist in calling them, and with results, which, in our judgment, are almost valueless, when compared with the labor expended in procuring them. Mr. Spencer's work, in this latter phase, differs from all its predecessors, in the thoroughness and minuteness of its elaboration. In his application of physical conceptions and a physical nomenclature to vital and mental processes, he does not content himself with generalities, but enters with remarkable particularity into his work. But after all, we are unable to point out to our readers any substantial results, of practical value, arising of Mr. Spencer's labors in the psychological field, except in the way of their suggestiveness. In this latter respect they are worthy of high praise.

II.—DISEASES OF THE NERVOUS SYSTEM.

HANDBUCH DES KRANKHEITEN DES NERVENSYSTEMS, XI. ERSTE
HAELFTE: von Prof. H. Nothnagel, in Jena; Prof. F. Ober-
neier, in Bonn; Prof. O. Heubner, in Leipzig; Prof. G. Hu-
guenin, in Zurich, and Prof. E. Hitzig, in Zurich. Mit 5
Holzschnitten. Leipzig, 1876. 819 pages. (*Hand-book of
Diseases of the Nervous System.*)

By far the most lengthy, and in some respects the most valu-
able portion of the book is the memoir of Huguenin, of Zurich,
on the acute and chronic inflammations of the brain and its
membranes. This we presume in substance is the same as
a part of the great work of Dr. Huguenin on the general
pathology and diseases of the nervous system, the anatomical
introduction to which was published several years since, and re-
viewed in this JOURNAL for October, 1874. We had occasion in
that notice to speak with approval of the author's methods and
thoroughness, and the general character of his work as far as it
was then shown, and we need not add many generalities of that
nature in this place, but can pass at once to note some points of
interest in his accounts of the various affections of this class.

Commencing first with the inflammation of the dura-mater,
after a few anatomical remarks, the author takes up the subjects
of pachymeningitis externa and interna. The first of these two
belongs almost entirely to the province of surgery, still there
are some features that call for attention from the physician and
the neurologist. Such are the chronic pachymeningitis of ad-
vanced age, which has been discovered after death to have
corresponded with certain vague cerebral symptoms. Its prac-
tical importance, however, is comparatively slight. The case is
very different with that form of the disorder connected with
caries of the temporal bone and otorrhœa, the importance and
even the frequency of which are probably much underrated by
the majority of practitioners. It is only quite recently that the
importance of ear troubles as regards life insurance was a sub-
ject of numerous communications in one or two prominent
English medical journals, as if it was an entirely new thing,
and there are, we believe, companies in this country that make
no special account of it in their examinations.

Hæmatoma of the dura-mater, or pachymeningitis interna
hemorrhagica, is given quite a lengthy consideration and its

pathology and pathological anatomy is very fully discussed.
Dr. Huguenin, after noticing the various opinions contained in
the literature of this affection, gives a detailed description of the
anatomical conditions observed by himself, and for these con-
cludes, in opposition to Virchow and the majority of the other
authorities, that it does not commence with alterations of the
dura, and that the hemorrhagic products are not derived from
that membrane.   His reasons for this opinion are laid down as
follows:

a. Notwithstanding abundant opportunities, he has never
been able to see any trace of the initial inflammation of the
dura-mater.

b. The dilatation of the middle meningeal artery, described
by Kremiansky, was also never visible.

c. From the beginning, up to the completion of the vessels
in the new membranes, and the establishment of the circulation
in the same, the epithelium of the dura was always found in-
tact.

d. The initial phenomenon is not the formation of connec-
tive tissue, but is a simple flake-like extravasation of blood.

e. This goes through the usual processes of coagulation.

f. The same produces also the new organization that is seen
later.   The function of the *Wanderzellen* that appear in all cases
is a matter for determination by future researches.

According to Dr. Huguenin's views, therefore, the hemor-
rhagic particles in this affection are simply the results of extrava-
sation from diseased and ruptured small vessels, which in the
ordinary conditions of this disease, organizes and unites to the
membrane, and later, increasing in thickness by new extravasa-
tions, it becomes united also to the arachnoid.   Its immediate
causes are various, but excepting from external injury, the
author holds it can never occur in perfectly sound persons in
whom the previous vascular alterations are not present.   Its
prognosis, he says, is not absolutely unfavorable, though re-
covery after extensive extravasation of this kind is of rare
occurrence.   He therefore advises active antiphlogistic measures
during the acute stage, when it is diagnosed, and treatment on
general principles after this stage if it is survived.   The prog-
nosis, however, is still unfavorable.

Affections of the pia mater, forming as they do the great mass
of meningeal troubles, have a proportionate space allowed them
and take up by far the larger portion of Dr. Huguenin's
memoir.   And in this connection it may be worth mentioning
that he rejects absolutely any such disorder as arachnitis as an
independent affection, though he admits that the arachnoid
may become secondarily affected.   The frequent usage even by
recent writers of the term *arachnitis* makes this point worthy
of attention here.   The too common use of the word to cover
every kind of meningeal inflammation, except that of the dura
mater, to which we have before alluded in this JOURNAL, is

based upon a false pathological idea, and should no longer be sanctioned. Nor is there, it seems to us, any good reason for the use of the words under any circumstances whatever, the secondary involvement of this membrane in inflammations of the pia, certainly is not sufficient to justify it.

It is absolutely impossible to condense all the matters of interest in this memoir within the limits of any review, that is not nearly as long as the original article. Dr. Huguenin writes in such a compact and condensed manner, that the whole of the pages in this volume, from his pen. seems almost like a condensation from a still more lengthy work. though in fact it is one of the fullest treatises on its subject with which we are acquainted. We can only therefore notice the general topics. and more particularly a few scattered points here and there, giving the rest only our unqualified admiration and commendation.

The discussion of the subject of hyperæmia of the pia mater, a condition of much pathological importance in children. and more or less so when occurring at any period of life, is very full and satisfactory. but hardly admits of special mention of particular points. We notice, however. a statement in the description of the symptoms of the affection in children that bulging of the fontanelles does not occur, the increase of the skull contents not being sufficient to produce this symptom. We judge this to be a too sweeping statement altogether, one that might lead to a more unpromising diagnosis than ought to be made in some cases. for we believe that cases do occur in which this particular phenomenon appears. that are only fluxionary hyperæmia, not yet reaching the condition of actual inflammation.

The inflammations of the pia mater are treated at great length, and with equal thoroughness with the other subjects. About two hundred and ten pages, in all, are occupied with the consideration of these affections, which the author classifies as follows:

I. The special non-tuberculous affection of infancy. characterized by effusion into and dilatation of the ventricles, for which, since the name "hydrocephalus" indicates only the "anatomical process, Dr. Huguenin proposes the designation Leptomeningitis Infantum."

II. Tuberculous meningitis. a disease affecting all ages and dependent upon the development of miliary tubercles in the pia. Its anatomical components are:

a. Miliary tubercles in the pia, varying as to numbers and age.

b. Inflammation of the pia most pronounced at the basis, but still not constant then. The inflammatory symptoms may be very insignificant.

c. Hydrocephalic effusion, not present, however, in all cases.

d. The consecutive cortical involvement.

III. Purulent leptomeningitis, without miliary granulations,

with purulent exudations at different points on the membrane, and sometimes also with well-developed hydrocephalic effusion. This evidently includes a number of etiologically very different conditions, and with our present knowledge they may be classed as follows:

1.　Basal meningitis, with pronounced ventricular effusion, a rare disease of adults and children, of rather protracted course and obscure origin.

2.　Spontaneous purulent meningitis of the convexity; called spontaneous from ignorance as to a cause.

3.　Traumatic meningitis.

4.　Meningitis arising from the propagation of suppuration from neighboring parts (caries of the bones, especially of the temporal bone, etc.)

5.　Metastatic meningitis, that is, the form accompanying suppurative processes in distant organs (peritonitis, puerperal fever, erysipelas, pyæmia, etc.).

6.　Epidemic cerebro-spinal meningitis, which the author considers as plainly belonging to the infectious diseases, and hence does not consider here.

We have given this classification in full, chiefly because it is a little more detailed and somewhat different from those usually adopted, and because it seems to indicate as concisely as possible the authors views as to the etiology and nature of these disorders. As we cannot, for reasons already explained, go into any thorough discussion of details of the memoir, such a general statement of views as is expressed in such a classification is alone of considerable value. And when, as in the case of the work before us, condensation is absolutely impossible, it is almost all that can be given. We shall have to refer the reader to the section itself, a translation of which will shortly appear, for any adequate idea of its contents, it is, beyond question, the most thorough discussion of the whole subject of meningeal in-inflammations that has ever appeared in any text-book of medicine. A number of well-detailed clinical histories, illustrative of these different forms of disease, accompany their descriptions and afford material assistance to the reader in forming his conceptions of the subject.

In the section on inflammation of the brain, while it fully deserves the same general commendation as the one preceding, there are several features that admit of more especial mention.

In the first place our author limits the term encephalitis altogether to circumscribed inflammations in patches, holding and expressly stating that such a thing as diffuse acute encephalitis is never met with. Then he only notices as chronic inflammation of the brain, the chronic form of abscess following traumatisms or aural disease, which is characterized by a latent stage, varying from a few days to several months, or even years, generally characterized with various cerebral symptoms, such as aphasia, hemiplegia, etc., and finally ·in the great majority of

cases, ending fatally through the extension of the suppurative process and the involvement thereby of the meninges or other organs in an acute inflammation. By this restriction of the term encephalitis, Prof. Huguenin excludes some conditions that can perhaps properly be considered under this head. Thus we have such cases as have been described by Elam under the name of "cerebria," in which there is an appearance of acute general inflammation of the cerebral substance, and even complete breaking down of its structure without implication of the meninges. That these conditions do sometimes occur, *seems* to be established by the observations of the above named author, and this view is held by Hammond in the most recent edition of his work on nervous diseases.

Then again, Prof. Huguenin specially excludes multiple cerebral sclerosis from consideration under this head, holding that it is a distinct affection, different in its etiology, its course and its anatomical characters, from encephalitis. It has been shown by Charcot and others, that the morbid process in this affection is a diffuse and generalized one, and we have been always inclined to consider it as of a sub-acute inflammatory nature. And though there are not many higher authorities in neurological medicine than the author of the work before us, we must admit that with the data afforded us, we hardly see the reasonableness of the course he here adopts. When his completed work appears, this classification, with the grounds upon which it is based stated, may appear more justifiable.

Perhaps the best way to give Huguenin's views as to the true nature of cerebral inflammmations and the restrictions he puts upon the term, is to quote somewhat in extenso his own words. He lays down the following as his standpoints:

I. "Red softening and suppuration are true inflammatory disorders of the brain substance; a tissue in which inflammation follows a very peculiar course, *i. e.*, the effect on the brain, on account of its special constitution and physiological functions, is an exceptional one, dissimilar to that of inflammation of other tissues. Still, we cannot fail to recognize the anatomical process.

II. "Encephalomalacia in its narrower sense, *i. e.*, the softened patches in the brains of old persons, and those suffering from heart disease, or more correctly, those due to arterial thrombosis, or embolic plugging of the vessels, must be referred to the formation of hemorrhagic infarctions, due to stoppage of the vessels, with the genesis and conditions of which we have been made acquainted by a series of beautiful investigations (Cohnheim). The initial disorders have no connection with inflammation. Microscopically they are also non-inflammatory, but such as we now know to be the case in necrosis of the central nervous system. Except under altogether exceptional circumstances, a simple necrosis never leads to suppuration.

III. "But as soon as a true encephalitis has run its course,

i. e., as soon as those processes that we have to consider as truly inflammatory, have subsided, numerous degenerations and resorptions which serve as compensatory processes for the organism, occur; and thus in some cases there is formed a breach of substance that is finally to be distinguished from a thrombotic, embolic, or even apoplectic remnant, only with the greatest difficulty, and with other assistance than that afforded by merely the anatomical peculiarities; other pathological phenomena of the brain and of other organs as well must be considered here. This difficulty is not met with in every case, but to it is due the obscurity that has so long existed in this matter.

IV. "The circumscribed, punctate, hemorrhages (infarctions) that lead to yellow necrotic softening, and which, as said before, have in their beginning nothing whatever to do with inflammation, serve as an excitant to inflammatory action in the surrounding brain tissues. Thus there is frequently set up in their vicinity a zone of inflammation, which nevertheless does not reach in its degree of extension and acuteness, the importance of true encephalitis. This inflammation is only accidental and secondary, and the initial processes in the vessels cannot be held directly responsible for it; that encephalitis may occur with thrombosis and embolism is certain, and we cannot therefore reproach the older observers with having wholly mistaken the conditions.

V. "On the other hand, true encephalitis very frequently causes a partial necrosis of the neighboring tissues, that microscopically resembles simple yellow softening, and microscopically also resembles the processes of the latter, still we cannot suppose here any combination of inflammatory disturbances with the above-mentioned conditions of the vessels. Any local pressure also, that thoroughly interrupts the blood supply of a part, also produces necrosis; another fact that has long hindered the investigator from distinguishing the different conditions apart.

VI. "There are conditions in which we may speak of a combination of both alterations in the central nervous system. There are specific emboli with especial inflammation—exciting peculiarities. These produce mechanical and dynamic effects at the point where they occur. The immediate effect is the formation of a circumscribed punctate hemorrhage; but the specific irritation causes immediately a local vascular excitement, and an inflammatory exudation, and in most cases a rapid suppuration.

VII. "Finally, there may be true encephalitic disorder in the vicinity of non-inflammatory lesions, causing pressure and neoplasms of the brain. Here the combination of inflammation with yellow necrosis is common; in most cases it remains undetermined whether the yellow softening is to be considered as the final stage of the encephalitis, *i. e.*, whether the simple necrotic patch was previously in the condition of red softening,

or whether it was deprived of its nutrition directly by the
pressure of the growing necoplasm. However this may be, we
have here again an easily comprehensible combination of both
conditions.

VIII. "The investigation may also meet with softened
patches (using the term in its most general sense) in the brain,
examination of which affords no data as to the previous con-
ditions. The peculiar combination in which inflammation and
necrosis are very frequently found, allows a ready compre-
hension of the confusion that has so long lasted in regard to the
two conditions; in the fresh state, a satisfactory microscopic ex-
amination will at once remove the doubt. It must always be
kept in mind, that the brain tissue is one that suffers from the
slightest compression, and loses most quickly, with its func-
tions, its normal texture; and then the combination of encepha-
litis with many conditions of simple necrosis will excite no
astonishment."

The succeeding pages on the anatomical characters, the æti-
ology, symptoms, etc., leave hardly anything to be desired. As
usual, the remarks on the therapeutics of these conditions are
brief but eminently judicious and conservative. Of course not
much is to be expected in a large number of these cases from
any measures, and the details of treatment have largely to be
left to the educated judgment of the practitioner. Actual
operative interference in cases of cerebral abscess is advisable
only when an exact localization of the trouble can be made, and
with our present imperfect knowledge of cerebral localizations,
this can be done only occasionally.

The last fifty or sixty pages of the book are occupied by the
articles of Prof. Hitzig, on hypertrophy and atrophy of the
brain, under which latter head he includes the general paralysis
of the insane. As these conditions, with the exception of the
last, general paralysis, are rare; many of the cases indeed being
among the curiosities of medical literature, they call for and ad-
mit of no very extended mention here. Dr. Hitzig's articles are
therefore brief, and while the subjects, as might be expected,
are well treated, we find nothing particular to say in re-
gard to them. The reader will find this, as well as the other
parts of the book, a very satisfactory and thorough exposition
of the results of medical research, brought up to a quite recent
period. The forthcoming translation will be a valuable ad-
dition to medical literature in our language.

III.—CARPENTER: HUMAN PHYSIOLOGY.

Principles of Human Physiology. By William B. Carpenter,
M. D., F. R. S., etc.   Edited by Henry Power, M. B., Lon-
don, F. R. C. S., etc.   A new American, from the Eighth
Revised and Enlarged English Edition, with Notes and
Additions.   By Francis G. Smith, M. D.   Philadelphia,
1876.   Henry C. Lea.   1,083 pages.   Chicago: Jansen,
McClurg & Co.

During the past four or five years physiological research has
been so active, and the advances in this department so great,
that text-books on physiology of only a few years' date have
fallen completely behind in regard to many very important
questions, and new editions have become an absolute necessity.
Therefore, we have seen appear within the past two or three
years, a new edition of the well-known work of Dalton, to a
great extent rewritten and very much enlarged, and a new can-
didate for public favor in the elaborate work of Flint.   Now, in
obedience to this necessity, we have a new edition of Dr. Car-
penter's standard work, which has so long been an authority in
this country, and in Great Britain.

It is needless for us to speak of the merits of the original
work of Dr. Carpenter, they are already sufficiently well known
and appreciated by the profession.   The only points we are
called to notice here, are the changes and additions introduced
by the editors in this addition, and principally by the English
editor, Mr. Henry Power.   The additions by the American are
valuable, but they form only a very small portion of the whole.
We shall also pay particular attention to certain portions relat-
ing to the nervous system, its functions, etc., and the nervous
apparatuses of the organs of the body, rather than to other parts
of the book, since these come within the special province of this
Journal; and while we are glad to bear testimony to the general
excellence of the work, and to the fact that it, with the ad-
ditions made by the editors in this edition, is more nearly up to
the times than any other manual of equal size, we shall have to
notice, in detail, several points in regard to which it appears to
us defective.   It is absolutely impossible for any work on physi-
ology to be a perfect epitome of our knowledge at the time of
its publication, the science is so rapidly advancing that even a
few months, or weeks, may develop very important discoveries.
Admitting this, however, it is still advisable to call attention,
in the notice like the present, to even unavoidable deficiencies,

not as an ungracious act of criticism, but to mark the points in
regard to which the statements of the work should be taken
with allowance, or supplemented by reference elsewhere.
Physiology now-a-days can be studied only by the aid of the
periodical literature of the science, the value of the text-books,
and even of more extensive manuals, is that they give us a start-
ing point from which to carry on still more extended studies;
and as they give us a more or less complete statement of the ac-
tual state of the science at the date of their publications, their
value as such a basis for study is correspondingly affected.

The present volume, as we have stated already, is, to all ap-
pearance, with the additions made by the editors, the most com-
plete manual of its compass in our language. Still, it gives
evidence of some deficiencies in details, and some general defects,
in the parts relating to the nervous system. The one of these
latter that seems to us to most prominently call for notice here,
is not one of this edition solely, nor of this work, but is com-
mon to all, or nearly all the physiological writings of the day.
It is a general custom in the descriptions of the functions of the
nervous system to speak of nerve fibres as if they, of themselves,
possessed special motor, sensory, or other peculiar properties.
The use of the terms "motor fibres," "sensory fibres," "trophic
fibres," etc., is almost unavoidable, and has its conveniences, but
they should not be used in such a manner as to imply in any
way that the nerves themselves, apart from their terminal ap-
paratuses, have any special motor or sensory endowments. They
are, in all probability, mere conductors, conveying impressions
to and from special apparatuses at either terminus, the sending
apparatus being, in the case of the sensory nerves, at the peri-
phery, in the motor nerves at the centres, and the receiving ap-
paratus correspondingly at the other termination. There is
every *a priori* probability in favor of this view of the function
of the nerve fibres, abundant analogy, and some quite strong
physiological evidence in its favor. In the Periscope of the
present number of the JOURNAL, we have alluded to this point,
and quoted the well-known experiment of MM. Vulpian and
Phillipeaux, who united the hypoglossal and lingual nerves.
The result was as might have been anticipated with the view
here presented, a complete suppression of function since a peri-
pheral motor apparatus was connected by the operation with a
central sensory receiving mechanism which could not give out
motor impulses, any more than the muscles of the tongue could
normally send to it sensory impressions. Nevertheless, irri-
tation of the common conducting fibres produced movements
of the tongue, for which everything was prepared except the
excitation, which had to be thus artificially supplied. The ex-
planation, however, given by the experimenters, that the lingual
became possessed of motor fibres after the operation, that it did
not previously contain, shows the physiological error we are
here combatting.

That excitation of the nerve at points along its course should produce motor or sensory symptoms, is not surprising, or in contradiction to the view here held. By producing a molecular commotion in the nerve it arouses the functional activity of its terminal receiving apparatus, central or peripheral, as the nerve happens to be sensory or motor. It is a fact, however, that has been pointed out by physiologists, that this clumsy method of exciting the functional activity of this terminal apparatus, is not nearly as effective as the normal physiological one; that a motor nerve may cease to react to external irritants whilst it still is capable of conducting impressions from its centre in reflexes.

We have given so much attention to this point here, because it appears to us that too little altogether is made of it in the work, and the impression is produced that the nerves are even more than mere conductors, and that conduction itself is a peculiar property, according as the nerve is motor or sensory. Then, the remarks upon centripetal motor conduction might be spared, if this view of nerve function were kept in mind. But the error, either of comprehension or statement, pervades nearly · all the literature of the subject, and we wish to enter here our protest against its becoming more general.

The view of Brown-Sequard that there are special fibres for the conduction of the different sense impressions of pain, heat, tact, etc , is mentioned with apparent approval, as being supported by very strong evidence, even in the way and to the extent in which its author holds it; that these special sense conductors occupy different tracts of the spinal cord. It appears to us that this is one of the more doubtful hypotheses in nerve physiology, and not by any means the best method of explaining the facts that have suggested it. Another of Brown-Sequard's views, that is at least partially favored by the editor, is that regarding the optic decussation, that it is complete and not, as according to the usually received opinions, a partial one. On this latter point, however, the editor is not very outspoken in his opinion, and his bias is only inferred from his manner of stating the facts and arguments of both sides of the question.

The same may be said of the remarks upon trophic nerves, the existence of which, we infer, the editor is not disposed to admit: even the opinion that nutrition is under the control of the nervous system, does not appear to meet with much favor in his mind. While we are willing to have the existence of special trophic nerves disputed, it yet appears to us that there are many better proofs of the influence of the nervous system over nutritive processes than are given in this work, and, indeed, that there are few physiological theories that have stronger support, inferential and absolute, than that of nervous trophic action. The statements of this volume, therefore, seem to us to be somewhat misguiding to the student.

In most other respects, the section on the nervous system is, on the whole, a very satisfactory statement of the present con-

dition of our knowledge, quite as complete as could be expected in a work of this compass. That portion on the spinal cord and medulla is more full than that on the cerebrum and the cerebral ganglia, but perhaps the latter is sufficiently so. A pretty well detailed account of Meynert's views of cerebral physiology is given, and is the more intelligible since a schematic illustration accompanies the text. A brief account is also given of the experiments of Fritsch and Hitzig, Ferrier, Nothnagel, and others, upon the motor centres of the cortex. The section upon the special senses seems to be also quite complete, and closes with an exposition of Fechner's law of the measure of our sensations, a matter of some considerable interest, which we do not remember having seen explained in other recent English text-books on physiology, at least to any extent.

As regards the other sections of the work, we have no special criticisms to offer. The nervous apparatuses of the circulation, the respiration, and of the digestive organs, are quite fully described and very recent authorities and memoirs referred to. There are undoubtedly some omissions and defects of arrangement, but we have been agreeably surprised to find the volume so complete in regard to the structure and functions of the nervous system in all its relations, a subject that, in many respects, is one of the most difficult of all, in the whole range of physiology, upon which to produce a full and satisfactory treatise of the class to which the one before us belongs. This is especially the case at the present time, when, as we have already said, physiological research is so active in this special department.

The additions by the American editor, though not numerous, nor generally lengthy, frequently supplement those of Dr. Power very usefully, and give to the work as it is, a considerable value beyond that of the last English edition.

In conclusion, we can give our cordial recommendation to the work as it now appears. The editors have, with their additions to the only work on physiology in our language that in the fullest sense of the word is the production of a philosopher, as well as physiologist, brought it up, as fully as could be expected, if not desired, to the standard of our knowledge of its subject at the present day. It will deservedly maintain the place it has always had in the favor of the medical profession.

IV.—FLECHSIG: THE ROUTES OF CONDUCTION IN THE BRAIN AND CORD.

DIE LEITUNGSBAHNEN IM GEHIRN UND RUECKENMARK DES MEN-
SCHEN AUF GRUND ENTWICKELUNGSGESCHICHTLICHER UN-
TERSUCHUNGEN, dargestellt von Dr. P. Flechsig. (*The con-
ducting tracts in the brain and spinal cord of man on the
basis of embryological researches.*) Leipzig, 1876.

Flechsig's researches are mainly based on the following laws,
which, though not entirely new, had not been previously
utilized to any extent in the study of the nervous system:

I.   In the course of development of the central nervous sys-
tem, the different distinct nerve-tracts are not formed simul-
taneously, but successively; the individual fibres, however, con-
stituting any one nerve-tract, are formed at one and the same
time.

II.   The formation of the medullary sheaths takes place in
the different tracts in the chronological order of the primary
development of these tracts.

III.   The mode of development of the central nervous sys-
tem thus divides it into separate and distinct systems of fibres,
(tracts).   The most convenient method of studying the topog-
raphy of the separate nerve-tracts during their development,
is afforded by the formation of the medullary sheaths.   The
fasciculi, the nerves of which have become invested with the
white substance of Schwann, are easily distinguished by their
dull white appearance, from the other *gray* fibres, which con-
sist, as yet, merely of an axis-cylinder.   This characteristic ap-
pearance of developed medullary sheaths (for which F. adopts
the term "Markweiss" (medullary white) is found for the first
time about the middle of intra-uterine life; the process is com-
pleted about the end of the fifth month after birth.   Although
dissection of the fresh material shows the topography of the
white nerve-bundles very distinctly, microscopical sections are
of course of still more value, especially after staining with sub-
stances, like osmic acid, chloride of palladium and chloride of
gold, which render all medullated fibres still more easily dis-
tinguishable by their color.   The author also mentions a new
method of staining with gold, which gives excellent results in
tracing the course of nerve-fibres.

On the strength of embryological observations, the author
divides the white substance of the spinal cord into the follow-
ing territories: I., *the anterior pyramidal tract* (vordere Pyra-

midenbahn), a narrow zone lining the anterior fissure; II., *the main anterior fasciculus,* (Vorderstrang-Grundbundel) identical with the anterior column, with the exception of the previously mentioned zone; III., *Goll's columns,* the narrow triangular region on each side of the posterior fissure, also known previously as the funiculus gracilis, but merely in the cervical part of the cord; IV., *Burdach's columns,* (B.'s Keilstraenge) occupying the remainder of the posterior columns, and continuous with the fasciculus cuneatus in the medulla; V., *the direct lateral cerebellar tract* (direkte Kleinhirn-seitenstrangbahn), a narrow zone at the periphery of the lateral columns, extending from near the exit of the posterior roots to about the centre of the periphery of the lateral columns; VI, *the lateral crossed pyramidal tract* (gekreuzte Pyramiden-seitenstrangbahn), extending inwardly from V., to the posterior horn of the grey substance; VII., *the remainder of the lateral column* (Seitenstrangreste),which he subdivides into (a) *the lateral borderzone of the grey substance* (seitliche Grenzschichte der grauen Substanz), a narrow zone surrounding the lateral boundary of the grey substance, and (b) *the anterior mixed region of the lateral column* (vordere gemischte Seitenstrangzone), the remaining (anterior external) part of the lateral column.

In the fully developed cord these separate tracts are not visibly isolated; but, as Flechsig maintains, they may be recognized by the size of their nerve-fibres. By dividing nerve-fibres into four classes, according to their thickness, he finds that each region differs slightly from the neighboring territory in the size of the fibres constituting the bundle. But more interest centers in the fact that the course of the separate system of fibres, traced by means of the embryological method, is identical with the course assumed by the so-called *secondary degenerations* of Tuerck. Flechsig's results on the course of the tracts may be summed up as follows:

The Pyramidal Tracts. The fibres of each pyramid of the medulla oblongata ordinarily follow two different roads in the spinal cord: viz., the anterior pyramidal tract of the same side, and the lateral (crossed) pyramidal tract of the opposite side. Considerable variations are found, however, in the numerical distribution of the pyramidal fibres in these tracts of the cord, not only in different individuals, but even in the two lateral halves of one cord. While *semi-decussation of both pyramids* has been most frequently found, Flechsig has also seen partial decussation of only one pyramid, with total crossing of the fibres of the other to the opposite side, and even *total decussation of both pyramids* with entire absence of both anterior pyramidal tracts. The other extreme—total absence of decussation, he has not yet met with, although occasionally but very few fibres did cross to the opposite side. Since the pyramids are now recognized as the tracts of voluntary impulses, this variability in the connection of one side of the brain with the same, or

opposite side of the cord, is of great clinical interest. The total area of the pyramidal tracts diminishes as they descend in the cord. The lateral pyramidal tract disappears usually about the level of the third or fourth sacral nerve, its fibres having gradually entered the gray substance. The anterior pyramidal tract disappears at the upper limit of the dorsal part of the cord, sooner or later, according to its original volume; the termination of its fibres is not settled.

A system of fibres running from the nucleus lenticularis, and internal capsule, through the pes peduncle and pons into the pyramid of the same side, and thence down into the spinal cord, is the tract of *Tuerck's descending degeneration;* any lesion of this tract is followed by degeneration of the fibres below the seat of the lesion. As far as Flechsig's researches go, this system of fibres is identical with his pyramidal tracts.* The variability in their topographical distribution in the cord, sheds a new light on the previously found differences in the locality of the degenerated fibres in the cord.

The direct lateral cerebellar tracts. Fibres radiating from the gray substance, especially from the columns of Clarke, collect in a flattened bundle, occupying a narrow zone at the periphery of the posterior half of the lateral column, which bundle is to be found as low as the lumbar part of the cord. Other fibres, however, from the same source, ascend the cord, intermingled with the remaining fibres of the lateral column, without forming a distinct bundle: they are shown to be equivalent to the first-mentioned fibres by the time of their development. Increasing in number as they ascend, both sets of fibres enter the corpus restiforme in the medulla, and as the *stratum zonale* gain its posterior part. Flechsig has traced them no further than the cerebellum, where they apparently run towards the vermis superior. In the *ascending form* of secondary degeneration this system of fibres is one of the tracts involved, any lesion in its course being followed by degeneration of the fibres above the lesion.

The remainder of the lateral columns consists of fibres radiating into it from the gray substance. The lateral border zone of the gray substance receives, besides, fibres coming from the posterior roots, while fibres from the anterior roots enter the anterior mixed region of the lateral column. On this account the area of the lateral columns enlarges considerable at the places where the roots of the spinal nerves contain most fibres; viz., at the cervical and lumbar enlargements of the cord. The fibres of these tracts enter the formatio reticularis of the medulla oblongata.

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* In a recent note in the *Centralbl. f. d. Med. Wissensch.*, 1877, No. 3, Flechsig states, that he has since traced the pyramidal fibres as a compact bundle through the pes pedunculi and internal capsule into the centrum semiovale, in the vicinity of the sulcus centralis.

The main anterior fasciculus receives fibres both from the anterior roots of the opposite half of the cord (through the anterior commissure), and from the anterior horns of the gray substance of both sides. It is continuous with the fasciculus longitudinalis posterior of the medulla.

Goll's columns are distinctly isolated in embryological specimens only as far as the upper third of the dorsal region, but very probably continue to the lumbar enlargement. Their constituent fibres are furnished partly by the columns of Clarke of the same side of the cord, partly by the posterior horn of the opposite side by route of the posterior commissure. Increasing in area as they ascend, they apparently terminate in the "nuclei of the funiculi graciles" in the medulla. This system of fibres (together with the lateral cerebellar tract) is the seat of the *ascending form* of secondary degeneration.

Burdach's columns are composed largely of fibres from the posterior roots, and hence show the greatest area at the enlargements of the cord, where the spinal roots are the most voluminous. But fibres composing these columns can also be traced into the gray substance, radiating towards the anterior horn, the posterior commissure, and the region of the columns of Clarke. The tract terminates apparently the "nuclei of the funiculi cuneati" in the medulla.

As regards the order of development of the different systems of fibres, we cannot follow the numerous details of Flechsig's researches; suffice it to say that the formation of medullary sheaths begins in the cord and gradually extends towards the cerebrum. The first appearance of "medullary white" is observed in the fœtus of 25 centim. length in Burdach's columns. The pyramidal tracts, on the other hand, the last in the cord, are not completed until after birth. About the end of the fourth month after birth all fibres of the brain have acquired their medullary sheaths.

This brief review of the most important facts shows the work to be one of real merit. Notwithstanding its size (over 300 pages), most of the contents are either wholly new or an *exact* reproduction of our hitherto uncertain knowledge on the subjects. But a more systematic arrangement would have enabled the author to avoid numerous repetitions, while making the the book a less difficult, if not more profitable study. H. G.

SHORTER NOTICES.

I. The Practitioner's Handbook of Treatment; or the Principles of Therapeutics. By J. Milner Fothergill, M. D.; member of the Royal College of Physicians of London; Assistant Physician of the City of London Hospital for Diseases of the Chest, etc. Philadelphia: Henry C. Lea, 1877. Chicago: Jansen, McClurg & Co.

II. A Practical Treatise on Diseases of the Skin. By Louis A. Duhring, M. D., Professor of Diseases of the Skin, in the Hospital of the University of Pennsylvania, etc. Philadelphia: J. B. Lippincott & Co., 1877. 618 pages. Chicago: Jansen, McClurg & Co.

III. Recherches Cliniques et Therapeutiques sur l'Epilepsie et l'Hysterie. Compte-rendu Observations Recueillies a la Salpetriere de 1872 a 1875. Par Bourneville, Ancien Interne des Hopitaux de Paris; Redacteur en chef du *Progres Medical.* 500 pages. Paris, 1876.

IV. Civil Malpractice : A Treatise on Surgical Jurisprudence. With chapters on Skill in Diagnosis and Treatment, Prognosis in Fractures, and Negligence. By M. A. McClelland. M. D. New York: Hurd & Houghton, 1877. 554 pages. Chicago: Hadley Bros.

V. The Treatment of Syphilis. By E. L. Keyes, A. M. M. D. New York: D. Appleton & Co., 1877. 83 pages. Chicago: Jansen, McClurg & Co.

VI. Memoires sur la Galvano-Caustique Thermique. Par le Docteur A. Amussat *fils.* Avec 44 figures, intercalees dans le Texte. Dessinees par Faguet, etc. Paris, 1876. Germer Bailliere. 125 pages.

VII. Transactions of the Twenty-sixth Anniversary Meeting of the Illinois State Medical Society. Held in the City of Urbana, May 16, 17 and 18, 1876. N. S. Davis, Permanent Secretary. Chicago, 1876.

I.   The author's plan in this volume was, as he states, not to produce an imperfect work on the practice of medicine, but to endeavor to give some idea of the *rationale* of our therapeutic measures. There was certainly abundant opportunity for the work; we know of no other with the same object and plan of

execution. Dr. Fothergill says that it is the result of the labor of nine years, during all of which time he has had its production as an object in view. Looked at in this light, it is, perhaps, in some respects disappointing, but he probably only means to say the idea of the work, so to speak, has been in his mind for that time, and that it embodies the results of the experience of perhaps even a much longer period.

There is probably nothing that is more difficult for the young medical man, just entering upon practice and meeting with what are certainly to him novel phenomena of disease, than to know just how to apply general principles to therapeutics, to cut loose from the notion of specifics that is as false as it is popular, and to at once apply his educated common sense to the task before him. A very large class of so-called doctors actually never acquire this ability, at least to any considerable extent. The consequences to their patients, whatever they may be to their practice, are not difficult to surmise; they cannot fail to be in the end disastrous. An examination of this book makes us feel sure that its perusal will serve, far better than that of ordinary works on practice alone, to enable the student and the imperfect practitioner to meet disease successfully and courageously, and to know how to do as nearly as possible the right thing in many trying emergencies. We do not say that the work supercedes works on practice; it is very far from being complete and comprehensive enough for that, but that it, as far as it goes, introduces to the mysteries of the healing art in a more practical, as well as in a more scientific manner. It is a work that we can cordially recommend to the attention of both students and practitioners.

II. This is the second American Manual of Skin Diseases to appear, and it is safe to predict that it will receive a cordial welcome from the profession. The author is well known as one of the leading dermatologists of the country, and a follower of the German school, and his work shows this to a certain extent, as well in its dedication to Prof. Hebra, as in its contents. It is, however, probably none the worse for this fact, in the minds of the majority of the American public for whom it is intended. While it is less elementary in its character than the work of Piffard, noticed last year in this journal, it is likely to be equally valuable to the student of medicine who commences the study of these affections, and to the working physician in practice. As a safe and comprehensive treatise on this special class of diseases, we consider it one of the very best in our language.

III. This work, one of the publications of the *Progrès Medical*, comprises three parts independent of each other. The first of these is a clinical study of the state of *mal epileptique;* a very fully reported fatal case is given with the results of the autopsy, etc. The second and longest part of the work is entitled

"Therapeutic Researches," and contains the results of clinical and experimental studies and discussions of the literature upon the therapeutic usage and value of several of the agents that have been recommended and employed in the treatment of epilepsy. We can give here only the general conclusions of these investigations, but these are of interest.

First, the results, as far as they go, of the use of the ammonia and sulphate of copper in this disorder are hardly encouraging, in only one of five patients was there any amelioration, in three the effects were not perceptible, and in one the disorder was decidedly aggravated.

The bromide of camphor was tried upon nine patients at the Salpetriere. Three of these were old and rather hopeless cases, in which but little was expected from the treatment. In the other six cases, which were less unfavorable as a whole, the amelioration was quite decided, the number of regular epileptic attacks, and especially of the attacks of vertigo were notably diminished. The fact that the vertigos were especially favorably affected is mentioned as promising well for the use of this drug in cases in which these are the principal symptom. From a very minute quantity the doses, gradually increased, ranged as high as twenty-seven grains in one case and to twenty-two or three grains in the majority of the others. It appears from this showing that this agent may render very useful services in this affection.

The other remedies experimented with were ice, locally applied, oxide of zinc, the results of the use of which were hardly so decidedly beneficial, and nitrite of amyl, which is the subject of a rather exhaustive study.

We extract the following summary of the more important points observed in the patients submitted in the action of this remedy:

" 1. The movements of the jaws, the trembling of the lips, and the chewing motions, observed by us in many cases, fully confirm the statements of Dr. Crichton Brown (in regard to the occurrence of these secondary phenomena).

" 2. The lowering of the temperature that has been noted in every case in which it was looked for, shows that in this respect the action of nitrite of amyl is the same in animals as in the human being.

" 3. From the beginning of the inhalation we may, without inconvenience, relieve the patient from all the restraints applied. To prevent the return of the severe clonic convulsions (of hystero-epilepsy), it is sufficient to recommence the inhalation. In Gen. . , Her. . , Mar. . , for example, movements of deglutition, nauseas, or vomitings, indicate the end of the attack. Thus it is possible to announce almost at once that Gen. . , will be again seized, even when she appears completely relieved, if the inhalation has not induced vomiting, or efforts to vomit.

" 4. Of consecutive phenomena we may cite, (*a*) a more intense and enduring cephalalgia than the patients suffer after the natural termination of their attacks; (*b*) a sensation of vertigo, complicated with a slight degree of hebetude: (*c*) troubles of vision. Some patients see the faces of those who surround them half yellow and black, others think they see flakes of yellow snow falling, million of sparks; or yet they see hideous black animals, circles of various colors, etc. In respect to these, although the statements of patients appear to agree, it is well to reserve an opinion, since hysterical patients communicate their symptoms very generously.

" 5. Generally, when the inhalation has been properly conducted, the patients, once come to their senses, do not have another attack the same day.

" Some of our observations indicate with what facility a tolerance of the remedy is produced, since it was necessary to increase the dose, so to speak, at each inhalation. To avoid any accident it would be prudent, it appears to us, to abstain from the use of the nitrite for a little while, in those patients who have taken it repeatedly within a short period.

" 7. Nitrite of amyl exercises an incontestable influence on the attacks of epilepsy and hystero-epilepsy. But does it have any permanent influence upon the progress of the convulsive accidents? We await new facts to answer this question. One patient, Dan. . , after the inhalations, has remained eight weeks without having any attacks; another, Her. . , has had none, though four months have passed since the use of the drug. Is this an amelioration due to the remedy, or merely a coincidence? At present it is impossible to say."

The latter part of the volume is devoted to the subject of hystero-epilepsy, a study of two or three of the more striking cases at the Salpetriere, one or two of which have already become classical, so to speak, as illustrations of the disease. The work, as a whole, is a very valuable contribution to clinical literature.

IV. This is not a law-book, but rather a medical work. It is designed for medical men, and to such will doubtless prove very useful. Dr. McClelland has collected together a large number of cases of suits for malpractice from the various State reports, together with some from foreign sources, which illustrate the course of the courts in dealing with these questions that so especially interest the members of the medical profession. While the majority of the decisions appear to combine law and justice, there are a few here reported that are not encouraging, and certainly seem to fail in respect to the latter. This applies, of course, more to the verdicts of juries, than to supreme court decisions, though the latter are far from being in all cases above medical criticism.

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The arrangement adopted in the book is convenient rather than scientific: the cases are classified according to the anatomical position of the lesion or injury, and not either according to the legal points involved or their more important surgical and pathological relations. The plan adopted is, nevertheless, very possibly the best for a work of this kind. We notice a few slips on the part of the compiler, thus the majority of the cases cited under the head of "Measure of Damages," for example, seem to us to have no particular reason for their position there.

The latter part of the work, upon skill in diagnosis and treatment, and the prognosis of fractures, dislocations, etc., contains matter that will be of use to the counsel in suits for malpractice, as giving a pretty good summary of the best surgical opinions on these subjects. The tables given are largely those of Dr. Frank H. Hamilton, but Dr. McClellan has supplemented them with still further data collected by himself. The closing chapters upon "negligence" will be useful reading for physicians and surgeons.

Altogether, we think this book will be a very valuable addition to the library of the surgeon and physician, and a useful work of reference, in many respects, to the legal practitioner.

V. This is a well written and able, though brief, treatise on the treatment of syphilis. The most striking feature is the author's use of mercury in what he calls the tonic treatment, consisting in finding experimentally and gradually the quantity required to begin to produce constitutional effects, which he calls the "full dose," and then reducing this to one-half for the continued treatment, after the active symptoms of the disease have subsided. This reduced quantity he calls the "tonic dose," and advises its continuous use for two or three years. This method of Dr. Keyes was made known to physicians by his papers published the past year, and therefore there has not been time for trials to have been made of it, thoroughly, by others but it seems from his own testimony to be altogether rational and very successful. It is based upon the results of the author's investigations in regard to syphilitic blood, and has, therefore, the recommendation that it is, so far, not an empirical method. We wish that all the other employments of mercury could be as well founded on a scientific basis,—that we could give at least a probable theory of its undoubtedly useful action.

The other agents and methods useful in the treatment of syphilis are briefly, but very satisfactorily, described, as is also the management of the various special symptoms. We can cordially recommend the book as a very excellent one of its class.

VI. This is a very elegantly gotten up work on one of the surgical applications of electricity. We cannot go at length into its merits, but it appears well written, and the methods it recommends seem useful. In typography and illustrations it is admirable.

VII. The transactions of the Illinois Society form a neat volume of two hundred snd seventy-one pages, containing the usual addresses, reports and papers and brief abstracts of the discussions of the same. The noteworthy ones of these, in a neurological point of view, are the report on otology by Dr. Hotz, and that on physiology by Dr. Stevenson. The latter of these reviews a number of the recent investigations in the physiology of the nervous system, and the former is devoted to the subject of brain disease caused by extension of aural trouble.

Other papers worthy of mention here are those on contributory negligence, by Dr. M. A. McClelland, and on menstruation by Dr A. Reeves Jackson. The general appearance of the volume is very fine.

Editorial Department.

RELATIONS OF WEATHER TO PAIN.

IN the April No. of the *American Journal of the Medical Sciences*, is an article by Dr. S. Weir Mitchell, on " The Relations of Pain to Weather, being a study of the Natural History of a case of Traumatic Neuralgia," illustrated by maps and charts, on which the relations in question are shown by curved lines. The study of the case shows that the occurrence of the pain has probably closer relations *to barometrical pressure*, than any other climatic factor, though it has relations apparently not so intimate to temperature, and possibly, electrical changes.

It is not claimed that these climatic variations produce neuralgia in healthy persons, but on the contrary in those who are suffering from some kind of nerve lesion. " Any lowering cause, such as dyspepsia, over work, and anæmia, however brought about, is apt to increase this sensitiveness to barometric changes, and so every enfeebling agency, as it were, tunes a man's nerves up to the capacity of producing pain, when once there exists a permanent cause in the way of neural disease." (P. 327.)

But Dr. Mitchell does not attempt an interpretation of the important relations, between climatic phenomena and neuralgia, which he has been at such pains to trace. We have for a long time past given attention to this subject, and in the present number, we have communicated some of the results of our observations in an article on the " Pathology and Treatment of Neuralgia." In that paper, after describing the material lesion which we believe to be the fundamental one in all true neuralgias, and on the basis of that lesion; an attempt is made to explain how climatic changes, such as variations in

barometrical pressure, temperature, etc., may excite the attacks
of pain in neuralgia.

We quote as follows:

" Then, again, vascular tension within the cranio-spinal cav-
ity, may be produced by means of certain climatic changes,
more particularly the following: In the first place, by changes
in temperature.   Either extremes of heat or cold, if they are
suddenly brought to pass, may change blood tension in the
cranio-spinal cavity.   If the surface is suddenly exposed to
cold, the skin and its small vessels contract, and as a conse-
quence, less blood circulates in the surface, than is necessary
to preserve an equilibrium, as between superficial and deep-
lying parts, and hence more blood circulates in the visceral
cavities, or tends to do so, than under different circum-
stances.   Or, on the contrary, if the surface is more or less
suddenly exposed to a high temperature, the cutaneous vessels
dilate and admit more blood to the exterior of the body, and
hence less relatively goes to the interior, thus giving rise fre-
quently, though not necessarily, to diminished vascular ten-
sion, in the cranio-spinal cavity.   Under such circumstances,
it is very common for a person to complain of temporary loss
of nerve power, or languor, and other symptoms which can
hardly be ascribed to mere changes in nutrition, but in part,
at least, to changes in pressure.   Under such circumstances,
neuralgias, *if the neuralgic condition exists,* are very likely
to set in on slight provocation.

" In the second place, changes in barometric pressure, give
rise, under the peculiar circumstances of the circulation in the
cranio-spinal cavity, to changes in vascular pressure.   This
may be seen in extreme forms in the effects of high altitudes
on the nervous system, and in the effects of increased atmos-
pheric pressure, a good example of which is seen in the case of
workmen operating in the condensed air of the caissons beneath
the piers of certain bridges.   The atmospheric pressure on the
surface varies greatly from time to time, as is well known, and
certainly leads to changes in vascular pressure within the air-
tight, unyielding cavity, in which brain and cord lie.   This, as
I believe, from pretty close observation, is *one* of the prime
exciting causes of the nervous symptoms,—and "neuralgia is

chief among them,—which seem to depend on the " weather." This view will also aid in explaining why steady, mild climates, are, as a rule, the most favorable to the removal of those nervous symptoms, which appear to be influenced by climatic changes. Within the past year, I have seen some unpublished charts, drawn up by Dr. S. Weir Mitchell, of Philadelphia, in which he had made the endeavor to trace, by means of lines which resembled isothermal and magnetic curves, the relations of neuralgia to certain climatic changes. I do not know what conclusions he may have reached, but it is a subject to which I have given considerable attention, and I am convinced that much of the influence exerted by climatic changes on certain nervous affections depends on variations in vascular pressure, produced by temperature and barometric changes operating in the way I have just described. I am far from thinking that these or similar exciting causes, can produce neuralgias in a healthy nervous system, but that they may do so, in cases where the ' neuralgic condition ' already exists, as it so often does."　　＊　　＊　　＊　　＊　　＊

It is said in another part of the same paper in regard to the nature of a neuralgia, and the final mode of operation of its causes that " the essential morbid condition in a neuralgia, is a nutritive lesion of the central sensory apparatus of cells, which are the seats of true nervous sensibility. This state is frequently caused by disease of the peripheral nerves, but even in such cases the more irritable state of the sensory tract is the main factor reached by judicious analysis of the phenomena of neuralgia. In this condition it reacts with pain to even trivial impressions made on the sensory nerves, which terminate in the affected region.

" The attack of pain may be due to over-excitation, and hence over-wear and waste of the affected centre, produced by the channel of its sensory nerves, or by changes in blood-pressure in the affected centre, caused by loss or increase of tonus of the peripheral vessels, or a change in cardiac action, or by changes of posture, or of temperature, or of barometrical pressure, or by influences acting on the vaso-motor nerves, distributed to the diseased centre, and which may be affected from either the peripheral nervous system, or from the cortex cerebri."

In this way it seems to us may we explain on the basis of established principles, and in apparently complete accordance with the facts, the *modus operandi* of climatic changes in the production or aggravation, and the disappearance also of nervous symptoms, chief among them being neuralgia and nervous languor.

THE ACTION OF COFFEE ON THE NERVOUS SYSTEM.

For some time past we have given attention to the physiological and therapeutic action of coffee. In this note it is our purpose to speak of some of the results of our observations, with a promise to give at some length hereafter, the facts, or what appear to us to be such, which we have accumulated.

Neither the facts nor the practical deductions we have been led to make, can be considered as novel.

We have been led to regard coffee as perhaps the purest *nerve stimulant* in the list of the materia medica. It appears to us to be worthily the type of its class. In its action on the nervous system, it differs from that of nerve tonics proper, in the following way: A nerve tonic is,— let us say,—a medicinal agent, which acts by stimulating the nutritive processes, as regards the nervous system. By such means we may hasten the intimate processes of nerve growth and nerve repair, and hence indirectly hasten the acquisition of nerve power. A nerve tonic does not, as a rule, contribute anything directly to the growth or repair of nerve tissue. It simply stimulates or quickens those processes on which the material and functional integrity of the nervous system depends. But a nerve stimulant does not act in this manner. Instead of contributing to the acquisition of nerve power, it hastens its expenditure. It excites the nerve activities proper, and not the nutritive processes upon which the acquisition of power depends. The action of the tonic is naturally followed by an increase of strength, of nerve force, while the action of a stimulant is followed by a period of exhaustion, varying in degree and duration. The one leads gradually to its results, the other much more rapidly. They act hence very differently and lead

ordinarily to diametrically opposite results; viz., the one to an increase, the other to a waste or diminution of nervous energy.

If this distinction between the modes of action of tonics and stimulants on the nervous system, (though in a certain sense tonics are stimulants) is well founded, it is of practical importance to be remembered. In cases where there is abnormal nervous excitability, conjoined as it so often is with neurasthenia, and in which the indications are urgent, on the one hand for increased rapidity and effectiveness as regards the process of nerve nutrition, and on the other for tranquility,—freedom from excitement, diminution of nerve activity, and hence of waste of nerve structure and of power; the spontaneous dictate of common sense would be to give nerve tonics, and except in some emergency to avoid nerve stimulants. And after a prolonged trial, in accordance with a set purpose, such is the deliberate conclusion we have reached; viz.: *Except as a mere temporary remedial measure, to avoid nerve stimulants as a rule, in the class of cases above described* in favor of nerve tonics, and on the grounds laid bare in the distinction drawn out above. Coffee as one of the purest of the nerve stimulants, falls into condemnation under this rule.

We have no hesitation in saying, not as something novel but believed to be true,—that in case of " nervous " or " neuralgic constitutions," in cases of neuralgia, insomnia, in many forms, and in almost all forms of chronic, or at least habitual neurasthenia, there is no one thing productive of more harm in a general and moderate way, than the use of strong coffee as a beverage. It temporarily excites the nervous system in such cases, only to lead to its speedy exhaustion, and to the multiplied though usually temporary ill-consequences of the same, such as depression of spirits, languor, loss of appetite, neuralgic symptoms, etc. It is true that the person who is well, especially if phlegmatic in temperament, may use it with comparative impunity. It is true that many persons are temporarily cheered by it to be let down again into the slough of despond, when its action has terminated, and it is true that it often allays a headache or some neuralgic pain, especially if

about the head, as guarana will do. But aside from its use in such cases as we have named, as a remedial agent, in which it acts admirably, its habitual and free use as a beverage, in case of persons of a nervous constitution, as a rule, is only injurious. But we cannot in this notice do more than call attention to the subject. We shall treat it at greater length at another time, and among other things may speak of its supposed effects in diminishing waste of the nitrogenous tissues.

AMERICAN NEUROLOGICAL ASSOCIATION.

The third annual meeting of the American Neurological Association meets the 6th of June this year, in the city of New York.

From the circular which has been issued to the members, we extract the following:

" We earnestly appeal to you to contribute to the interest of the meeting by means of formal essays, by clinical notes of interesting cases, by the exhibition of gross or microscopical specimens, casts, photographs, new instruments, etc., or by physiological demonstrations.

" The committee of arrangements will spare no pains to provide means of demonstrating specimens, and of performing experiments, and they will also endeavor to make the reunion a pleasant one."

We would heartily commend the objects mentioned, to members of the association, and hope to see a full representation at the approaching meeting at New York.

After May the first, the office of publication of the JOURNAL will be No. 70 E. Monroe street, Chicago, to which place all communications should be addressed.

The title page for the last volume will be found bound in the present issue. It should have appeared in the October number of last year, but was inadvertently omitted.

A few typographical errors escaped notice while the earlier signatures of this issue were passing through the press, we here take the first opportunity to correct one or two of them. On page 282, for the proper names Ecker and Ramon, read Ekker and Ramaer, and on page 284, read Ekker instead of Ecker.

Also in our last number, (January), in the prescription given on page 193, the sign for ounces was used where that for drachms was the proper one. There are some other errors less important, but we will not take the space to notice them here.

The final part of the review of Dr. Ferrier's book on the " Functions of the Brain," has been crowded out of this number. It will appear in our July issue.

𝕻eriscope.

a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

THE CENTRIPETAL AND CENTRIFUGAL DIRECTIONS OF SENSIBILITY IN THE NERVES. At the meeting of the Soc. de Biologie, Dec. 16 (rep. in *Gaz. des Hopitaux*) M. Bert announced that he had for a dozen years been seeking to answer experimentally the question whether shock communicated to the nerves could pass both ways, centripetally or centrifugally. In other words, whether a pinch on the arm, for example, communicates to the nerve at once a centripetal and centrifugal shock. The first was long ago answered, the second still remains to be determined; but to do this it is needful to place, in some way, a brain at the finger-tips. The following experiment for this purpose was performed by M. Bert. Having skinned the end of a rat's tail, he bent it backward on the back of the animal and inserted its tip into a little wound there, so to make the tail, as it were, take root there again. After a sufficient time had elapsed for the circulation to be well established between the exrtemity of the tail and the back in which it was fixed, M. Bert divided the tail into two equal parts, one in its normal position, the other grafted into the centre of the back. Then testing the sensibility of this last portion, he at first obtained no result, but a few days later when its end was pinched the animal turned and attempted to bite, not the end, but the point where it was fixed in the back. From this experiment he inferred that sensibility travels normally over the nerve fibre, either centripetally or centrifugally.

Nevertheless, the objection had occurred to him that the nerves so divided might have become regenerated, as is observed to be the case after surgical operations. He therefore renewed the experiment, taking care to wait eight months before dividing the tail of the animal. Allowing then that the sensibility in the grafted end of the tail was due to regeneration of the nerves from the wound in the back, we ought, in testing it, to find it immediate. But this was not the case, after the section the grafted portion of the tail was insensible up to one centimetre of the back, and it is only after the second day that it becomes sensible over its whole extent. On the second day the sensibility is even exaggerated, and the animal was very sensitive to pinches of the end of the new tail. M. Bert therefore believed his former conclusions to be correct.

M. Onimus observed that the sensibility noted by M. Bert in this exper-

iment, might well be due to some nerve filament near the cicatrix, and was then simply a centripetal sensibility. What led him to this opinion was the exaggeration of this sensibility two days after the section. It is, in fact, the same phenomenon we observe in parts even destitute of nerves, such as cartilages and tendons, in the midst of inflamed tissues.

M. Henocque remarked, relative to the comparison made by M. Onimus, that, up to the present, there is no fact proving the sensibility of tendons. The examples that have been given, relate all to sections around the tendons, and the sensibility observed in such cases is due to nerves in the immediate vicinity of the ligaments, but it is well demonstrated to-day that no nerves exist in the tendons.

M. Pouchet, returning to the communication of M. Bert, said that it did not seem possible to him that the same nerves can conduct sensibility in two directions, both centripetally and centrifugally. It is not on the nerves, but on individual nerve tubes that experiments must be performed to clear up such a case as this.

In the discussion which followed, M. Bert stated that he intended to make a careful microscopic examination of the nerves in the grafted portion of the tail.

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At the meeting of the Society de Biologie, Jan. 6, (rep. in *Le Progres Médical*) M. Bert again brought up the subject. He had completed this physiological study by an experiment that was absolutely conclusive. He divided, some centimetres from its base, the tail of a rat bent backward and grafted into its back. He then determined directly after the section, and the two days that followed, that the trunk of the tail adherent to the back was perfectly sensible, but this sensibility then disappeared. Further, histological examinations by M. Ranvier showed that the nerves of the trunk were completely degenerated when the sensibility disappeared. This experiment showed, therefore, that the nervous influence was propagated toward the peripheral end of the nerves of the tail, which were found to be in connection, at the point of union with the back, with new sensory centres, but had lost their trophic centre.

At the meeting Dec. 24, M. Laborde recalled the subject, disputing the conclusions of M. Bert. He said "M. Bert sought to find if there was a double current in a sensory nerve, if a shock communicated to one of these nerves was transmitted at once in both directions, to the centre and to the periphery." He at first was astonished at this question, since we all know that the sole function of the sensory nerves is to transmit sensations to the centre of perception.

In thus grafting the tail of a rat to its back, what had M. Bert accomplished? He had to admit that, basing himself on his own researches, the sensory nerve, or nerve tube, united itself with another similar one passing to the cord. But was there even such a nerve so disposed, going from the cord to the cord. Such a nerve does not exist said M. Laborde. M. Bert's experiment therefore proves nothing.

Admitting this, M. Laborde seeks to find one more satisfactory. He suggests the section in a frog of one sciatic and uniting its superior portion to a sensory nerve of the other leg. Thus we would have a true periph-

eral nerve returning to the cord. But this would be a very difficult experiment to carry out successfully.

M. Laborde thought it difficult to understand how a shock communicated to a sensory nerve is arrested at once at the point where received, without centrifugal transmission. Clinically he thought, that we have some proofs to the contrary. When, for example, we strike our elbow, we feel not only at the point struck but all along the course of the ulnar nerve, a painful sensation, even at the finger-tips.

M. Gubler did not agree with M. Laborde that the two currents do not exist in the sensory nerve, he thought on the contrary that it was even needful to admit this. As regarded M. Laborde's illustration of the blow on the elbow, from which he deduced the centrifugal as well as the centripetal conduction of the nerve to indicate rather a kind of reflex action, analogous to that observed in amputated limbs.

M. Gubler then mentioned a fact not well known, but which he had constantly had occasion to observe. If we irritate any point whatever of the body, we feel more or less pain at the point irritated, but if the sensation is very intense there is instantaneously produced another pain at a distant point but always at the same. If, for example, the pain is felt in the thigh the other will be felt at the waist; if the primary pain is in the side, the secondary or sympathetic one occurs in the shoulder or arm. In a number of observations by M. Gubler on the same individual, the sympathetic pain was uniformly at the same point. Now it is impossible, he said, that this secondary pain can be the result of a shock communicated to the nerve conducting the primary pain, and he proposed the following hypothetical explanation. There is, he thought it necessary to admit, something analogous to a reflex action here, that is, a sensation conducted to a perceptive centre, an impression received at that point, and this centre reflecting this impression to another point than the one from which it was received. But this secondary current can, like the primary one, pass only over a sensory nerve. These nerves therefore conduct sensibility both centripetally and centrifugally.

M. Laborde considered the illusions of people who had suffered amputations as being nothing in the line of reflex action, and proving nothing in favor of centrifugal sensory currents.

M. Gubler, in reply to a question of M. Pouchet, whether these secondary painful points were not individual peculiarities, stated, that as far as his observations had extended, he had always found them in the same point, and that, the location of the primary pain being known, it was easy to name the place of the occurrence of the secondary one. In other terms, the locality of the two corresponding points are the same in all subjects.

[The above discussion, as reported, indicates what appear to us to be rather strange physiological misconceptions on the part of some of the speakers. If M. Bert's experiments indicate anything, they show that nerve fibres conduct impressions indifferently in either direction, and that their functions depend upon the character of their central or peripheral terminal apparatus. Thus sensory fibres might conduct impressions in either direction, but for the fact that sensation is always a central phe-

nomenon, and therefore, the conductions of its nerves must be centripetal. The anatomical data in our possession are sufficient to render this probable *a priori*; but there are also good physiological evidences in its favor. Thus, when MM. Vulpian and Phillippeaux united the central portion of the divided lingual with the peripheral end of the divided hypoglossal, they united two receiving apparatuses, so to speak, but, instead of their both being of the same kind—sensory—as in M. Bert's experiment, one was motor and the other sensory. The result, as might have been expected, was a suppression of all function, except when aroused by external irritation along the common line of connection between the two. Then movements of the tongue were observed and, had the observers been able to note accurately and completely the subjective sensations of the animal, they would doubtless have found that the sensory centre of the lingual was also excited.

It is not to be interred that no centrifugal influence is conveyed over sensory nerves—but it cannot be sensory. There are many facts, and among them the one developed by M. Bert in the above experiment, that seem to show that trophic influences pass out over the same routes that convey sensory impressions to the centres. But unless we are willing to admit that consciousness exists throughout the nervous system, in its peripheral as well as its central portions; it seems to us as unreasonable to speak of centrifugal sensory conduction as it would be to talk of centripetal motor impulses acting upon central ganglion cells.]

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CEREBRAL LOCALIZATIONS.—A note by M. Maurice Raynaud was offered at the Acad. de Médecine of Paris at its session, Dec. 5, 1876, (rep. in *Gaz. des Hopitaux*), called out by the memoir of Dr. Proust on cerebral localization, in which he gave the history of a consumptive patient, in whom, three days previous to his death, there appeared suddenly a paralysis limited to the left superior extremity, and affecting exclusively the hand of the fore arm. At the autopsy, the only lesion discovered was a very minute patch of red softening, developed in the right hemisphere, around a meningeal tubercle. This patch, which was only as large as a twenty centime piece, was situated on the ascending parietal convolution, and in the gray substance forming the base of the fissure of Rolando, five centimetres from the superior internal border of the hemisphere. This point is precisely the one that has been experimentally demonstrated to be in the ape, the centre in relation with the movements of the inferior member.

This observation is the only one in M. Raynaud's knowledge, the only one in which the brachial paralysis existed, to the exclusion of every other cerebral phenomena, permitting the establishment of a certain convolution between the functional trouble and the anatomical lesion.

The following are views of M. Onimus, as recently expressed by him at the meeting of the Soc. de Biologie, Feb. 10, (rep in *Gaz. des Hopitaux*):

M. Onimus said that there were many objections to be made to the hypothesis of these localizations. In spite of the tendency that had pre-

vailed since the agitation of this question, to admit the results of MM.
Hitzig and Ferrier, after having studied the works of others on the sub-
ject for the past three years, and after having repeated many of the experi-
ments, he was convinced that this doctrine was supported by no incon-
testible physiological facts. It is probable that there are in the cerebral
lobes, regions possessing special psychical functions, this is the old theory
of Gall, but at the present day, the tendency is to admit motor centres in
the cortex. The phrase " cerebral localizations " is also too vague, for we
can admit the existence of psychic localizations, such as that of the fac-
ulty of speech, without becoming partisans of cerebral localizations in
the actual sense of the word, indicating psycho-motor localizations. M.
Onimus offers the following objections to the physiological facts that gave
rise to this hypothesis :

Every nervous element is put into action either by chemical, physical,
or electrical excitations. A motor nerve causes a contraction, and a sen-
sory nerve a sensation, whether the excitation is produced by mechanical
pressure, cauterization, or by an electric current. But excitation of the
cerebral lobes causes absolutely no motor phenomenon when we irritate,
chemically or mechanically, their cortical portions, we may burn them
with a red-hot iron without producing any movement. Electricity alone,
among all the excitants, produces motor phenomena. Why is this?
Simply because its effect does not remain isolated in the point of applica-
tion like that of other excitations; it penetrates more deeply.

Electric currents are not transmitted by the nerves, but by the organic
liquids, which are always better conductors. The diffusion of the cur-
rents, when applied to the cerebrum, they diffuse themselves in all direc-
tions, particularly downwards into the brain, since they follow the ves-
sels. For this reason, it is near the fissure of Rolando, where the vessels
are very numerous and communicate with those of the corpus striatum,
that we meet with the pretended psycho-motor centres.

Moreover, after removing the hemispheres from an animal altogether,
and replacing them by a mass of bloody matter, M. Onimus had found
that the application of electricity in this mass produced the same effects
as electrization of the cerebral lobes themselves. If the electrodes were
applied superficially to the anterior part of the mass, movements of the
eyelids were obtained; more deeply and posteriorly, those of the members
were produced. There can be no motor centres here, and the experiment
demonstrates very well that currents applied to the cortex penetrate more
deeply.

Electrical currents, acting in a general way upon all the nerves, do not
affect all alike: some are excited and put into action, while others seem
unaffected, or only manifest their function with the use of a very strong
current; there is an elective action. Thus, of all the nerves, those of the
eyes are most impressionable, next, those of the face, and lastly, those of
the members, especially those whose functions are most exercised. Thus,
when we electrize the cerebral lobes, the first movements to be excited are
those of the eyes, and their motions are even produced when we carry the
rheophores over the supposed centres for the members. These phenom-
ena seem to indicate that the results obtained depend upon a general excita-

tion, and not upon any special and local action. We may form an idea of the diffusion and the elective action of the currents, when we remember that in man a very feeble current directed in the neck is sufficient to cause, through the epidermis, the tissues, and the bones, an excitation of the optic nerve. In this case, there is neither cutaneous sensation nor contraction, and yet the current penetrates a considerable mass to excite at a distance the optic nerve.

In the lower animals, destruction of the cerebral lobes causes no disorder of motricity, but even in the higher animals the troubles so caused are very different from those obtained by injuring other portions of the encephalon. A simple puncture, or the presence of a very minute foreign body, suffices to produce considerable modification in the motor symptoms, while in dogs, rather extensive lesions cause only a transient enfeeblement.

Further, we have, from the *ensemble* of the experiments performed upon the cortex, the following curious and contradictory result, that the destruction of the cortical regions, *the electric excitation of which causes contraction of the flexors, does not induce paralysis of these muscles, but rather of the extensors.* Thus, the movement we obtain in the anterior members by electrizing certain portions of the cerebral lobes, are those of flexion and adduction, and if we remove these parts with a curette, the animal exhibits no symptoms, or rather he shows for from two to four days a paralysis of the extensors. This transient paresis is here only the result of the shock to the nervous centres, since it is almost a general law that every momentary enfeeblement, not localized, shows itself by paresis of the extensor muscles of the fore-arm.

Further, every excitative action on the whole of the nervous centres, or of a plexus, causes movements of flexion and adduction; this is likewise the case, in clinical observations.

En resumé it is incorrect to say that the excitation of the cerebral lobes *functionally*, produces motor phenomena, for none of the excitants, except electricity, produce these results. The electrical currents are an exception only because they penetrate more deeply, by reason of the conductibility of the liquids, and they produce the same effects when the cerebral lobes are replaced by an amorphous mass, also a conductor of electricity. Moreover, the transient paralyses, following destruction of the cortex do not correspond at all to the groups of muscles caused to contract by the excitations of these same regions. The only fact that agrees with the experiments on which the theory of psycho-motor centres is based, is that excitations applied to the vicinity of the fissure of Roland, act more energetically on the inferior nuclei than when applied to other regions of the brain.

M. Duret, following M. Onimus, said that there were three methods of experimentation available for demonstrating the existence of cerebral centres: 1. Electrization; 2. Ablations of parts of which we wish to learn the physiological functions; 3. Clinical and surgical observations. The electrization experiments are exceedingly difficult to interpret, on account of the diffusion of electricity; there are, in fact, regions that do not respond to electricity, and he asked a physiological explanation of this fact.

He did not think, with M. Onimus, that the conduction of the vessels suf-ficed to explain certain phenomena of electrization that were observed. The experiments by ablation, on the other hand, gave us to-day, positive facts and results perfectly clear and well defined.

M. Onimus said he had never any permanent paralysis by the mere ab-lation of a part of the brain in animals.

M. Charcot observed that certain pathological facts, too well studied, and already very numerous, also were in favor of localizations, and that we cannot counterbalance them by experiments upon animals.

M. Laborde asked to be allowed to add a fourth to the three methods mentioned by M. Duret, for the study of cerebral localizations, one that he had attempted to introduce into physiological experimentation; he re-ferred to the artificial production, in the brain of the dog, of hemorrhages exactly similar to those observed in man. He recalled the fact that he had communicated to the society the results of these experiments, and that he had obtained by this procedure, symptoms exactly like those observed in man in cases of cerebral hemorrhages. These facts demonstrate that there are in the brain of the dog, limited regions, the excitation of which pro-duces perfectly localized phenomena.

M. Lepine remarked that M. Laborde's method was a great advance in physiological experimentation, and that it was much to be preferred to that of Goltz, who had destroyed portions of the brain by a jet of water, at high pressure introduced through a small orifice in the skull.

The Vaso-Motor Nerves.—P. Bricon. *Thèse de Strasbourg*, 1876, (abstr. by M. Duval in *Revue des Sci. Méd.*) Examining the mode of action of the so-called vaso-dilator nerves, the author is led to adopt the theory of the autonomous contraction of the vessels (the theory of Legros and Onimus), that is, the theory of peristaltic contraction.

His experiments may be stated as follows: 1. Whenever, after section of a nerve containing vaso-motor fibres, we apply feeble irritation to its peripheral portion (multiple sections, chemical agents, or weak induced currents) we always obtain in a healthy adult animal an elevation of temperature and a dilatation of the vessels of the corresponding member. 2. If the irritation is produced by a strong induced current, we obtain a lowered temperature and a vascular constriction.

Considering the section of the nerve as one of the class of feeble irrita-tions, the author is led to admit that the immediate effect of the opera-tion is an active and not a passive one, that is to say that the peripheral vascular dilatation is not due to paralysis but rather to excitation of the vaso-motors, which, producing peristaltic and autonomous contractions of the vessels, enlivens the circulation. On the other hand every violent excitation produces a spasmodic contraction of the vessels, and conse quently a diminution of the sanguine afflux. There is, therefore, but one species of vaso-motor nerves, and the vaso-dilator and vaso-constrictor effects will be due solely to the degree of excitation of the same nerves, inducing sometimes peristaltic action, sometimes tetanic contraction of the

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arterioles. There follow some experiments made upon tubes of India
rubber, compressing them with a wooden roller, to imitate the compres-
sions produced by the peristaltic action, and to show that these compres-
sions favor the circulation and augment the flow of the liquid.

THE INFLUENCE OF INDIRECT CURRENTS UPON THE VESSELS. — A
recent communication by M. Onimus in the Soc. de Biologie (Dec. 30,
1876), is thus reported in the *Gaz. des Hopitaux*. It is known, said he,
that electrization of the vaso-motor nerves causes contraction of the
smooth fibres contained in the vascular walls, contracts the caliber of the
arterioles, and diminishes the circulation. The conclusion has been
drawn from this that excitation of the vaso-motors always provokes per-
manent contraction of the vessels and reduces the circulation. Account
was not taken, however, of the excitation, with ordinary induced currents;
we have, then, in effect, to deal with a series of rapid irritations that
cause a tetanus of the muscular elements of the vessels. If with the same
intensity of current the number of the interruptions is varied, the
phenomena of vascular contraction and reduction of the temperature
diminish alike and in the same proportion. When there are only one,
two, or three excitations per second, the temperature instead of being
lowered, is slightly elevated, and the circulation appears more active.

Under the microscope, we observe in the natatory membrane of the
frog, the capillaries dilating and the circulation becoming more active
when we electrize the vaso-motors with indirect currents, interrupted once
or twice every second. The same occurs in warm blooded animals.
We may conclude as follows:

1. Rapid excitations affecting the vaso-motor nerves cause tetanic con-
tractions of the vessels, and diminish the circulation.

2. These same excitations, when they approach in their order of suc-
cession, the normal conditions of the rhythmic movements of the vessels
enliven these movements and render the circulation more active.

These facts also confirm the theory of the autonomous contraction of
the vessels, since they show that, if indeed the tetanic contraction of
the vessels reduces the circulation, successive and *rhythmic* contractions,
on the other hand, increase the sanguine flow.

It is important in these experiments, to employ currents of moderate
intensity, since with very feeble currents we often obtain, even with rapid
interruptions, a slight vascular dilatation. These last facts accord with
those observed by M. Goltz and M. Bricon, one of his pupils.

The differences in the number of the excitations in a given time, and
the influence of the intensity of the induced current upon the peristaltic
movements of the intestines, have a very striking analogy with those
observed in the vascular phenomena.

THE DIFFERENCES OF THE ACTION OF THE TWO PNEUMOGASTRICS.—At
the meeting of the Soc. de Biologie, Dec. 9, (rep. in *Gaz. des Hopitaux*).

M. Leon Tripier (of Lyons), presented in his own name and that of M. Arloing, a communication on the different actions of the two pneumogastric nerves.

He referred to the fact that in June, 1872, M. Brown-Séquard had kindly presented for them a former communication on the same subject. They had claimed in that paper that the right pneumogastric acted more energetically upon the heart than the left, and *vice versa* as regarded the lung. (See this JOURNAL, No. 1, Vol. I., Jan., 1874). At the same time, M. Masouin (of Liege) arrived at identically the same result as regards the heart. Since then the experiments have been repeated by M. Tarchanoff. A little later they published in the "*Archives de Physiologie normale et Pathologique* " the whole of their researches on this subject, and in the part that more particularly related to the heart, they always admitted the fact of the inequality of the action of the two pneumogastrics, but as they had found in a few cases a predominance of the left vagus, they were less positive. They also gave more details on the modifications produced in the respiration after division of the left pneumogastric. Finally they stated that all their attempts to discover a difference of action of the vagi on the œsophagus and stomach were fruitless, or rather indecisive.

Nevertheless, at that time they had observed two cases of death after section of the right pneumogastric in the ass; but this had caused no surprise, it had been long admitted that death sometimes followed section of the two pneumogastrics. Observing the relative frequency of this event, they sought to ascertain the ratio of this frequency. In twelve sections in the ass, death occurred seven times, in four of which it was the right, and three the left pneumogastric that was cut. In nine sections of the nerve in the rabbit, death occurred three times, the right nerve having been operated upon in all three. In more than forty operations on the horse, it occurred only once, and then it was the right nerve.

From these we conclude that division of one vagus may cause death, and that division of the right nerve is more likely to produce this result than that of the left.

In all these cases the stomach was found filled with food, and particles of the same were met with even in the air-passages. This is very well explained by the paralysis of the lower part of the œsophagus in section of the pneumogastric in the middle cervical region. It is none the less true, that we know as yet, relatively nothing of the causes and nature of the phenomenon. Therefore, MM. Arloing and Tripier hesitate to pronounce upon it. But whatever they may be, these facts have a double interest, both in a physiological and a practical point of view.

In the discussion that followed, M. Tripier stated, in reply to a question of M. Trasbot, that he had not specially observed for any pulmonary lesions following the divisions of the pneumogastric that might cause death, but had had in view the difference of action of the two vagi nerves.

M. Claude Bernard rejected Traubes explanation of these phenomena, that in these cases the animal died from the results of the admission of foreign bodies in the air passages, setting up inflammations and abscesses.

But if we divide the two vagi in a rabbit, and then introduce a tube into the trachea, in such a way as to allow it to respire freely and prevent any foreign substance from entering, the animal still dies with patches of pulmonary disorder, and the quicker the younger it is. It is a question of the resistance of the lung; moreover, all the questions attaching to this subject are exceedingly complex

To an objection of M. Trasbot, M. Claude Bernard said that he did not deny that animals might sometimes die in the way indicated, but that that was by no means always the case, and the question was a very complicated one, and required much more study.

M. Lepine asked M. Tripier if he would not admit, to explain the difference between the action of the two pneumogastrics, either a greater number of fibres in the right, or, what is perhaps more probable, a difference of action of the two cerebral hemispheres from which the nerves arose, and a sort of preponderant influence of the left hemisphere over the right, explaining the preponderance of action of the right nerve, there being a decussation of the pneumogastrics as of the other nerves. There would be, therefore, something analogous to what we observe in aphasia.

M. Tripier replied that he had begun, with his collaborator the study of the mechanism, but that he was not yet able to report upon it, and that in this point he agreed with M. Claude Bernard.

THE NERVES OF THE OVARY.—The following is a preliminary communication by Dr. Julius Elischer in the *Centralblatt f. d. med. Wissensch.* No. 50, 1876, on the finer details of the ovarian nervous supply.

While the anatomical course of the ovarian nerves may be considered, as decided by the researches of Frankenhaeuser, a knowledge of the finer histological details of these elements has hitherto been lacking.

The majority of investigators have not agreed as to the presence in, and the entry with the vessels into the hilus of the ovary, of the nerves, and hence it seemed a useful task to seek out the nerves in the stroma and to follow them to their terminations, which I have done so far in the rabbit, the sheep, and the cow.

The best method has been, hardening the specimens in a two per cent. solution of chromate of ammonia for a short time, changing the fluid daily, then coloring with gold chloride, according to the Gerlach-Boll formula, and finally the restoration of the dissected preparation to the hardening fluid again.

In all the animals examined there were found fine twigs of medullated nerve fibres entering the stroma with the tortuous vessels, and also in the hilus as well as in the ligamentum ovarii proprium. These branched off from the hilus after two fashions. While one division passed as medullated fibres to the follicular layer of the periphery, and there began to branch dichotomously into a finer and finer network of non-medullated fibres, that are sometimes extended directly around the follicle, but that sometimes form very tortuous curled branches,—another division remains

as a coarse-meshed network, visibly encircling the vessels. The riper the follicle, *i. e.*, the more developed its granular membrane, the more prominently the separate parts of the follicle are seen and simultaneously there is formed in the theca folliculi a coarse meshed numerously anastomosing network of tolerably large fibres, out of which an elongated meshed fine fibred network arises and lies upon the peripheral layer of the membrana granulosa. This last, through its branchings, forms a dense network which surrounds the membrana granulosa and which is easily recognized as a nerve plexus by numerous knots and varicosities. It is somewhat more difficult to find places where twigs from this plexus enter into the cells of the granular membrane.

I have, nevertheless, seen many cells, of the granular membrane (distinguished by well-marked processes, etc.) into which the nerve twig entered and disappeared in the nucleus.

The most suitable object for examining the nerve terminations is the ovary of the sheep, and with care as to methods the plexuses I have above sketched, may be found without too much trouble, especially if the remaining deposit of gold chloride is cleaned off from the section with a brush in oil of cloves.

THE CORRELATION BETWEEN THE DISTRIBUTION OF THE CEREBRAL ARTERIES AND THE PHYSIOLOGICAL REGIONS OF THE BRAIN.—At the session of the Soc. de Biologie, Jan. 6, (rep. in *Le Progres Médical*) M. Duret exhibited designs and injections intended to show that there exists a great correlation between the distribution of the arteries and the physiological regions of the brain. As in man, so in the dog, cat and rabbit, the territory of Sylvian artery corresponds very nearly with the situation of the voluntary motor centres described by Ferrier. Hence we may divide the brain into three great regions, having the same limits as the vascular territories, the motor, the sensory, and the intellectual regions, corresponding to the distribution of the Sylvian, anterior cerebral, and posterior cerebral arteries, respectively. The lobes and the convolutions are only accidents of the region, playing a secondary *role*, M. Duret took up the study of the development of the brain in this point of view; he attributed a preponderant influence to the mechanical action exerted by the cranium on the encephalic vesicle at the moment of the formation of the folds of the hemispheres. It is necessary to say that, the smaller the facial angle, the nearer to the anterior parts of the skull is the fissure of Rolando in man, and the crucial sinus in the lower animals. The skull has already its definite form and is resistant when the encephalic vesicles are still smooth and membraneous, compressed between the anterior and posterior halves of the skull, they fold themselves in the direction of the slightest force, that opposite the base of the two cerebral nuclei, already formed at this time; the smaller the bony skull, the more direct is the force it exerts, and the more anterior are situated the fissures of Rolando and the crucial sinus.

There is a third point insisted upon by M. Duret. There exists for the third convolution in man, a special artery, met with in all animals whose

brains have been studied, and it occupies an analogous position, even in those whose brains are smooth; it corresponds to the centres described by Ferrier, for the movements of lips and tongue. Following this idea, M. Duret extirpated this region in three dogs, to see whether the same phenomena as those observed in man after injuries of the part, would follow the lesion. Although the result was not quite definite, it still appeared that two of the animals had lost the power of barking.

THE RAPIDITY OF SENSORY PERCEPTIONS.—M. Chas. Richet reported to the Soc. de Biologie, Dec. 2 (rep. in *Gaz des Hopitaux*) the results of his studies in sensibility by experimentation on healthy individuals. From these he laid down the following propositions:

1.   The sensibility, put in play by a feeble excitation, decreases slowly, but rapidly returns to a normal condition after even a very short period of rest.

2.   Multiplied excitations produce a sensory effect beyond that which a single excitation can incite, this fundamental fact can be explained only by addition (summation of Gruenhagen and Pflueger), by which the successive excitations accumulate in the brain.

3.   The direct consequence of this proposition is, that slight excitations are very slowly perceived, while stronger ones are nearly instantaneous: this is true only for series of electric excitations, but is not correct for single excitations, which, whether strong or weak, are felt at the same time.

4.   A relation exists between the frequency, the intensity and number of the excitations, thus the number of the excitations necessary to provoke a perception, is in an inverse ratio to their intensity and frequency.

5.   The persistence of an impression in the nervous centres is in direct ratio to the intensity of the excitation that produced it.

From all these facts it follows that we must distinguish the excitation of the nerve and the excitation of the nerve centres. Excitation of the nerve is a simple transmission; excitation of the centres is a sort of shock, of vibration that endures a long while after its primary excitation. In this point of view the nerve centres resemble the muscles, and there is a direct relation, not between the functions, but between the form of the functions of these two tissues.

THE ELECTRIC IRRITABILITY OF THE FROG'S BRAIN.—O. Langendorff. *Centralblatt f. d. Med. Wissensch.* No. 53.   1876.

A series of experiments performed by the author upon the electric irritabilitty of the cerebrum of frogs have led to the following results:

1.   By irritation with weak, constant or interrupted currents of certain portions of the hemispheres, we may induce movements of the muscles of the body.

2.   By simultaneous irritation of both hemispheres we produce movements of all four members and of some of the muscles of the back. Unilateral excitation produces movements of the body and of the extremities of the opposite side.

3. The "irritable zone" lies in the parietal section of the hemispheres. Irritation of other parts produces no effect when weak currents are employed.

4. After complete separation of the cerebrum from the parts of the central nervous system lying behind it, the results of irritation of the hemispheres disappear.

5. Ether narcosis destroys the electric irritability of the cerebrum. On the other hand it is not affected by complete removal of the blood from the body of the frog.

6. There is one point in the uninjured skull of the frog, electrical irritation of which produces the same effect as direct application of the current to the same side. This point lies between the tympanic membrane and the eye, and can be easily located by a suture-like dark line connecting the eye and ear.

The author is still engaged upon his experiments and hopes soon to give further results.

THE PHYSIOLOGY OF THE PERSPIRATION.—At the session of the Berlin Physiological Society, Jan. 26, Herr Adamkiewicz offered a communication upon the physiology of the sweat secretion which is thus reported in the *Deutsche Med. Wochenschrift.*

Physiology is acquainted with two causes for secretion within the glandular substance;—physical force causing the processes of filtration and physiological causes acting through the direct mediation of the nerves. Hitherto, we have had only one experimentally confirmed example of this second kind of agency in the production of secretion, that of the submaxillary gland and its functional relations with the excitation of the chorda tympani and the sympathetic. Herr A. had been able to demonstrate by experiments upon men that the sweat secretion is also a nervous act which may be induced by artificial irritation of centrifugal nerves (Luchsinger has recently arrived at the same conclusion from experiments upon animals). These nerves follow the motor routes of the regions concerned and react, under favorable conditions to irritation, for which the influences of temperature are of importance, within less than one minute. They have a common centre for the lower extremities, in the spinal cord, are not directly dependent in their action upon the circulatory processes, and indicate that the process of the secretion of sweat is a simple reflex act, obeying the well known laws of these actions, and properly to be classed among the bilateral reflexes.

NUCLEI OF THE GANGLION CELLS —G. Schwalbe, *Jenaische Zeitschr. f. Naturw. X.*, 1876, 25, (abstr. by Loewe in *Centralbl. f. d. med. Wissensch.*).

If a fresh, still perfectly transparent retina of a sheep is carefully spread on the object platform, with its inner surface in the vitreous humor, upwards, it is easy to perceive the ganglion cells, well defined, in a fresh state, in the peripheral parts adjoining the ora serrata, since here the mass of nerve fibres are reduced to scattered bundles. We recognize-

imbedded in a smooth, shining homogeneous mass, certain round, clear spots, which seem as if filled with fluid. Closer examination reveals within these clear spaces a circular nucleus, having every appearance of a ganglion cell nucleus. The whole surrounding space is clear, with the exception of a little circle of finely granular substance around the nucleus. The addition of iodized serum renders the whole transparent space turbid, and apparently finely granular. As an argument against the supposition that the granular layer is nervous, Schwalbe adduces the entirely different optical condition of fresh ganglion cells, and fresh granular substance. The latter appears as if penetrated with numberless little vacuoles. The shining homogeneous substance between the ganglion cells, allows no recognition of any trace of form elements, and is plainly comparable to the cementing material of the epithelium. The nuclei of the ganglion cells of the retina possess a membrane, the inner surface of which is furnished with little prominences. Often they contain a pointed nucleolus, furnished with feathery processes. This is formed of the same nucleolar substance as the nucleus membrane and its excrescences. In adult animals the differences in the size of the ganglion cells are proportionately slight. In young animals (calves) they are, on the other hand, extraordinarily large, and the nuclei in proportion. The smallest nuclei are the youngest. They have no trace of nucleolus, and consist of a uniformly granular mass. A differentiation into nuclear membrane and contents is impossible. The substance which later forms the membrane and nucleolus, is, at the beginning, uniformly distributed through, and fills the whole nucleus, and is filled with little cavities containing another substance. With the growth of the nucleus, the vacuolar substance increases, without, at the same time, any perceptible addition to its other constituents. The result is that the latter is divided into different portions, of which one always occupies the superficies of the nucleus to form the so-called nuclear membrane, with numerous pointed projections, the mural nucleoli which project toward the interior of the nucleus, while other portions gather into one or more nucleoli. In the measure, as the clear substance within the nucleus increases, so the inner prominences of the nuclear membrane constantly flatten out in consequence of the increased extension of the latter. The whole process may be looked upon as a vacuolizing similar to that observed in vegetable cells during the separation of the protoplasm from the cell fluids. In ganglion cells in other localities (anterior horns of the spinal cord in rabbits and pigs, the gasserian ganglion of rabbits, the spinal and sympathetic ganglia of the frog), the nuclear membrane is lacking, and also the mural nucleoli, (*i. e.*, the thickenings of the nuclear membrane on its inner surface). The clear nuclear fluid with a vacuole containing nucleolus, immediately borders the cell substance. This observation of Schwalbe is not in agreement with the statement of Auerbach, that the nucleoli enter the nucleus from the cell protoplasm. According to Schwalbe, this, like the nuclear membrane, is formed from the original nuclear substance, since the latter, through the accumulation and increase of the transparent nuclear fluid, is ruptured in several places. There is, moreover, no increase of the nucleolar substance; this remains the same,

and even relatively decreases in comparison to the growth of the nucleus Hence, it follows that we see in the ganglion cells, in opposition to Auerbach's statements as to other cells, a multi-nucleolar condition precede a uni-nucleolar one, and that this latter may pass into an a-nucleolar condition, in which the general nucleolar substance is changed into nuclear membrane. Schwalbe comes to the conclusion that Auerbach's statements as to the formation and increase of nucleoli, are not of general application. In the bodies of the spinal ganglion cells of the frog there are two substances, one of which forms a very fine net-work that extends from the membraneless nucleus to the external cell wall, while the other more transparent substance fills its interstices. The substance of the nuclear granules appears optically different from both; on the other hand, the nuclear fluid seems identical with the substance, filling the interstices of the meshes of the net-work. We have, therefore, three substances to distinguish in the ganglion cell—the nucleolar substance, the nuclear fluid (cell fluid), and the reticular substance. The opinion of Max Schultze as to the fibrillary constitution of the nerve cell, depends, according to Schwalbe, on an imperfect appreciation of the reticular substance. In conclusion, he calls attention to the variations in the structure of the ganglion cells from different localities.

NERVOUS SYSTEM OF THE CETACEA.—At the last meeting of the British Association for the advancement of science (rep. in *Nature*, September 28, 1876), Dr. D. J. Cunningham read a paper on the Spinal Nervous System of the Cetacea. He found that while great similarity prevailed between their cervical and dorsal nerves, and those of other mammalia, the nerves of the lumbar and caudal regions differed widely. The superior and inferior divisions of those nerves in cetacea, were of nearly equal size. Two great longitudinal cords, or trunks, are formed by their union on each side of the vertebral column, and these become situated on either side of the spines of the vertebræ, and on either side of the bodies below the transverse processes. These great cords supply the four great muscular masses which act upon the tail.

The following are the titles of some recent papers on the Anatomy and Physiology of the Nervous System:

VINTSCHGAU and HONIGSCHMIED, Experiments on the Reaction-Time of a Gustatory Sensation, *Pfluegers Archiv.* XIV., XI. and XII, 529; KUEHNE, The Coloration of the Retina and Photography in the Eye. *Rev. Scientifique,* March 3; LAUTENBACH, The Conducting Power as Distinct from the Receiving Power of the Nerve, *Phil. Med. Times,* March 17; CHARCOT and PITRES, Contribution to the Study of Localizations in the Cortex of the Cerebral Hemispheres, *Rev. Mensuelle de Médicine et de Chirurgie,* January, February and March, (Cont.Art.); GRANVILLE, Ideation in Utero—a suggestion; *Lancet,* March; MARCACCI, Determination of the Excitable Zone in the Sheep's Brain. *Archivio Italiano,* January.

b. PATHOLOGY OF THE NERVOUS SYSTEM AND MIND. AND PATHOLOGICAL ANATOMY.

APHASIA.—The following are the conclusions of a lengthy memoir by Dr. Augusta Tamburini, concluded in the last issue (II. Fasc. V. and VI., Sept.-Dec., 1876) of the *Rivista Sperimentale di Freniatria e di Medicina Legale*:

1. The faculty of language is a complex function, constituted of various elements, isolated in their pathological lesions, which may be put in relief by clinical analysis.

2. Anatomo-pathological researches, based upon a large number of cases, and supported by physiological data, have been able to localize each of the functional moments of language.

3. The memory of verbal images and their co-ordination, which constitute the more intellectual part of language, have their seat in the gray matter of the cerebral hemispheres, and more particularly in the frontal lobes.

4. The transformation of sensory (optic, acoustic, etc.) impressions and their verbal reflexion in a motor impulse toward the extrinsic muscles, has its localization in the marginal convolutions of the Sylvian fissure, and more especially in the third frontal convolution.

5. The route followed by the motor impulse to join the nerves animating the muscles of speech, runs from the convolutions of the insula (which are only the continuation of the marginal ones), by way of the nucleus lenticularis and the corpus striatum, traverses the cerebral peduncles to the protuberance and the medulla in which are found the nuclei of the muscles of phonation.

6. The motor impulse sent to the phonetic muscles is co-ordinated from its point of origin, and it is not needful to look for co-ordinating centres along its course.

7. Neither physiology nor pathology authorizes us to retain the olive as the special co-ordinating centre for speech, to it, and to the bulbar system in general can be attributed the bilateral harmonization of speech as of every other voluntary movement.

8. The lesion of any one of the points in which are located the mnemonic sonorous images of words, in which thus kept they are transformed into motor excitations, and from which they are transmitted as centrifugal impulses to the executive muscles of speech, may produce aphasia of ideation, of transformation, or of execution, and secondly, according as the seat of this lesion is more or less central, the functional disturbance may be more pronounced in the intellectual or in the peripheral phenomena of speech.

9. The facts adduced against the functional localization of speech are explained either by the unilateral predominance of this function, or by the laws of cerebral compensation, and of irritative irradiation.

10. Aphasia, although met with more frequently as a symptom, may nevertheless be studied as a malady of itself, taken in the sense and in cases of simple alteration of one of the elements constituting the function of speech.

CEREBRAL HEMIANÆSTHESIA.—At the meeting of the Soc. de Biologie, Feb. 24 (rep. in *Le Progres Medical*), M. Raymond Tripier reported a series of researches by himself upon incomplete hemianæsthesia of cerebral origin which indicated that the sensory disturbance is in its course like the motor one. The anæsthesia is most pronounced at the terminations of the nerves; pressure upon a part of the skin caused anæsthesia of this and neighboring points. Sensibility persisted in the deeper lying tissues even when lost at the surface.

PERIPHERAL PARALYSIS.—At the meeting of the Physiologischen Gesellschaft of Berlin, Jan. 26, an address was delivered by Herr Bernhardt on the subject of Peripheral Paralysis, which is reported as follows in the *Deutsche Med. Wochenschrift* of Feb. 10:

Our knowledge of the paralysis of peripheral nerves is apparently an acquisition of the most recent times. Only since the introduction of the method of electrical examination into the pathology of nervous diseases, have we been able to obtain a nearer insight into these processes. We distinguish them generally into the light and the severe forms of paralysis. Those forms are called light that reach a complete cure inside of a few weeks, and that in the electrical examination are characterized by perfect retention of the electric irritability of the nerve-muscle tracts, not differing in this from the sound side. Those cases are called severe, in which the irritability of the affected nerve sinks so perceptibly within ten or twelve days, as to be as good as lost entirely for such currents as may be safely employed in living human beings. This is true for the first two or three weeks of the disease in regard to both nerves and muscles, and also for both currents. The speaker then examined the hyper-excitability of the diseased muscles to the galvanic current coming on in the course of the third week, and explained the well-known observations upon the "*Entartungsreaction.*" As especially interesting, he mentioned the condition, appearing at the time of the regeneration of the affected nerve, in which the muscle reacts to the voluntary impulse, or to electric irritation applied above the affected spot, while it still exhibits the "*Entartungs-reaction*" to direct galvanic excitation. Erb and the author had recently described another (middle) form, in which this peculiar phenomenon, appearing in severe cases first, after the lapse of a month, is present even inside of a week. These facts (the possibility of direct and indirect excitation of the muscles by the faradic current, the "*Entartungs reaction*" of the same with indirect galvanic irritation) led the speaker to think that

in this form of paralysis, the muscles might be the points of attack, they thus proving by this condition their own special irritability independent of any nervous influence.

To these three forms of paralysis, the slight, medium severe, and the severe, the speaker associated a fourth, very interesting, from which he very recently (October, 1876,) had observed in two instances. These were in two cases of radial paralysis in men, in whom irritative phenomena were not produced in the extensor muscles (with the exception of the triceps, and in one case also the supinator longus), by the application of either the faradic or the galvanic current at the point of trouble in the radial nerve in the upper arm, or from still higher points (supra-clavicular fossae). Direct irritation of the muscles, on the other hand, by placing one or both electrodes below this point, and on the dorsal aspect of the forearm itself, caused a good, prompt, contraction of the extensors; it seemed indeed as if the contractility at the beginning was even higher than that on the sound side. This occurred with the use of either current, the contractions were instantaneous; there was no predominance of the opening contractions, or that from the closing of the anode, over that from the cathode closing noticed. All this was observed not only between six and ten days after the paralysis, but also after three, four and six weeks, so that we have here a remarkable example of a complete interruption of the conductibility of both voluntary and electrical irritations, with evident integrity of the nerves and muscles below the point of injury. Besides the speaker, only Vulpian and Brenner had hitherto described similar cases, and Vulpian had called attention to the analogy of the phenomena with those of curarized muscles. The speaker suggested that, perhaps in these cases we might infer an influence of cold acting somewhat in the same manner as curare on the terminations of the nerves. This, however, he considered as purely hypothetical. If, since we have not the opportunity for autopsies in these cases, we attempt an explanation on the basis of the data previously afforded by Erb, we must suppose an interruption of nervous conduction at one point in a nerve trunk, sufficient to prevent the passage of any excitation from points higher up in the course of the nerve, but not serious enough to cause the production of the degeneration of the portion of the nerve situated below (towards the periphery) the point of interruption, that is usually observed in such conditions.

THE COLLECTIONS OF WHITE BLOOD CORPUSCLES IN THE CEREBRAL CORTEX.—Herzog Carl, of Bavaria, who, it appears, has chosen the honorable profession of medicine, publishes (*Virchow's Archiv*, LXIX, I, p. 55), the results of a series of microscopic examinations, incited by the discovery of Popoff (*Virchow's Archiv*, LXIII.) of the presence of accumulation of white blood corpuscles in the cortex in typhus and typhoid fever, and the statement of that author that they were also present in the substance of the ganglion cells, causing, in that case, a nuclear prolification of the same. He, therefore, in order to test, as fully as possible, the correctness of Popoff's statements, commenced his examinations on the

brains of subjects of typhoid fever, using, as far as possible, the same portions and similar methods of preparation with carmine. Later, he extended his researches into other parts, and also adapted another color-ing agent, aniline, which he found more satisfactory. He examined the brains of twenty-two cases of typhoid altogether, and of cases that had had various durations of the disease, while Popoff's cases were only twelve in number, and all of the second week. Our author also examined the brains in various other diseases—measles, pneumonia, phthisis, acute atrophy of the liver, cancer, etc., as well as two perfectly normal brains of individuals who died suddenly from traumatism. He sums up the results of his examinations as follows:

1. White corpuscles are present in the cortex in every brain, even the most normal one.

2. Retarded circulation, with increased watery contents, causes an in-creased collection of these white blood corpuscles.

3. The corpuscles lie, besides in the perivascular spaces of His and the adventitial canals of Robin, preferably in the periganglionary spaces.

4. They never penetrate the substance of the ganglion cells and produce multiplication of the nuclei of the latter.

5. The cerebral symptoms do not depend upon the presence, nor at all upon the increase of the white corpuscles, nor upon the retardation of the circulation, or the increased watery contents, though they follow in a striking manner, but are to be considered simply as the expression of an acute aggravation of all three moments, for which the fever process and its causes is to be held responsible.

The author closes his paper with a description of the special appear-ances observed by him in several cases of disease in which he examined the brains, for the details of which we must refer the reader to the article itself.

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CEREBRAL COMMOTION.—M. Dumenil (of Rouen) read before the Soc. de Chirurgie, of Paris, Jan. 24 (rep. in *L'Union Médicale*), a memoir entitled *Note to Serve for the Study of Cerebral Commotion.* He sought to show in this paper, which was based upon two observations with autopsies per-formed with the minutest care, "that we cannot refer all cases of cerebral commotion to contusion of the brain; that in a certain number of cases we can only distinguish, of important lesions, those that follow disorders of the circulation; that these troubles, which should be explained by paralysis of the vaso-motors, are the direct cause of death by their gen-eralization, and especially by the impediment that they cause in the pulmonary circulation, and that consequently we should consider them as the characteristic of cerebral commotion, undergone by the central nervous system, or, in other words, as the effects of profound perturba-tions in the vaso-motor functions. This is somewhat analogous to the relation, admitted by M. Verneuil, between hernial strangulation and pulmonary congestion, with the difference that we may suppress, in such cases as those of M. Dumenil, the intermediation of the reflex acts."

SUDDEN DEATH FROM EXCITATION OF THE PNEUMOGASTRIC AND LARYN-
GEAL NERVES.—At the meeting of the Soc. de Biologie, December 2, (rep.
in *Gas. des Hopitaux*) M. Bert, *apropos* to the instances recently reported
of death following thoracocentesis, recalled his experiments performed
many years ago, in which he had found simple excitation of the nasal,
superior laryngeal, or pneumogastric nerves in animals, to cause not
merely arrest of respiration, but even sudden death. Death in these cases
is never the result of syncope or asphyxia. For example, if we compress
the trachea of a duck, a hard-lived animal, as is well known, death in-
stantly occurs. Five minutes later the muscular contractility is gone,
and cadaveric rigidity very quickly appears. M. Bert is disposed to at-
tribute the result in these cases to a commotion in the floor of the fourth
ventricle, to a shock bearing on the vital knot by a centripetal action
having for its point of departure an irritation of one of the above
mentioned nerves. The majority of cases of sudden death that have been
observed, either from foreign bodies in the larynx, or following ammoni-
acal cauterizations of the pharynx, or even after the ingestion of ice, are
evidently, according to M. Bert, instances like those he has mentioned.
And, finally, may not the sudden deaths in typhoid fever be explained
better by these facts than by reflex action starting from the intestines.

NEURITIS.—Prof. H. Nothnagel, *Volkmanns Samml. Klin. Vortrage*,
No. 103, (abst. in *Deutsche Med. Wochenschr.*) Prof. Nothnagel adds to the
account of a case of traumatic tetanus, in which, in spite of indications of
abnormally increased irritability of the implicated median and radial
nerves, the autopsy showed no trace of an inflammatory process, an ex-
tended account of the characteristic symptoms for the diagnosis of neu-
ritis as it occurs in the sensory, motor, or mixed nerves. We must rec-
ognize an inflammation of a nerve, (1) if in an indubitably peripheral dis-
order in the tract of a mixed nerve, zoster and disorders of sensibility ap-
pear simultaneously; (2) if there occur trophic disorders of the hair,
nails, skin; (3) if there is continuous pain increased by pressure on the
nerve; and, (4) if anæsthesia appears within a few days. In neuritis of a
mixed nerve, motor paralysis may or may not occur; the same is the case
with muscular atrophy; still, the occurrence of the latter indicates neu-
ritis. Redness of the skin, fever, and increased electric irritability, are
less constant.

The secondary affections caused by ascending or descending neuritis,
are especially important, as has been directly shown by the experiments
of Tiesler, Feinberg and Klemm. Still, the progression of the anatomi-
cal process cannot be demonstrated in all cases, the abnormal irritative
condition in an inflamed nerve appears rather to be able to induce these
secondary affections in predisposed individuals, without gross lesion.

In conclusion, the author discusses the conjectural connection of pro-
gressive muscular atrophy, reflex paralysis, secondary (reflex) epilepsy,
as well as chorea and the reflex psychoses, with primary neuritic
processes.

RELATIONS BETWEEN THE MOTOR DISORDERS OF GENERAL PARALYSIS AND CORTICAL LESIONS IN THE FRONTO-PARIETAL CONVOLUTIONS.—At the session of the Acad. de Médicine of Paris, Dec. 5, (rep. in *L'Union Médicale*), M. Foville, *fils*, (of Rouen) read a paper, of which the following are the conclusions:

1.   General paralysis of the insane has for its pathognomonic characters, in a symptomatic point of view, constant disorders of motility; and in an anatomical point of view, a constant alteration of the cortical substance of the fronto-parietal convolutions.

2.   The most recent works have been inclined to attribute the troubles of motility in general paralysis, to histological modifications, more or less pronounced, in the medulla and in the cord; no relation of cause and effect can therefore be established between the constant anatomical lesion and the equally constant symptomatic manifestations.

3.   The discovery by Hitzig and Ferrier of an excitable motor region on the surface of the convolutions of the median part of the cerebral hemispheres, permits us to establish this relation.

4.   The existence in the excitable region of the convolutions of distinct cortical motor centres for the movements of the superior members, the inferior members, the neck, head, tongue, jaws, face, lips, the orbit and the eyelids, enables us to localize with exactitude the ataxias, the convulsions, the contractures, and the partial paralysis limited to such, and such of these organs in general paralysis.

5.   It is by the excitation that causes, in these various motor centres, the initial hyperæmia of the disease and the congestive obstructions in its intermediate stage, and next by the progress of the sclerous degenerations of the period of decline, that we can explain the progressive motor troubles, such as the embarrassment of speech, the fibrillary spasms of the lips and cheeks, the ataxia and dissociations of movement in the members, the grinding of the teeth, the contraction or dilatation of the pupils, the convulsions limited to a single muscle or to a few muscles; the unilateral epileptiform attacks, the partial or transient hemiplegias, the persistent contractures, and, finally, the more or less complete paralysis.

6.   In conclusion, in general paralysis, the cortical lesions of the fronto-parietal convolutions are the direct cause of the motor troubles; and upon the localization and intensity of these lesions depends the locality and the intensity of the spasmodic and paralytic accidents.

DISTURBANCES OF SENSIBILITY IN ATAXIES.—The following are the results of a series of observations on the disorders of sensibility in ataxies, made at the Salpétriere by M. P. Oulmont, and reported by him to the Soc. de Biologie, Feb. 17, (*Gaz. des Hopitaux*). His procedure was to make a figure of the patient, and to mark on these with colored crayons the anæsthetic patches. From these designs, the comparison of about twenty unselected ones, taken as they happened, permitted the deduction of the following conclusions:

1.   The disorders of sensibility, (that to pain is here alone considered), are nearly constant.

2. They are scattered over the entire body, the head even being implicated in a majority of the cases.

3. They are generally disposed with a kind of symmetry, especially manifested on the body and limbs.

4. They occupy in each portion of the body certain points of election. On the head these are the cheeks and supra-orbital regions, while the neck is almost invariably free. On the trunk they are the bosom, the omoplates and the lumbar region, while the sternal region is more frequently intact. In the superior as well as in the inferior members, the lesions of sensibility are more extended and more advanced toward the periphery than nearer the centres. The islets of healthy skin occupy in preference the fold of the elbow and the palm of the hand, and in the upper extremity, and the internal aspect of the thigh in the inferior members.

This division of the anæsthetic patches seems characteristic of tabes dorsalis, and M. Oulmont had been able to make it useful, in two doubtful cases, in confirming the diagnosis.

THE SYMPTOM NUMBNESS.—The following is a part of a clinical lecture by Dr. S. Weir Mitchell, reported in the *Med. and Surg. Reporter*, Dec. 2, 1876.

A man (it is nearly always a man) overworked, or a student who has used his mind too steadily at the age of growth, begins to have, and it is usually in the spring-time, tingling of the hands and feet. Very often I can find no sign of anæmia or of remarkably lowered health, all the functions are in order, and the appetite and digestion are sometimes faultless, but nearly always the heart is irregular, in spells, especially at night. Then also there is sure to be a sense of weight or pain in the occiput. The numbness is purely subjective. It at times invades the whole skin, and the face and scalp are favorite sites. It is here felt in islands, and with it there is often a feeling as if the facial skin was drawn tight. The numbness in these cases is sometimes intense, and the prickling feeling so great as to be painful. I have heard one sufferer remark, that he had once been poisoned with aconite, and that the formication was like that which he then felt.

When this "all-asleep feeling" is most vivid, there is apt to be with it a noise in the head, a faint singing, which is not usually referred to the ears, but is felt in the occipital region, with considerable irregularity of the heart's action. In the graver cases, the finger-tips often burn severely, and there are at times in the hands and feet islets of vasal dilitation. More or less insomnia and general nervousness completes my picture, or rather my sketch.

The numbness which this curious neurosis presents, although most alarming to the patient, is really free from danger; yet also, it is an obstinate malady, unless at the outset it be broken up by some complete change in habits, occupation or residence. After awhile it is not readily relieved, or rather cured, even by travel abroad, but is apt to return again and again. The regular life of a well-ordered water cure, with mild use

of spinal douches or shower baths, I have seen of great value, but I do not know of a single water cure in America which commands the full confidence of the profession, and, unluckily, travel in Europe is not at every one's disposal.

I am puzzled in many of these cases by the fact that the patient seems to be in a good state of physical vigor, so that tonics appear not to be indicated, and are, indeed, as a rule useless; but riding on horseback, exercise afoot, the life of the camp in summer, utter revolution in the ordinary habits—these forms of tonic are serviceable when used with discretion, and one of them, the out-door camp life, is in the power of a vast number of our people.

I have said that you would find this group of symptoms obstinate and troublesome under any treatment, but you will also find that the cases you do not cure get well as time goes on, the changes in life or habits, or the natural revolutions which time effects in all of us, being often sufficient to cure.

There is another form of numbness which may or may not be free from dysæsthesia. I think I mentioned it a few weeks ago in connection with a case of general nervousness; at all events I have been frequently consulted for it. It belongs only to the night, and cannot be confounded with that numbness, with loss of power, which comes of sleeping with the arm bent in some awkward position, or so situated that it is subjected to pressure on one of the main nerves. Of this we see almost every week good examples, but the form of trouble I now desire to recall to your attention is rare.

The last case seen at this clinic was a middle-aged woman, who was in rather feeble health; now and then she awakened with numbness of the whole side, left or right. She was not clear as to whether it affected the face or not. After an hour or more it gradually faded away. Sometimes it affected a single limb, but this was rare. I used to fear this symptom and suspect that it might presage a true hemiplegia, but I have now seen it so often in people who suffered no evil consequences, that I have ceased to dread it. It usually yields to tonics, and is one of the many nameless neuroses which are seen by busy city physicians, and which require that general fortifying of the system which is the most effectual means of dealing with such disorders as grow out of the constant strain and struggle to which modern civilized life subjects those who are involved in its vortex.

When this symptom, numbness occurs in the daytime, as a unilateral trouble, and is associated with headache, or noises in the head, and is found also in the face, and involves some thickness of speech, it is a graver matter. When, also, it occurs in people past fifty, and when there is with it any tendency to inco-ordination of movements in the hand, and the least trace of lessened sense of tact, it should at once put the physician on his guard. Then there is another question to solve. If the patient be weak and anæmic the path is plain, and we need to use good diet, cod-liver oil, and tonics. If there be grave valvular disorder of the heart, we may suspect that a minute embolus has entered one of the vessels, and so affected the blood-supply of the regions in or near the opposite side of

the optic thalamus, or the posterior parts of the corpus striatum. But if the sufferer be a vigorous man, in general good health, with or without distinct evidence of altered vascular walls, there is one remedy which I am sure is of value. It is simply a change of diet to milk, and vegetables and fruit, and a total abandonment of all meats. I do not mean to pause here to reason on the why and the wherefore of this treat-ment. I can only assure you, from long experience, that it is of the utmost value, and that the change is often followed by a continued sense of relief from the numbness, and from all feeling of pressure and fullness. Of course as every one knows, the organic palsies of the spine are apt to set out with sensations of numbness in the feet. These at first, are often unassociated with true, or at least with perceptible changes in the capacity to feel or to localize touch, although this is apt very soon to follow, and to end in more or less dysæsthesia. There is no need to dwell on such well known facts. I should like, however, to remind you that lead poisoning sometimes gives rise to formication, that ague-poisoning sometimes assumes this disguise, and that both, in Bright's disease and saccharine diabetes numbness of the limbs may be met with. I have seen within a week, illustration of both of the latter causations.

A lad of eighteen consulted me last week for formication of the feet, without dysæsthesia. He told me that he had had, three years before, scarlet fever, followed by slight general œdema, but the water had not been examined. The left eye ground showed three small splotches of old retinitis, and he had evidence, of mitral regurgitation. The urine was highly albuminous, and had in it an abundance of fatty and granular casts; also the feet were slightly swollen. Under the use of skimmed milk diet, with tincture ferri in full doses, the numbness is already much better, and the œdema has gone.

Numbness from diabetes is a yet more serious symptom because it is as-sociated with true anæsthesia, and is due, I believe, to plugging of the peripheral blood vessels. I have seen it but two times in this disease; once it was a simple subjective sense of formication, and once it was a grave trouble, resulting in gangrene of one foot and death. Sometimes, however, the slough is local and small, and recovery takes place. I have thus run over some of the rare causes of numbness, and some of the more common ones, but wherever it exists, you will do well to study it thor-oughly, because, whether it has been as a mild tingling, without dysæs-thesia, or as a profound and lasting symptom, with grades of lack of feel-ing, it is always a valuable symptom when viewed with the other signs, which it in turn helps to make clear.

The sensation of prickling, of being asleep—in a word of numbness in its various forms and degrees—is due always to a slight irritation of the nerves, or their connected nerve centres, so that at any point of a nerve track, from the sensory ganglia to its ending in the skin, a slight irritation will give the referred sensation we have been discussing. In the extrem-ities we can cause it by rolling a nerve under the finger, or by an electric current through it, or by freezing the nerve at any point; and thus, in the chapter of accidents, tumors, pressure from any cause, blow, wounds, any-thing which slightly hurts without destroying, may cause numbness. So

too in the centres, all disturbances of nutrition from imperfect circulation, or from small emboli, may cause it, while it is probable that the intrusion on the brain of small aneurysmal dilatations of minute vessels, such as Charcot has described, may have a like result, and, also, it seems, that in the presence of increased blood pressures this symptom may get worse.

Numbness is often associated with other forms of what Erb calls paræsthesia, as distinguished from dysæsthesia, and hyperæsthesia. Among these are sense of local constriction, of burning, of elongation of the limb, a very rare symptom, but nearly all of these curious forms of morbid feelings are due to cerebral disease, and will repay a fuller study and a more detailed description.

NEURALGIAS AND VISCERAL NEUROSES IN CEREBRO-SPINAL DISEASES.—M. Teissier, *pere*, (of Lyons) read a paper on the neuralgias, etc., occurring in the course of cerebro-spinal disorders before the French Association for the Advancement of Science, at its last meeting, the substance of which is thus abstracted in the *Union Médicale*, Sept. 26.

Visceral disorders are met with alike in the first period of general paralysis and of sclerosis of the anterior columns; M. Teissier has observed them, in fact, in a case of angina pectoris, which for many months had marked the commencing symptoms of diffuse encephalitis, in gastric attacks with hæmatemesis, likewise at the commencement of general paralysis; in a case of violent and paroxysmal enteralgia in a lady who afterwards became paralytic; in two cases of convulsive bronchitis, resembling in all respects hooping-cough, and lasting several months in ataxic patients; in many cases of extreme frequency of the pulse with irregularity, without appreciable cardiac lesions; in one isolated case of neurosis of the heart, of which the attacks were later replaced by epileptiform crises.

ATROPHY OF THE GLUTEAL MUSCLES.—At the session of the Paris Academy of Medicine, July 8, 1876, M. Onimus called attention to the atrophy of the gluteal muscles which he had observed in different affections, loco-motor ataxia, traumatisms, sciatica, etc., and which is very likely to be overlooked from the situation of the muscles themselves. The cause of this atrophy may be multiple; myositis, neuritis, etc., may combine to produce it. It is never due to mere disuse, which may cause diminutive but never active atrophy. Electricity, the constant current especially was, the most efficient curative measure.

INVOLUNTARY BACKWARD MOVEMENTS IN DISEASE OF THE BRAIN. Fr. Pengoldt *Berliner klin. Wochenschr*, Sept. 18, 1876, gives an account of a case, in which at the close of a lingering pulmonary affection, among other cerebral symptoms, there appeared a backward motor impulse; when the patient attempts to move forwards, he actually progressed backwards.

This was observed only at a late period of the disease; paralysis and unconsciousness supervened in a few hours and the patient died.

The autopsy revealed chronic left pachymeningitis, meningeal tuberculosis, old encephalitic traces (yellow patches) in the left frontal and temporal lobes, tubercle in the white substance of the right occipital lobe, and a recent one the size of a pea in the pons. There was also tuberculosis of the lungs with other morbid appearances, and tubercles in the pericardium, liver, spleen, kidneys, and large intestine.

Of all these lesions, the author thinks only one, the tubercle of the pons can be held answerable for the symptoms of backward propulsion noted in the patient. They have not been observed in previous cases of the other lesions, and though the characteristic symptoms of those of the pons have not all as yet been determined with positiveness, he thinks that this spinal symptom of backward propulsion in this case is probably caused by the participation of that part in the general disease in this case. No cerebellar lesion is reported that might account for the phenomenon here.

INFLUENCE OF HEREDITY IN THE PRODUCTION OF CEREBRAL HEMORRHAGE.—At the session of Paris Academy of Medicine, Sept. 19 and 26, 1876, (rep. in *Bull. gen. de Thérapeutique*) M. Dieulafoy read a paper in which he demonstrated that cerebral peri-arteritis, with its consecutive miliary aneurysms, causing cerebral hemorrhage, is only, in fact, an abnormality of nutrition, a substitution of tissue, affecting the contractile and elastic elements in the vascular walls. The vicious impulse given by the nutritive activity at the formation of the individual, is latent for a while, and manifests itself at different epochs. Here, as in all hereditary maladies, as in general paralysis, phthisis or cancer, it has a latent period of an indefinite duration of many years, twenty, forty, sixty years, and even more, and its manifestation, which, happily, is not always fatal, varies according to circumstances favorable or otherwise to the latency of the affection.

M. Dieulafoy summed up his memoir as follows:

1. The disease of cerebral hemorrhage is hereditary.

2. It causes in the same family sometimes apoplexy, sometimes hemiplegia; and the severity of the accidents, the rapidly fatal issue, when it occurs, are only subordinate to the localization of the cerebral lesion; it usually occurs at an advanced age; nevertheless it rather often attacks several members of the same family at various periods of life; it is not rarely observed in a family that the young are attacked before the aged.

The following are the titles of other recent papers on the Pathology of the Nervous System and Mind, and Pathological Anatomy:

LILIENFELD, Two Cases of Apparently Simple Hemicrania, terminating in Death, *Wiener Med. Presse*, No. 50, 1876; WITKOWSKI, The Melancholic Initial Stage of Insanity, *Berliner Klin. Wochensch*, No. 50, 1876; PALLEN,

Some Suggestions with Regard to the Insanities of Females, *Am. Jour. of Obstetrics*, April; MACDONALD, General Paresis, *Am. Jour. of Insanity*, April; ACH. FOVILLE *fils*. The Relations between the Troubles of Motility in General Paralysis and Cortical Lesions in the Fronto-Parietal Convolutions, *Annales Méd. Psych.* January; DAGONET, Moral Insanity and Intellectual Insanity, General Considerations and Classification, *Ibid;* BUZZARD, Sciatica, *Practitioner*, February; DOWSE, Diphtheritic Paralysis, *Ibid*, March; MACGREGOR, A New Form of Paralytic Disease, associated with the presence of a New Species of Liver Parasite, *Glasgow Med. Journal*, January; DELASIAUVE, Classification of Mental Diseases on the Double Basis of Psychology and Clinical Observation, *Le Progres Médical*, March 3.

c.—THERAPEUTICS OF THE NERVOUS SYSTEM AND MIND.

BROMIDE OF POTASSIUM.—Krosz, *Arch. f. exp. Path. u. Pharm.* (Abst. in *Revue des. Sci. Méd.*)

This inaugural dissertation begins with a critical review of previous memoirs. Then Krosz analyzes the symptoms experienced by himself and many of his friends, after the administration of gramme doses of bromide of potassium. It is a new confirmation of already known facts. In effect, he observes a sensation of warmth, and of weight on the stomach, with generally an abundant intestinal flux, cephalalgia, anæsthesia of the pharynx, with decrease of the salivary secretion, a very plain enfeeblement of the intellectual faculties, and finally a slight lowering of the temperature, and slowing of the pulse.

Never any diplopia nor myopia. Occasionally eruptions of acne and of erythema nodosa.

Experiments upon animals confirm the facts already known; it is a sedative, and hypnotic paralysis of the heart that appears to be the cause of death.

Is this action due to the bromine or to the potassium? To solve this problem, Krosz administered to a number of healthy adults, successively, thirteen grammes of chloride of potassium, and seventeen grammes of bromide of sodium. In these doses the bromide of sodium contained as much bromide, and the chloride of potassium as much potassium as the previously employed dose of bromide of potassium. These experiments prove that bromide of potassium combines the action of bromide of sodium and chloride of potassium. They also permit us to determine the part played by each of the elements of the potassic salt.

It is the metal that produces the paralysis of the cardiac muscle, the retardation of the respiration, and the reduction of temperature, the para-

lysis of the nerves and muscles. To the metalloid should be attributed the pharyngeal insensibility, the central paralysis of the bundles that unite the sensitive ganglia to the motor and sensorial ganglia, the exanthema; and finally, the bromine appears also to slow the heart.

As to the therapeutic action of bromide of potassium, it yet remains obscure. We cannot admit that this salt contracts the blood vessels of the brain, since nothing of the kind is observed in the retina to follow its administration. Bromide of potassium acts probably in a direct manner upon the cerebral muscles.

The Influence of Alcoholic Liquids upon the Action of Toxic Substances.—Raphael Dubois, *Thesé de Paris*, 1876. (Abst. by A. Richeraud in *Rev. des Sci. Méd.*)

The author passes in review the different groups of toxic substances, and seeks to establish, by clinical observations and experimentations on animals, how the ingestion of alcohol modifies the effects of the poison, and reciprocally.

The following are the principal results arrived at and stated in this thesis:

The effects of alcohol and of ammonia, separately injected into the cellular tissue, do not neutralize each other; nevertheless, in two experiments the convulsions caused by the ammonia appeared to have been diminished by previous intoxication; but in these cases ammonia was incapable of doing away with the effects of the alcohol (four experiments on guinea pigs).

In digitalis poisoning, alcohol always hastened death, and favored the reduction of the temperature, whether given before or after the digitalis, even in the minute dose of two cubic centimetres. Digitalis did not hinder the production of the symptoms of alcoholism, (six experiments on guinea pigs). In man, on the contrary, in cases of delirium tremens, we may administer large doses of tincture of digitalis without inducing the accidents that follow the use of this drug (18 to 20 grammes).

Eleven experiments upon dogs, guinea pigs, and fowls, showed that atropine did not prevent the production of drunkenness; nevertheless, certain manifestations due to atropine have been observed to disappear under the influence of alcohol, but to give place to the symptoms of alcoholism, or to combine with them.

The effects of alcohol and strychnia do not neutralize each other; the special manifestations of the two substances are produced either simultaneously or alternatively; those of strychnia ordinarily predominating, but not effacing those of alcohol. If the economy is saturated with this liquid for a certain time, the accidents appear less rapidly, with less violence, and the death may be put off (15 experiments on different animals).

Alcohol seems to retard the toxic effects of hydrocyanic acid; it diminishes or prevents the convulsions; but when the dose of prussic acid is large, alcohol has not time to act, (13 experiments).

En resumé the reciprocal action of alcohol and of the poison is generally

nul or very slight, and the physiological effects of the two substances show themselves sometimes simultaneously, sometimes successively.

It is true, nevertheless, that in some cases the action of the poison is less rapid and less energetic, if it is introduced into the stomach or the cellular tissue at the same time with a certain quantity of alcohol.

These researches are doubly interesting; in a therapeutic point of view, alcohol having been considered as an antidote for a large number of poisons; and in a medico-legal point of view, since drunkenness accompanies a certain proportion of criminal or accidental poisoning.

THE TREATMENT OF TETANUS.—Dr. De Renzi, *La Nuova Liguria Medica* (abst. in *La France Médicale*), reports two cases of tetanus observed at the hospital of Panimatone, both treated unsuccessfully by subcutaneous injections of curare.

In the first case the cause of the disease was a fall on the feet from a moderate height. The patient entered the 26th of November, and succumbed the 3d of December.

In the second case the disease was rheumatismal in its origin, a variety much less grave in its nature than the traumatic form. The patient submitted to injections of curare, and died of slow asphyxia three days later. Commenced by giving him the first day 7 grammes of chloral, and then injected 65 milligrammes of curare. The second day the treatment was continued with 5 grammes of chloral and 35 milligrammes of curare. This day the respiration became more precipitate and anxious, and the day following the vital forces were so depressed that it became needful to resort to excitant medicines, such as castorum and turpentine.

In collating these cases with others that have been published, the author concludes that chloral does not justify the hopes to which it has given rise, for the cure of tetanus, and that, on its side, the application of curare in the treatment of this disease does not constitute any real progress.

Dr. Renzi recognized in his patients, and especially in the last, the injurious influence of excitations of any nature whatever, and especially of the excitation produced by light.

The convulsions were less frequent and less intense in the darkness than in full daylight. The punctures made in the hypodermic injections themselves also produced a very injurious effect.

At the autopsy the gray substance of the cord was found rose colored, by reason of an intense hyperæmia.

BLISTERING BEETLES AS A CURE FOR HYDROPHOBIA.—M. de Saulcy, *pere*, laid before a late meeting of the Entomological Society of France, the *debris* of two species of beetles belonging to the meloidæ (*Meloe tuccius* and *Mylabris tenebrosa*), and which had been sent to him from Gabis in Tunis, by M. de Chevarnier, and which constituted the medicine in use by the people of Amerna as a cure for hydrophobia. It is known under the name of *Dernona*, and is mentioned in several Arabian works

on medicine. A portion about the weight of a grain of corn is given to the sufferer. The medical formula directs that it should be taken in some meat soup by the person bitten, between the 21st and 27th day after the bite; if taken before or after these dates it will not effect a cure. The natives of Amerna seem to have great faith in this cure, and preserve the dried beetles as a treasure. It might be worth while to try a series of experiments on the use of vesicating beetles in this terrible malady. But it should not be forgotten that as long ago as 1750, Linnæus, in his dissertation, "De materia medica in Regna Animalis," suggested the employment in such cases of the common blistering beetle, and in 1856, when M. L. Fairmaire laid before the Entomological Society of France a *brochure* by M. Saint Hombourg, on the treatment of hydrophobia by the administration of a species of meloe, many of the members then present, mentioned that this remedy was known for a very long time in Germany. (*Ann. Soc. Ent. de France*) *Nature,* Jan. 18.

ACUTE BROMIZATION.—Dr. Geo. M. Schweig publishes in the *N. Y. Med. Record,* Dec. 30., an interesting communication on the effects of acute bromization. He gives the history of three cases, in which the bromides were used in large doses and for a considerable period, and in which the effects, presumably of these agents, were manifested to a very marked degree. His concluding remarks upon these subjects, leaving out of consideration the points in which his observations agreed with those of other investigators, are as follows:

Cardiac action. Most observers speak of the pulse as becoming "slower and more feeble" while others (Saison, Damourette and Pelvet, Laborde,) deny all influence on the heart. In my first case the pulse became much more frequent and feeble; in the other two it remained unaffected.

Respiration. "The stertor I noticed in case I., I have not seen recorded elsewhere. On the contrary, Clarke speaks of the respiration as without the stertor of opium and alcohol."

"Of the transient abrogation of *hunger* and *thirst,* I have seen nothing mentioned elsewhere.

"The *Conjunctivitis* may have been accidental.

"It is remarkable that no *acne* appeared in any of the cases.

"With respect to the *urine,* my observations are totally at variance with those of all other observers, with the exception perhaps of Rabuteau. In no instance did I find an increase, and in case I., a positive and marked decrease of this excretion. This circumstance would appear to militate against the views of Drs. Amory and E. H. Clarke, of Boston, and Dr. McElroy, of Zanesville, Ohio, 'that the bromides encourage a destructive metamorphosis of tissue.' I am of opinion, rather, that the emaciation accompanying the prolonged bromine influence is due to the retardation of *assimilative* metamorphosis (of food), as shown by Dr. Bartholow, of Cincinnati. Rabuteau claims that there is no change, either quantitive or qualitative in the urinary excretion.

"What I have said under the head of *Sensation* shows (1) the absence

of the (true) anæsthesia that is generally considered so uniform an effect of bromization, and (2) the presence of reflex action, the loss of which is insisted on, more especially by Echeverria.

"My observations on *Motion* exclude the 'general paralysis' claimed by many, and introduce a new phenomenon, viz., *temporary loss of co-ordination.*

The Mind. "What I have observed in this connection is nothing new. It appears probable, however, that the temporary insanity accompanying or following acute bromism takes form in accordance with the temperament and disposition of the individual. Thus in an excitable person we should have delirium or mania; in one prone to dejection, simple, or suicidal melancholia; in shallow minded individuals, idiocy, imbecility, or a merely apathetic condition, etc., etc.

"There is one further conclusion at which I think myself justified in arriving, as a result of my observations; it is, that *the effects of the bromides are due, not to the alkali, but the bromine.* A study of the manner in which the bromides were here administered, and of their attendant effects, will, I believe, furnish ample proof in this direction. So far as my individual judgment goes, both the sodium and calcium bromides are superior to potassium for the purpose of obtaining the effects of bromine."

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ANEMONIN.—Curci, *Lo Sperimentale*, 1876, XXXVIII., No. 7. (Abstr. in *Centralbl. f. d. med. Wissensch.*)

The author experimented with the so-called anemonin (pulsatilla camphor), or with the watery extract of fresh Anemone pulsatilla (the dried plants are inactive). Both substances were injected subcutaneously, the anemonin dissolved in warm glycerine; in cold it decomposes. Both produced the same general effects on the animals subjected to the experiments (frogs, mice, rats). The animals fell first into a sleepy condition and moved only upon external stimulus. Larger doses about 5 mgms. (=.075 gr.) for a rat, and with longer continuance of the poisoning, the stupor increased, the sensibility of the cornea disappeared entirely, so that it could be touched without inducing reflex movements. Reflex movements could be induced by sufficiently severe sensory irritations of any other part of the body. In the extremities, especially the posterior ones were temporarily in tetanic contraction, the extensors, as it appeared, paralyzed, the frequency of the respiration diminished, while the heart's action remained unaltered until death. Electric irritability of the nerves and muscles retained. Locally, anemonin is an irritant. On rabbits the the experiments apparently failed, on account of the slight solubility of anemonin a sufficient quantity could not be injected. It must also be added that the preparation acted also by vascular dilatation; in frogs, at least, the natatory membrane was strongly injected and reddened.

The author thinks that anemonin acts first on the brain, and in larger doses on part of the medulla oblongata, as is indicated by the altered respiration and the described muscular spasms.

Electro-Capillary Currents and their Influence on the Organism.—At the session of the Soc. de Biologie, Jan. 27, (rep. in *Gaz. des Hopitaux*) M. Onimus presented a series of minute apparatuses from the laboratory of M. B·cquerel serving to show the effects of electro-capillary currents. If two solutions of mineral salts are separated by a capillary space, a cracked glass, for example, there is found between them an electrical current causing double decompositions; on one side of the partitions that of the positive pole, there is oxidation, on the other, that of the negative, there is reduction and a deposition of pure metal. Thus, M. Onimus showed glass tubes in which there was a minute fissure, the interior of which were filled with a metallic salt, nitrite of silver or of copper, or chloride of gold, and then they were plunged into a solution of mono-sulphide of sodium. Through the fissure there was produced an electric current, and the silver, gold, or copper were deposited in beautiful crystallizations.

If we separate these same solutions by an endosmotic membrane the same phenomena take place, on one side of the membrane there is a reduction of the salt and on the other oxidation; we may thus obtain metallization of membrane *en papier a dialyse*, or rather a double decomposition with production of an insoluble substance. Separating by a membrane, nitrate of lime and sulphate of soda, we form on one side nitrate of soda, and on the other stalactites of sulphate of lime. M. Onimus presented some vessels in which were these stalactites, and two sheets of parchment paper completely metallized by a deposit of copper on one side.

The interposition of a membrane is therefore sufficient to make liquids thus in contact an electric pile, and to cause oxidations and double decompositions. If we plunge a plate of platinum into the liquid on one side the intensity of the electric currents is considerably increased, as M. Becquerel has recently noted, and it is on this fact that M. Onimus bases himself to explain the pretended special effects of metallo-therapy

In a physical point of view, these electro-capillary currents have a great importance, because they show the existence of electric currents without the necessity of the intervention of the metallic conductor. They appear even to regulate the laws of endosmose and exosmose, since the direction of endosmosis is nearly always the same as that of the electric current. On the other hand, the application of these laws to physiological phenomena is more considerable; these currents, in fact, are formed in every kind of organic liquid, and they explain to us the formation of electricity in certain fishes, in which the special electric apparatus is constituted by very similar membranous partitions formed of an infinite number of cells filled by an albuminous semi-fluid substance.

M. Onimus has observed that albuminoid substances act in the same manner as the membranes, and that they cause the same electro-molecular action. Separating phosphate of soda and nitrate of lime or chloride of calcium by a layer of albumen, we obtain an electro-capillary current, with the formation of phosphate of lime on the one side, and nitrate of chloride of soda on the other. This experiment exhibits well the phenomena that ought to take place in the formation of osseous tissue.

We may therefore affirm that electric currents exist in all the tissues, the phenomena of endosmosis, the chemical reactions, the double decompositions, which constitute, so to speak, the essence of nutrition, are controlled by these currents. The idea emitted by Scoutetten, that mineral baths act by an electric influence, has certainly some foundation, since we cause an electric current to be formed through the skin between the alkaline or sulpuhrous bath and the tissues beneath the epidermis. But the facts discovered by M. Becquerel do more than prove the existence of these currents, they show us at the same time their results, since the subjacent liquids undergo a reduction, they become de-oxidyzed while in the baths where the reaction is different; in the carbonic acid baths, on the other hand, there is oxidation of the sub-cutaneous substances. The electric currents are more intensified by sulphur baths with the mono-sulphide of sodium or potassium than with the sulphurous preparations usually employed. In the medical use of the metallic salts the phenomena should be as follows: if the salt is taken internally it is reduced and deposited as metal on the internal face of the membrane. This is, in fact, what takes place with nitrate of silver since we find metallic silver under the epidermis. If, on the other hand, the salts are applied externally there is a double decomposition, but the metal will remain deposited on the exterior; this is perhaps the reason that these substances are so little absorbable by the skin. For other compounds, such as the iodide of potassium, the following ought to be the order of the phenomena according to the laws of electro-capillary currents; the iodide of potassium should decompose, the iodide penetrate the epidermis, and the potassium form another combination externally. The acid or alkaline condition of the sweat and of the vesicles which contain the salts, ought also to play an important part in the absorption of the medicine and in the action of the medicine absorbed.

We may still form an idea of the valuable indications furnished by these electro-capillary currents, by the fact that there exists between the white of the egg and the yolk an electric current the direction of which proves that, for the latter, the chemical effects are those of reduction and de-oxidation, while the white disappears by a series of oxidations. In general, in the tissues, that is, in that collection of organic electric piles, where nutrition, and in consequence, life itself, is the result of electro-chemical phenomena, the albuminoid substances take the negative electric condition, and it is on them that the oxidations take place.

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METALLOTHERAPIE.—M. Charcot has recently been experimenting, with the aid of M. Burq, on the therapeutic effects of the application of metals to the surface of the body, according to the method employed by the latter, and previously described by him. The patients experimented upon were mostly hysterical anæsthetic cases, and the results of the treatment were rather striking. He reported to the Soc. de Biologie (his remarks printed in the *Gaz. des Hopitaux, Nos.* 8 *et seq.*), that he had apparently induced a return of sensibility in these cases by the application of various

metals, gold, copper, etc., the former, however, being the most generally effective. He made also the somewhat remarkable statement that this treatment was also of effect in the organic anæsthesias dependent upon actual cerebral lesions, and that in two of these cases the sensibility thus restored had been even more enduring than in merely hysterical cases. He, therefore, rejected the opinion expressed by Trousseau, that these applications would afford a good means of diagnosis between cases merely hysterical and those due to organic lesions.

In a discussion which followed, M. Onimus held that, all the effects were those of electric currents generated by the metal or the skin. Anæsthesic conditions were very sensitive to electricity, and the fact that gold, one of the metals that in this situation produced the least deviation of the galvano-meter, did not invalidate this statement. The intensity of the current is not indicated by the effect on the needle which is the result of oxidation.

M. Gubler thought that the effects of the electricity generated in this case was not upon the nerves themselves, but upon the capillaries, inducing an afflux of blood, and secondarily restoring the sensibility.

Whatever may be thought of these experiments, it appears to us that there is a great possibility, at least, that mental influence may have its part in the production of these therapeutic effects. It would certainly be of great interest to have further experiments performed upon cases of actual organic disease.

At the session of the Soc. de Biologie, Feb. 3, M. Charcot recalled the subject, and announced that some of the questions raised by it were already solved, one of which is, do we have to do with electrical phenomena here? which he says no one doubts; and another, to determine the intensity of the current produced by a piece of gold applied on the skin, and to find out whether a pile giving the same intensity would produce the same effects. M. Regnard had been experimenting in this direction, and he asked that he have the floor to explain his experiments.

Before ceasing his remarks, however, M. Charcot stated that there were some points still to be cleared up, and alluded to what M. Burq had called the individual idiosyncrasies in regard to metals. Thus some patients, as he had previously stated, were strongly affected by gold, but not by copper, which induced a much stronger current.

M. Regnard said he had begun a series of experiments to decide the following questions: (1). Do electric currents exist in the cases described by M. Burq? (2). In this case will piles, producing currents equally intense, produce the same effects? (3). What is the cause of the difference in the effects produced by gold, copper and iron on different patients? The first question may be answered affirmatively *a priori*. It will suffice to hold a piece of copper in one hand and a piece of iron in the other to form an intense electric current. A piece of gold laid upon the skin and connected with a galvano meter of 30,000 coils, *i. e.*, one of great sensibility, will cause a deviation of the needle some eight or ten divisions, and a piece of copper will produce much more decided effects. Pure

gold, or platina, produces a very feeble current, with very little effect on the patient.

M. Regnard then related the experiments he had performed on some of M. Charcot's patients, and with the same results with the use of a galvanic pile as with metallic applications. He thought he could justly answer the two first questions named by M. Charcot, as follows: (1). Yes, there are electric currents; and (2). Yes, graduated currents obtained from any pile whatever produce the same effects. The third query remains—Why is a patient sensitive to gold, representing a certain determinate current, and not to copper representing one much more intense? He hoped to be able to answer this question. In a woman, sensitive to gold, he had observed the following results from the use of graduated currents: A current of 2 ° (the figure representing the deviation of the galvanometer needle) was without effect; one of 10 °, equivalent to that caused by the application of gold, recalled the sensibility; one of 45 °, equal to that caused by copper, was ineffective, while one of 90 ° again produced a return of the sensibility. He, therefore, offered as a hypothesis, that the difference of these effects was due to interference of the currents with sensibility.

In the discussion following, M. Charcot stated that he had found the same results to follow in the case of the special as well as of the general sensibility. In cases of hysterical amblyopia, for example, he had found a notable extension of the visual field to follow the application of the metal for a quarter of an hour.

ERYTHROPHLEINE.—The following are the concluding paragraphs of a paper by M. M. Gallois and Hardy, *Bull. gén. de Thérap.*, Aug. 15, 1876, on the bark of the mancona (*Erythrophleine guineaense*).

En resumé erythrophleine, the extract of mancona bark, is a poison to the heart. Two milligrammes injected under the skin of the paw of a frog, paralyze the heart in the space of five to eight minutes, according to the vigor of the animal. Death is not immediate and when it occurs, the nerves and muscles retain their sensibility to the electric current for several hours, according to the season, while the cardiac muscles remains immobile to the same current. The ventricles are arrested in systole, while the auricles are habitually arrested in diastole.

The poison acts more rapidly and in smaller dose when it is put directly in contact with the heart. Its effect is a little more slow when we suppress the circulation; but the intoxication is nevertheless produced by interstitial absorption. Atropia does not hinder the poisoning; curare delays its effects.

In warm-blooded animals erythrophleine causes convulsive jerkings and violent dyspnœa, following disorders of hœmatosis. Death occurs as soon as the cardiac arrest. At the autopsy the ventricles are commonly found soft and full of blood, and in this latter liquid, we discover the presence of the alkaloid by the aid of re-agents; four milligrammes injected under the skin of a guinea pig have caused death at the end of a few hours; three or four centigrammes hypodermically injected in a dog

produce the same result in a longer or shorter time. By the use of graphic apparatuses one is enabled to follow the different phases of the poisoning. Alterations of retardation and accelerations of the pulse are met with. When the pulsations are slowed they possess the greatest fullness; at the later period of the intoxication they are very frequent and weak. But the phenomenon which seems to be most constant, is the augmentation of the arterial tension under the influence of each injection of the toxic alkaloid.

If erythrophleine paralyzes rapidly the cardiac muscle, while the other muscles still preserve for long hours their contractile powers, these latter come none the less under the influence of the agent which should be considered as a poison of the muscular tissue. If the cardiac muscle is the first to die, it is because it receives in a given time a much greater quantity of poisoned blood than the others. The double chloride of erythrophleine and of platina acts as does the alkaloid and causes the arrest of the frog's heart. According to all probabilities, the leaves and grains of *Erythrophleine coumingo* contain an alkaloid, which in its chemical composition is very closely related to, if not identical with, erythrophleine.

It is hard to say just at present whether mancona bark will ever be susceptible to any therapeutic applications. If sternutatories were still employed in medicine it might take its place among them. As to erythrophleine, in giving it in large doses so as to judge of its toxic effects, we have produced serious cardiac disturbances evidenced by sudden alterations of acceleration and retardation of the pulse. If given in very small doses, and during a number of days, it is probable that quite different symptoms would be manifested and possibly it might be assigned a definite position among therapeutic agents.

GALVANISM IN CEREBRASTHENIA.—Dr. G. M. Schweig, *N. Y. Med. Record*, Nov. 4, 1876, recommends as the principal therapeutic measure, besides rest, in cases of cerebral exhaustion or cerebrasthenia, the use of electricity in the form of the electric bath. Both the constant and the faradic currents, he says, are beneficial in this disorder when judiciously applied, and meet all the conditions except that of affording direct nutrition to the brain. For this last, he gives phosphorus and cod liver oil, rejecting most other medication except in cases where there exists great irritability or decided hyperæmia indicating the necessity of special measures to combat these complications.

Inasmuch as the electric bath requires somewhat expensive and complicated appliances for its proper administration, the exclusiveness of Dr. Schweig's method of treatment is to say the least inconvenient to the majority of practitioners who have to deal with this complaint. It may be well to say therefore that less elaborate means have often been found efficient.

THEVETIN.—Theo. Husemann and A. Kœnig *Arch. f. d. Exp, Path.* (Abstr. in *Centralblatt f. d. Med. Wissensch.* No. 40, 1876) gives an account of experiments on the action of a new cardiac poison, a glycoside

derived from the fruit of an East Indian plant, *Thevetia nervifolia*, Juss, and which had been called by its discoverer, Blas, Thevetin. Its formula is $C_{54} H_{84} O_{24}$. By boiling with dilute acids it is resolved into glycose and theveresin. Both theveresin and thevetin were found by Blas to be energetic poisons, very similar in their action. Their symptoms were vomiting, diarrhœa, salivation, muscular tremor and weakness, and dyspnœa. With theveresin, the diarrhœa and salivation were lacking.

Husemann and Kœnig's experiments were on frogs and rabbits. In the latter the ptyalism and diarrhœa failed, but there was marked irregularity and retardation of the pulse, and later, severe dyspnœa and gradually increasing paralysis. Death occurred without convulsions, and the heart's action ceased before respiration. The autopsy generally showed the ventricle in systole, but sometimes in diastole. Irritation of the heart was ineffective, but the muscles still responded though less vigorously than usual. The temperature was unaffected. The symptoms agreed throughout with those of digitalis poisoning; thevetin also increased the effect of excitation of the vagus. Atropine had no effect on the action of the drug. At the place of injection it caused inflammatory action. The view of Blas was that thevetin taken into the stomach was dissolved by the gastric juice into theveresin and glucose, the poisonous action being perhaps due to the former, which when separated had the same effect both qualitatively and quantitatively. In the experiments, the results of which are here given, this was not the case, for the hypodermic method of application of the poison was used in all.

BICARBONATE OF POTASH AS A NERVE SEDATIVE.—Dr. J. A. Erskine Stuart,*Brit. Med. Jour.* Dec. 9, 1876, says, that the bicarbonate of potash is a nerve sedative can easily be demonstrated by any one taking a large dose of it, such as a drachm, in water. A peculiar tingling, numbing sensation is soon felt in the lips and cheeks, and afterwards in various parts of the body, extending gradually downwards to the lower limbs. He has used this drug, combined with the bromide of potassium, in epilepsy, with decided effect. That this sedative action on the nerves has a great deal to do with the successful treatment of rheumatism by the bicarbonate, He has not the slightest doubt. As this is a fact, it is desirable, in treating rheumatic fever with this drug, to give it in sufficiently large doses, such as a drachm dissolved in water, every few hours. That some cases are best treated by means of alkalies there can be no doubt. Others, however, and especially those where there is great hyperpyrexia, are better under the salicine or salicylic acid treatment.

CAUTERIZATION OF THE PHARYNGEAL MUCOUS MEMBRANE IN NEUROSES OF THE HEAD.—The following are the conclusions of a paper read by M. Bitot, professor at the medical school at Bordeaux, before the Soc. de Biologie, Oct. 31, and reported in the *Gaz des Hopitaux* No. 128, 1876.

1. The head is the seat of certain nervous disorders, the **exact locali-**
zation of which is not precisely determined.

2. The cranial portion of the sympathetic has its part in their
pathology.

3. In such cases it is natural to suppose that the superior cervical
ganglion, which forms the principal centre for the cranial sympathetic, is
the point of departure for the trouble.

4. The anatomo-physiological importance of this real brain of the veg-
etative life of the head should be considered by the observer, when ever
he has to deal with a neurosis of this part.

5. He should keep it specially in view when the neurosis is rebel-
lious to ordinary means of cure.

6. The relations of this ganglion to the pharyngeal mucous menbrane,
make of this latter a point of election for acting upon it and its more
distant appendages with certain irritants.

7. Painting this mucous membrane with tincture of iodine, has afforded
remarkable results in functional nervous disorders. It has been found
ineffectual in the disorders consecutive to apoplectic attacks.

8. In many cases complicated with amnesia, the memory has recovered
its former vigor after this treatment.

This method of treatment, we understand, is a revival of that proposed
many years since by Dr. Ducrot, of Marseilles, and which has not been
practiced since 1849 or 1850.

The following are a few of the articles that have recently appeared on
the Therapeutics of the Nervous System and Mind:

GAMGEE, On the Curative Effects of Mild and Continued Counter-
Irritation of the Back, in cases of General Nervous Debility, and in cer-
tain cases of Spinal Irritation, *Practitioner*, February; RINGER, Case of
Acute Mania Treated with Large Doses of Hyoscyamia, Daturine
Atropia, and Ethyl-Atropia, *Ibid*, March; BRIDGES, Two Cases of Violent
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THE
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Original Articles, Selections and Translations.

Art. I.—THE DEVELOPMENT OF THE NERVOUS
TISSUES OF THE HUMAN EMBRYO.

By Dr. H. D. Schmidt, of New Orleans.

Member of the American Neurological Association.

THE following pages embody the results of a series of re-
searches into the development of the nervous tissues of
the human embryo, which were commenced several years ago.
More than four years, indeed, have elapsed, since they were
finished, but not until now have circumstances allowed me to
present them to the medical public in proper form, and illus-
trated with appropriate drawings. It is true, that some refer-
ence was made to these investigations in my article on "The
Structure of the Nervous Tissues" etc., published in the Tran-
sactions of the year 1875, of the American Neurological Asso-
ciation; but only such a brief sketch was given, as the ar-
rangement then in hand required.

I was fortunately able in the e researches to command an
abundant supply of good material. The greater portion of
embryos employed—about eighteen in number—belonged
chiefly to periods of pregnancy, anterior to the fifth month.
In all, they ranged from one exceedingly small specimen, only

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6. mm. in length, through all the intervening stages of fœtal evolutions, up to the mature fœtus born at full term.

The examinations were made, as circumstances required, both from fresh specimens and from others preserved and prepared in a weak solution of chromic acid.

Inasmuch as the nervous tissues are not developed from organic cells, in any true sense of the word, as has been so generally supposed, I have deemed it expedient to introduce the subject with some prefatory remarks in explanation of some terms, which I have employed.

Ever since the discovery of the organic cell in animal tissues, it has been a favored theory, among histologists, that the primary form of all tissues was that of a cell. This theory still almost universally prevails, although in one instance, at least, in the so-called "white fibrous" or connective tissue, it has been sufficiently shown, that it does not originate in a cellular form. But neither the striated muscular, nor the nervous tissues originate from cells, unless we look upon the nuclei, concerned in their origin and development as such. The idea, of what constitutes an organic cell itself, has of late years undergone considerable modification, since it was discovered, that the only essential part of this body, was the protoplasm, and that in many cases the whole cell consisted only of a minute portion of this substance. Thus, the wall and the nucleus are now considered to be non-essential parts of a cell, both being products of the protoplasm. The wall, when present, must manifest itself by a double contour; a cell, presenting only a single contour, is said to possess no wall. And yet, it seems to me, that even in this instance, the density of the protoplasm must be greater at its surface, than in the interior of the mass; for, if there were no limiting elements, the protoplasm of contiguous cells would be liable to become fused. Such a layer may differ so slightly from the whole mass of protoplasm, of which it forms a part, and may be so extremely thin, as to manifest itself only by a delicate single contour, and nevertheless, its density may be sufficient, to prevent its fusion with a neighboring cell.

The nucleus, which, according to the older cell theory, was pre-existent to protoplasm, as well as to the wall, is now known

to originate from and within the protoplasm. There are, however, some exceptions to this rule.

According to what has been said above, a nucleus, surrounded by a portion of protoplasm, however minute or of whatever form, constitutes a perfect cell. But, now I may ask, whether the term "cell" may equally be applied to a body consisting of a more or less oblong nucleus with a minute portion of protoplasm, in the form of granular filaments or appendages, adhering to its opposite poles? Such bodies are met with in the developments of the striated muscular fibres (Fig. 4), and in that of the smaller blood vessels in the human embryo. To extend to these bodies, the term "cell" appears to me to overstretch the true meaning of the word. It was for this reason, that sometime ago, in connection with my researches of the development of the smaller blood vessels in the human embryo, I preferred to call these elements "spindle-shaped bodies," and I shall make use of this term in the following pages. It is true that in the development of the striated muscular fibres the nuclei of these elements become eventually entirely surrounded by the granular fibrillæ (Fig. 6), and the whole body might then be compared to an organic cell. But, as these bodies soon fuse with each other, in order to form another distinct tissue, and thus never perform the functions of a true organic cell, I shall still prefer to apply the above mentioned term to them, especially, as their fibrillæ may be easily separated after the fusion has taken place (Fig. 5). In the development of the nerve fibres, no such spindle-shaped bodies occur, for the nuclei are lying at first free between the granular fibrillæ, only later, when the axis cylinders begin to be formed, they become fused with fibrillæ.

The nervous tissues, during the earlier stages of development, are exceedingly delicate, of a jelly-like consistency, and to a certain degree transparent. It is this delicacy of structure which renders their histological investigation so difficult, as it frequently interferes with their successful removal from the body of the embryo. In very small embryos, therefore, it becomes often necessary to immerse them for a short time in a weak solution of chromic acid, in order to render them sufficiently consistent for dissection. If the solution is not

strong, and the material is only exposed a short time to its action, the changes taking place in the tissues by its action will be so slight, as not to interfere with the investigation. Nevertheless, in order to come to correct conclusions, the examinations made of specimens treated with a solution of chromic acid, should always be compared with such as are made from fresh unaltered material.

Another difficult point in the investigation of embryonic tissues, is to determine the exact age of a human embryo, especially when we obtain our material from unknown sources. To estimate the age of an embryo by its size is inadmissible, as embryos of the same age and stage of development may differ in size as well as adults do. To avoid errors, I shall therefore state in the following pages simply the length of embryos under discussion, measured from the top of the head to the point of the big toe, and allude to their ages only approximately.

The smallest embryo, which I ever had occasion to examine, measured 6 mm. in length, (Fig. 1.) The brain here, consisted only as far as I could ascertain, of the same embryonic elements as the dermal surface and other tissues; viz., of small embryonic nuclei, embedded in a soft, somewhat transparent material, the protoplasm. A differentiation of the nervous tissues, and those surrounding them, had as yet, not begun.

In examining the nervous matter of the brain of embryos of about 9 mm. in length (Fig. 2), we find it to consist of an amorphous matter of a gelatinous consistency, holding, besides innumerable minute granules, a very considerable number of nuclei (Fig. 7, *a*). The greater portion of these nuclei present an oval form, some of them are round, while others have assumed more or less the form of a spindle. They all present a distinct double contour, and contain a number of granules, frequently unequal in size. On some of the nuclei, small appendages in the form of granular filaments, were observed to adhere to their opposite poles. It is difficult to decide, whether these bodies were the primary representations of ganglionic bodies, or whether they were destined to take part in the formation of blood vessels. Besides these elements,

a small number of certain cells, filled with small nuclei were observed. The latter, measuring from about $\frac{1}{600}$ to $\frac{1}{300}$ mm. in diameter, were distinguished by a very dark and heavy contour and by a greenish lustre. In most instances, no cell membrane could be seen, the whole represented a minute ball of nuclei (Fig. 7, *b*). To these cells we shall refer again hereafter. Some other elementary forms, observed in the matter of the brain at this embryonic period, remain to be mentioned. These were a number of long, spindle-shaped opaque bodies, containing one or more rows of granules. Some of these bodies already adhered to each other (Fig. 7, *c*). They undoubtedly represented the first traces of the blood vessels, and judging by the resemblance they bore to those spindle-shaped nuclei, above mentioned, they were very likely identical with them. In my article " On the Development of the Smaller Blood-vessels in the Human Embryo," published in the January number of the *Monthly Microscopical Journal*, 1875, I have described one mode of the formation of blood vessels, taking place at a later period, by the formation of certain spindle-shaped bodies, consisting of granular fibrillæ; the latter starting to be formed from the opposite poles of a pre-existing oval nucleus, by successive adhesion of granules, and similar to the formation of the striated muscular fibre. The observation of these spindle-shaped opaque bodies in the matter of the brain above described, however, seems to indicate that at this early period of embryonic life, blood vessels may also be developed directly from nuclei.

Although several embryos of this early period, either of the same size or slightly larger, fell into my hands, I never succeeded in isolating successfully the spinal marrow for the purpose of an exact examination. The tissues in general at this period are very soft, and their differentiations still too slight to admit of an easy separation. But judging from the examinations made of the spinal marrow of somewhat older embryos, to be described directly, I suppose, that it consists of the same elements as the brain.

In a perfectly fresh and normal embryo, of about 16 mm. in length (Fig. 3), I found the brain to consist of the same ele-

ments as above described, which, however, were now further
advanced in development (Fig. 8, *a*). Thus, the minute
granules were more numerous, so much so, as to form a granu-
lar mass or substance. A considerable portion of them had
already been arranged into rows, forming granular fibrillæ.
The nuclei were also more numerous, they all presented a dis-
tinct double contour, and contained a number of granules, the
largest one of them representing the nucleolus. A few
granular fibrillæ were observed to adhere to some of these
nuclei (Fig. 8, *c*). Now, whether this adhesion took place
accidentally, or whether we behold here the first traces of the
formation of ganglionic bodies, I am not prepared to decide;
especially as I did not observe the same phenomenon in the
spinal marrow of this embryo.

Finally, a considerable number of clear cells, containing
coarse granular nuclei, were met with in the matter of this
brain (Fig. 8, *b* and *d*). The outlines of these cells represent
a delicate, double contour, and being very clear, contain nothing
else besides the nucleus, except in a few instances in which I
observed a few single granules. The nucleus consists only of
a collection of coarse and mostly irregular-shaped granules,
possessing no enveloping layer or wall. In many instances,
some of these granules were observed to separate from the
general mass; while in others they were seen to escape through
the ruptured wall of the cell. The diameter of the clear cells
is not constant, for it ranges from $\frac{5}{400}$ to $\frac{7}{400}$ mm., the
nucleus, however, whether enclosed by a large or small cell,
varies but little in size, its diameter being about $\frac{3}{400}$ mm.
The form of the cell also varies, for, while some of them
appear round or oval, others are irregular in shape (Fig. 8, *d*).
These cells are not only found in the nervous tissue, but also
in all other tissues during the earlier stages of their develop-
ment. Considering their general occurrence and the separa-
tion of the granules of their nuclei, it almost appears as if
they were playing some part in the multiplication of nuclei.

As regards the spinal marrow of this embryo, I succeeded
in isolating a portion of it in the fresh condition. With some
slight exceptions, it consisted of the same elements, as those
of the brain just described. A portion of it still consisted of

free granules, though the arrangement of these minute
granules into rows or fibrillæ had advanced much further than
in the brain, the formation of fibrillæ was already so decided
as to allow them to be separated into bundles in the white
substance.   Fig. 10 represents a small portion of the spinal
marrow in the fresh condition, exhibiting the formation of
fibrillæ.   The bundle, lying across the general mass, repre-
sents most likely a spinal nerve.   The nuclei were also more
numerous; they usually contained one or two nucleoli among
the granules packed in their interior.   They were either dis-
tributed throughout the whole, as in the white substance, and
unconnected with the fibrillæ, or collected into masses as seen
upon the pia mater in Fig. 9, *a*.   Those clear cells with
coarse granular nuclei were also met with here in very consid-
erable numbers (Fig. 9, *b*).   Besides these, however, a number
of these mother cells, packed with small nuclei of a greenish
lustre, alluded to before, were observed in their various stages
of development.   As stated before, some of them represented
only a minute ball of nuclei, while in others a delicate single
contour was observed; in these instances, the nuclei were quite
small.   But there were still others, further advanced in devel-
opment (Fig. 9 *c*), which were distinguished by a double con-
tour.   In some of these, the nuclei had obtained a considerable
size, and even showed a double contour.   The development of
the nuclei, however, seemed to bear no fixed relation to the size
of the cell, neither did the contour of the cells.   The whole
character of these elements shows, that they are mother cells,
engaged in the multiplication of nuclei.   Like those clear
cells, they are also met with in other tissues, at this early
period, especially in the skin.   In the spinal marrow, these
cells were not observed between the fibrillæ of the white
substance, but they were especially observed in the vicinity of
the pia mater.   It was in this locality also, where the nuclei
were met with, collected in masses, which circumstance induced
me to think, that the growth of the spinal marrow in thickness
was most active near this membrane.   In examining a small
piece of pia mater (Fig. 9), it was found to consist of a deli-
cate amorphous membrane, the inner surface of which was
dotted with innumerable granules; a number of these were

arranged in the form of rows. Now, whether these rows of granules represent the first traces of the formation of the fibrillæ of the connective tissue of the membrane, such as I observed in other places, or whether they were fibrillæ of the nervous substance, adhering to the membrane, I must leave undecided.

In reviewing the results obtained from the examination of the nervous tissues of this early period of embryonic life, we can not fail to recognize the first traces of the future nerve fibre in the form of a row of minute granules, which, held together by an intermediate substance, are transformed into nervous fibrillæ, thus representing the fundamental anatomical elements of the axis cylinders. The first traces of the formation of ganglionic bodies, or so called " nerve cells" are seen in those short fibrillæ, adhering to some of the nuclei, as before described.

To satisfactorily demonstrate the development of the peripheral nerves at this early period, is very difficult, on account of the fact that the striated fibres of the muscles, by which the nerves are surrounded, still consist of granular fibrillæ, rendering it impossible to distinguish between the two tissues. But in examining the bundle of nervous matter, lying across the fragment of spinal marrow (Fig. 10), and which we may safely regard as a spinal nerve that during the manipulation was accidentally dragged across the preparation, we find the development of its composing fibrillæ as far advanced, as that of the fibrillæ of the white substance of the spinal marrow. No traces of the formation of blood vessels were discovered in the spinal marrow at this period.

A spinal ganglion, which I succeeded in separating in the embryo under discussion, I found to consist, like the brain and spinal marrow, of a great number of nuclei, imbedded in the mass of granules, of which a great number were already arranged in rows, representing granular fibrillæ; even some small bundles of them were observed.

Let us now direct our attention to embryos, about nine weeks old, measuring from $5 \frac{6}{10}$ ct.m. to $5 \frac{9}{10}$ ct.m., from the top of the head to the point of the toes, and of which I had occasion to examine some fine specimens thoroughly. In com-

paring the nervous system of this period with that of the small embryo last discussed, we find that during the intervening time, its morphological development has been proportionately greater than its histological. In embryos of this period the head is still large in proportion to the whole body. The eyes and nose are formed; the mouth is open, and its cavity quite well developed. The extremities are formed, but the fingers and toes are to a certain degree still connected with each other. The lungs, heart and larger blood vessels are distinctly formed, also the liver, spleen, intestines, etc. The farthest developed of all is the muscular system, for the muscles not only possess their individual sheaths, but they are, moreover, already provided with blood vessels. From the advanced development of the heart and the blood vessels, as well as from the quantity of mature colored blood corpuscles, it may be presumed, that the circulation of the blood is carried on pretty completely. In opening the cranium and spinal canal, the brain and the spinal marrow with their membranes are found to be morphologically much farther developed, than in the embryo last discussed. The membranes, though still passing through their phase of histological development, are nevertheless, formed in all their details. Thus, the pia mater, delicate and transparent as it still is, not only embraces the whole cerebro-spinal axis, as in later periods, but also extends in the form of the neurilemma over the peripheral nerves and their ramifications; and notwithstanding its delicacy of structure, it can now be separated under water from the nervous tissues without difficulty, offering the best opportunity for the study of the development of its connective tissue and blood vessels.

The ganglia of the sympathetic nerve are also distinctly formed. They are seen extending along each side of the spinal column, forming the gangliated cords. Their size, however, in comparing it with that of the perfectly developed ganglia in the adult, seems rather larger in proportion, for which reason they also appear to be closer to each other.

The nervous substance of the brain and spinal marrow of this period, presents a white semi-transparent appearance, similar to the "Milk glass" of certain lamp shades. It still consists principally of those primary anatomical elements, already de-

scribed. The histological development of these elements, has however not kept equal pace with the development in volume and form of the whole nervous apparatus. Not only has the brain and and spinal marrow assumed a more decided form, but the peripheral nerves are also distinctly formed and may even be separated. In the spinal marrow, the gray substance is softer than the white. The formation of ganglionic bodies is still confined to a small number of nuclei, to which a few short granular fibrillæ are seen to adhere. The rest, being quite numerous, are distributed throughout the granular substance, the granules of the greater part of which are now arranged into regular rows. The first traces of the formation of blood vessels are now observed in the form of oval nuclei, with small granular appendages, adhering to their opposite poles, and giving to the whole body the form of a spindle. In the white substance also, the greater portion of the granules are found to be arranged into parallel rows, while the rest are distributed between these primitive fibrillæ, to furnish, in the course of the developement, material for the formation of others. Those subdivisions of the white substance, as seen in the fully developed spinal marrow, into larger or smaller bundles, and effected by partition-like processes derived from the neuroglia, may be observed to commence in the embryonic spinal marrow of this period.

An interesting fact to be noticed here is, that while in the spinal marrow the granules are arranged, as before mentioned, into regular rows, which, however, may be easily deranged again by manipulation, in the peripheral nerves, as for instance in the brachial plexus, they are observed to be already fused with each other into perfect fibrillæ. This observation, with others to be stated hereafter, shows, that the development of the nervous tissue is farther advanced at the periphery, than in the centre.

In the brain the histological development has not advanced further than in the spinal marrow. On the contrary, the granular fibrillæ are not even so distinctly formed as in the latter. The formation of ganglionic bodies is still limited to those few nuclei with granular filaments adhering, as mentioned before.

The pia mater extends from the brain and spinal marrow,

over the peripheral nerves in the form of a single sheath. The whole nerve consists, therefore, like the nerve of an insect, only of a bundle of granular fibrillæ, which in pursuing a slightly wave-like course, are placed parallel to each other, and surrounded by their sheath, the neurilemma. A number of oval nuclei, varying in diameter, are distributed and imbedded between the fibrillæ. The blood vessels of the pia mater are very numerous and much further advanced in development than those of the nervous substance. When the pia mater is illuminated with oblique light, very fine fibrillæ, lying parallel to each other, may be observed. Its inner surface is dotted with numerous nuclei of different diameters, similar to those of the white substance of the spinal marrow, but there is also a small number of others, which are spindle-shaped and probably belong to the connective tissue.

It deserves to be mentioned here, that those large mother cells, packed with larger and smaller nuclei of a greenish lustre, and found, as described above, in the substance of the spinal marrow near the pia mater of a former period, are no longer met with. The multiplication of nuclei by the endogenous mode, has ceased; another mode, that by gemmation or budding, will henceforth be observed.

In the sympathetic ganglia of this period, the formation of their peculiar ganglionic bodies has distinctly commenced. Although the greater portion of their nervous substance still consists of the same elementary forms, as found in the brain and spinal marrow, there are nevertheless a considerable number of nuclei now met with, surrounded by a mass of granules, from which the rudimentary processes are seen to proceed in the form of granular filaments. Now, whether these filaments have already been connected with the granular nervous fibrillæ, or whether this connection takes place subsequently, will be difficult to determine. I am inclined to think that it has existed from the beginning of the formation of the ganglionic body.

The sympathetic nerves still consist, like those of the cerebro-spinal axis, of bundles of granular fibrillæ, each surrounded by a sheath. A number of oval nuclei are imbedded between the fibrillæ. The sheath represents the neurilemma

and is derived from the membranous capsule, surrounding the whole sympathetic ganglion.

In the spinal ganglia of this period, the development of the ganglionic bodies has still farther advanced, for they not only present all the characteristics of those of the thoracic ganglia, above described, but they have besides already assumed a more definite round or oval form.

In reviewing the above observations, regarding the development of the nervous tissues, our attention cannot fail to be directed to quite an interesting fact, namely, that the development does not keep equal pace in all localities, but may be more advanced in one than in the other. Thus, while in the brain and spinal marrow we have so far only met traces of the formation of ganglionic bodies, we find them already formed in the sympathetic thoracic ganglia. And again we find the primary fibrillæ of the nerve fibres in the peripheral nerves farther advanced in development, than in the spinal marrow or the brain. We shall refer again to this fact.

After the embryonic period, thus far treated, in this paper, the nervous tissues commence to assume very gradually their later complicated structure, for which reason, we shall prefer to treat each tissue individually.

In commencing with the nerves of the cerebro-spinal axis, in embryos of 15 ctm. to 15 $\frac{2}{10}$ ctm. in length,—about three and a half months old,—we find that each of them no longer consists of only a single bundle of nervous fibrillæ, enclosed in the rudimentary neurilemma forming a single sheath: but that on the contrary, the fibrillæ have subdivided into a number of bundles, each of which is surrounded by its own individual sheath of delicate connective tissue, which is derived from the neurilemma surrounding the whole nerve. Besides this, the fibrillæ of each bundle have also commenced to separate themselves, either singly, or by twos, sometimes even by threes. The interspaces formed by this separation, are filled up with numerous nuclei of different diameters, and with granules. From the latter, new fibrillæ are formed during the course of the development of the nerve. The nuclei, of course, are identical with those already described, between the granular fibrillæ of a previous period.

Somewhat later, in embryos, about 18 $\frac{7}{10}$ ctm. long, and

four months old, the fibrillæ forming the individual groups of
two or three, have approached each other more closely and ad-
here to each other. This phenomenon signifies the first step
to the formation of axis cylinders. The interspaces formed,
in consequence of this mutual approach of the fibrillæ, have
been rendered larger and more distinct. A number of these
primitive axis cylinders, as already indicated before, consists
of only one fibrilla. At the same time it is observed, that the
smaller nuclei, lying in the interspaces, appear to become at-
tached to the axis cylinders, in order to fuse with them, as we
shall see directly. The rudimentary axis cylinders are difficult
to separate from each other, which may be attributed to the ab-
sence of the sheath, by which they will somewhat later be
surrounded; and also, because they are imbedded together with
the nuclei and granules, in an amorphous matrix of proto-
plasm, which holds them together.

Again, somewhat later, in the fœtus of four and a half
months, about 20 to 22 $\frac{5}{10}$ ct.m. in length, a number of prim-
itive axis cylinders are observed to be surrounded each by a
sheath of its own, which, when fully developed, is distinguish-
able by a delicate double contour (Fig. 12). We must, however,
remember, that all the axis cylinders of one bundle, do not
keep equal pace in their development; on the contrary, while
some of them may be already surrounded by their sheath,
others may still be seen to lie bare, or the formation of the
sheath may only be indicated by a delicate single contour.
The nuclei, previously adhering to the axis cylinders, have
now begun to fuse with them; they are also observed in differ-
ent degrees of fusion.

The next phenomenon in the development of the double
contoured nerve fibre is the appearance of the nerve medulla,
surrounding the axis cylinder, and manifesting itself by an
extremely delicate single contour (Fig. 13, *b*). In some in-
stances, however, traces of the formation of the tubular mem-
brane, or external sheath of this nerve fibre, may also be
observed in the appearance of a second delicate contour, inside
of the first. A few single granules are observed in the interior
of the nerve fibre (Fig. 13, *c*). The diameter of the axis
cylinder of this stage of development is not the same at all

points.   In the vicinity of the nuclei fusing with the axis cylinders, it is usually greatest.   The larger nuclei, which were previously observed to remain lying free and unconnected with the axis cylinders, are now found to rest upon the tubular membrane of the nerve fibre.

Finally, about a month later, the double contoured nerve fibre is met with again, marked by all its peculiar characteristics.   The tubular membrane, together with the fibrillar layer of the nerve medulla, manifest themselves now distinctly by their characteristic double contour.   The delicate, smooth fibrillæ of the fibrillar layer of the nerve medulla may now be seen in their wave-like course, crossing each other in the interior of the nerve fibre, or protruding in the form of loops from its torn ends, such as I have formerly described them[*] in the fully developed nerve fibre.   The axis cylinder may also be seen in many instances protruding from the open end of the nerve fibre.   It is only in the diameter, that the double contoured nerve fibre of this period of fœtal life still differs from that of adult life, it being still smaller.

The first distinct traces of the formation of ganglionic bodies in the spinal marrow, I observed in embryos measuring from $7\frac{5}{10}$ ct.m. to $8\frac{1}{10}$ ct.m. in length, and about ten weeks old.   The formation of these bodies takes place by a gradual accumulation of granules around one of the larger nuclei; from the granular mass fine processes are seen to arise, establishing a communication with the still granular nervous fibrillæ of the future axis cylinder.   The nucleus has as yet undergone no visible change in its character, for it still represents a vesicle filled with granules, of which one or two of them, larger than the rest, may be regarded as nucleoli.   By the continued attraction of fresh granules, the ganglionic body enlarges in circumference, and its processes in thickness and extension.

Finally, in the spinal marrow of the fœtus, about four and a half months old, a number of ganglionic bodies are observed, bordered by a delicate single contour, which in some instances even extends over their processes, and indicates the formation of the delicate sheath, enveloping the whole body, and being

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[*]Monthly Microscop. Journal, May, 1874.

continnous with the sheath of the axis cylinders arising from the processes (Figs. 14, 15, and 17). At the same time, however, many of the ganglionic bodies have not yet passed the first stage of their development. But in the nucleus, also, a change has taken place. Its nucleolus, namely, having by this time gained considerably in its dimensions, shows a granule of a bright lustre in its centre.

Still a month later, in the fœtus of about five and a half to six months of age, the ganglionic bodies with their processes, besides having gained in volume, are now completely surrounded by their sheaths (Fig. 18). The interior of the nucleus is filled with small granules of the same nature as those of the mass of the ganglionic body; its nucleolus has enlarged, and is seen more distinctly; the granule in the centre has become brighter. Those accumulations of dark bordered pigment granules in the vicinity of the nucleus so characteristic of the ganglionic bodies of the nervous system of the adult, also have now made their appearance. Thus, all parts, characteristic of the ganglionic body, are now present, and, excepting its further development in volume, it appears but for one element to be near its completion. The missing part belongs to the nucleolus. In the fully developed ganglionic body of the spinal marrow, the nucleolus is distinguished by a distinct double contour; its interior, besides being filled by small granules, contains one or two clear bodies of a reddish lustre. One of these, being always present, is also distinguished by a double contour, and shows, besides this, a dark granule in its centre. The fully developed nucleolus of the ganglionic body, therefore, is quite a complex body, for which reason, on a former occasion, I assigned to it the character of a nucleus, and regarded the so-called nucleus as the true nerve cell. The nucleolus of the ganglionic bodies of the spinal marrow of the fœtus under discussion, on the contrary, shows only a single contour, and the sole object it contains is the bright granule, which, however, is not as yet distinguished by a double contour, nor does it contain a dark granule in its centre.

In consideration of the fact, therefore, that the nucleolus is that part of the ganglionic body, which, last of all others,

attains its perfection, we cannot but suspect, that its office in
the function of the ganglionic body must be an important one.

The ganglionic bodies of the cortical layer of the brain ap-
pear to attain their full development somewhat later than
those of the spinal marrow, or even those of the medulla ob-
longata, or the larger cerebral ganglia, the corpora striata, etc.
In the earlier periods of embryonic life, we have met only
their first traces in the form of a few nuclei, to which some
short granular fibrillæ were adhering. The first distinct traces
I observed in embryos, measuring about 15 or $17\frac{5}{10}$ centi-
metres in length. The brain, at this period, from the third to
the fourth month, still consists, in its greater part, of the
granular substance and those nuclei already described before.
A considerable portion of the granules, of course, are now
arranged into rows. The force, however, which binds them
to each other is still very feeble, so that they become very
easily deranged and disconnected, even under the most deli-
cate manipulation. Amidst these elements, a small number
of ganglionic bodies are observed in different stages of devel-
opment. Though, in those specimens, furthest developed, the
granular mass completely embraces the nucleus; the processes
proceeding from it are still short; they are usually from three
to four in number (Fig. 16). As has already been remarked,
in connection with the spinal marrow, those mother-cells
filled with nuclei of a greenish lustre, and found in the matter
of the brain and spinal marrow of an earlier embryonic pe-
riod (Figs. 7 and 9), and serving for the multiplication of
nuclei, are seen no more. The multiplication of these bodies
is now accomplished by another mode, namely, by the process
of budding, or germination. Thus, instead of those large
mother-cells, we observe a number of nuclei of a greenish tint,
provided with small, clear vesicles, which become ultimately
detached, in order to be transformed into nuclei themselves
(Figs. 16 and 24). A number of nuclei are also observed bear-
ing one or more spherical depressions, the remaining traces of
former vesicles. This mode of multiplication of nuclei, I
described some time ago, in connection with the development
of the smaller blood vessels in the human embryo.*

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* Monthly Microscop. Journal, January, 1875.

Let us pass over now to a period of fœtal life, when the brain has attained such a degree of development, as to possess sufficient consistency to allow, after having been hardened in a weak solution of chromic acid, of making transparent sections, the only mode of preparation, which admits of an exact examination. In examining such a section of the cortical layer of the cerebrum of a fœtus, seven months old, we find the nervous elements, composing this layer, already arranged in regular order (Fig. 19). The ganglionic bodies have now assumed their later pyramidal form, their long-pointed processes can be distinctly seen stretching toward the surface of brain, also their lateral and local processes, pursuing their course laterally or downward toward the white substance. In some instances, even, I have observed a primitive axis cylinder in the form of granular fibrillæ, arising from one of the latter processes. A considerable number of granular fibrillæ are seen running vertically from the surface toward the white substance. They represent the future double-contoured nerve fibres. Numerous free nuclei, distributed between the ganglionic bodies, are seen imbedded in the granular substance. But the most interesting of all these parts, composing the cortical layer, is the fine terminal network of nervous fibrillæ, distinctly seen to extend throughout this substance. We see, therefore, that although these anatomical elements of the cortical layer of the cerebrum are not fully developed, they are, nevertheless, already arranged in perfect order, and we must not be surprised to see a fœtus, born at the end of the seventh month, live without detriment to the development of his mental faculties.

In the newly-born child, finally, we find the mental apparatus almost fully developed (Fig. 20). The ganglionic bodies have nearly obtained their full growth and structure. In some of them, the characteristic collections of dark-bordered pigment granules have also made their appearance. The rudimentary axis cylinder, which, in the fœtal brain of seven months, we observed arising from the basal processes of the ganglionic bodies, and pursuing their course toward the white substance, have now been developed into double contoured nerve-fibres. Nevertheless, some of the granular fibril-

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læ are still noticed among these fully developed fibres; and I doubt not, but that the development of new axis cylinders continues as long as new ganglionic bodies are formed. In fact, it would be an interesting question to solve, until what time of life this last process goes on. Among the free nuclei, embedded in the granular substance of the cortical layer of the brain under discussion, I have several times observed two nuclei, overlaping each other, suggesting the question, whether they do not owe their origin to the division of one nucleus. Finally, the terminal net work of nervous fibrillæ is now as distinctly seen as in the adult brain; and, moreover, the fine branches of the long pointed process are observed to lose themselves in its meshes.

We have now arrived at the most difficult part of our subject, the study of the development of the sympathetic ganglionic bodies. The structure of these bodies has always been one of the most obscure subjects in histology, and hence the discrepancies, which have arisen concerning it, in the views and statements of different investigators. The chief difficulty in the examination concerns the characteristic capsule, which incloses the ganglionic body of the sympathetic nervous system and its relation to, and its connection with this body. Therefore, before giving a description of their development, I deem it necessary to recall to mind the structure of the fully developed sympathetic ganglionic body, as I have described it. According to my investigation,[*] the sympathetic ganglionic body, which is generally round in form, consists of a large nucleus, surrounded by a mass of granules. From this mass, a number of larger and smaller processes are seen to arise; the whole enclosed in that peculiar membraneous capsule. The *larger* processes, from one to four in number, after arising from the body, pierce the capsule, and disappear in the form of naked axis cylinders, at a distance of about $\frac{6}{100}$ mm. or more, among the neighboring bundles of sympathetic nerve fibres. What becomes of them is not certainly known, but I have reason to believe, that they are finally transformed into dark-bordered nerve fibres. The *smaller* processes, arising from the body, are more numerous than the

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[*] Transactions of the American Neurol. Assoc'n, 1865, p. 107.

former, and consist mostly of only two or even one fibrilla. After a short course alongside of the body, they enter the capsule at its inner surface, and form, by means of ramification and reciprocal connection, a *network* extending throughout this membrane; the interspaces of the network are filled up by small granules. The capsule of the sympathetic ganglionic body, therefore, represents a *complicated membraniform, nervous structure*, derived from and connected with the body, which it encloses. On the surface of the capsule, formed in this manner, a number of fine fibrillae arise from the network, a part of which pass, in the form of a finely reticulated plexus, over into the capsules of neighboring ganglionic bodies, and thus establish a reciprocal communication; the rest surround the axis cylinders arising from the larger processes and having pierced the capsule, and running in the same direction with these, unite among themselves to form finally the so-called *sympathetic nerve fibres.* Scattered over the inner as well as the outer surface of the capsule, a considerable number of round or oval nuclei are observed. They are especially numerous in the reticulated fibrillous plexus, connecting the ganglionic bodies with each other, whence they extend, while assuming a more oblong form, between the sympathetic nerve fibres.

As we have already seen in the preceding pages, there exists no essential difference in the formation of the primary ganglionic bodies of the cerebro-spinal axis and those of the sympathetic ganglia. They are all formed by an aggregation of granules around a pre-existing nucleus; from this granular mass, the processes arise, to be subsequently connected with the primitive nervous fibrillae. While, however, the cerebro-spinal ganglionic bodies attain their full development in this manner, those of the sympathetic system, must deviate from it, in order to form their characteristic capsule.

In embryos of $5 \frac{6}{10}$ to $5 \frac{9}{10}$ ctm. in length we find the ganglionic bodies of the thoracic ganglia, as already mentioned, to consist of a nucleus, only surrounded by one or two layers of granules from which a few filamentous processes are seen to arise. These primary ganglionic bodies are embedded in the general mass of granules and nuclei, of which at this time the

greater part of the ganglion still consists. A considerable portion of the granules, however, are arranged into rows, representing primitive nervous fibrillæ. In fact, this was already the case to some extent, as will be remembered, in the spinal ganglion of an embryo, only 16 mm. in length. But the adhesion of the granules is still too feeble, to prevent them from being deranged by the most delicate manipulation. It is for this reason, that the filamentous processes are always found to be torn (Fig 21). In the *spinal* ganglia of the same embryo, however, the development of their ganglionic bodies is found to be considerably in advance of those of the thoracic ganglia. Not only have these bodies assumed a more definite form and gained in size; but they are already attached to each other by filamentous processes, forming small groups, as later in life, when they are fully developed (Fig. 22). Other bundles of fibrillæ are seen to arrive from them, uniting to form the sympathetic nerve fibres. It will be noticed, however, that there are, as yet, no nuclei attached to any of the nervous fibrillæ. In my previous descriptions of the structure of the sympathetic ganglionic bodies, I said, that I suspected their larger axis cylinder processes would be ultimately transformed into a dark-bordered nerve fibre. But in taking into consideration the reciprocal connection of these bodies by their larger processes, as seen in Fig 22, it may be possible also, that by means of these processes, the destination of which is still unknown, a reciprocal connection is established between the ganglionic bodies of one and the same group.

Now, in examining a small group of ganglionic bodies of the spinal ganglion of an embryo, 10 $\frac{9}{10}$ ctm. in length, and about eleven to twelve weeks old, we behold the first traces of the formation of the capsule. It will be noticed here (Fig. 23), that some of the filamentous processes have become attached to the nuclei, lying between the ganglionic bodies. In the preparation, a number of processes have been torn from the bodies by the manipulation.

A few weeks later, in the embryo of about four months, and 17 $\frac{5}{10}$ ctm. in length, the formation of the capsule is seen more distinctly. The fine filamentous branches arising from a number of the processes of the ganglionic bodies, have

now commenced to unite with each other, in order to form the filamentous network of the capsule. At the same time, they are observed to adhere to the numerous nuclei, surrounding the ganglionic bodies.

Fig. 24, *a*, represents a ganglionic body of a thoracic ganglion of this embryo; it illustrates the formation of the capsule better, than it can be described, for it shows distinctly the ramifications of the processes, the communication of the filaments in forming the meshes of the network, and their attachment to the nuclei. In *b*, of the same figure, we behold some of the nuclei, entangled in the filamentous mass found between the ganglionic bodies.

In Fig. 25, which represents a ganglionic body of a spinal ganglion of the same embryo, the formation of the network of the capsule is still more distinctly seen.

In studying the formation of the capsules of the sympathetic ganglionic bodies on the preparations just described, it must be remembered that they have been made by separating the component anatomical parts of minute portions of a ganglion, with finely pointed needles, and that it is impossible to avoid tearing and displacing some of these parts. It is for this reason, that we do not see the anastomosing nervous filaments with their nuclei surrounding the ganglionic body while forming the rudimentary capsule, but observe them only attached to the processes of the body. In the foetus of six and a half months of age, however, the first cervical sympathetic ganglion has attained a sufficient size to allow thin transparent sections to be made, after it has been hardened in a weak solution of chromic acid.

Such a section we find represented in Figs. 26 and 27. In examining Fig. 26, we observe three capsules, cut at one and the same level. The ganglionic bodies, being exposed by the section, are seen in the interior of the capsules. While in the fresh specimen, however, the ganglionic body nearly fills the interior of the capsule, we find in this instance the granular mass of the body considerably contracted by the action of the chromic acid. The processes which, in consequence of this contraction, have to a certain degree been put on the stretch, appear somewhat larger; they are subdividing, and the branches

resulting from this subdivision are seen ramifying throughout
the wall of the capsule, forming the network. From the outer
surface of the different capsules, small bundles of nervous
fibrillæ are seen to arise, joining with each other in their course
in order to form larger nerve bundles. This preparation also
shows how the component anatomical elements of adjoining
capsules run into each other. The nuclei are seen dispersed in
the capsules and in the reticulated plexus, arising from these.
In examining this preparation, it must be remembered, that a
number of the smaller processes have been torn off by the knife
in making the section, and furthermore, that the structure of
the capsule has, as yet, not attained its perfection.

In Fig. 27, which was copied from the same section, we see
the outer surface of a capsule. The network or plexus, formed
by the nervous fibrillæ, derived from the ramifications of the
smaller processes of the ganglionic body, is here very dis-
tinctly exhibited, and, moreover, small bundles of fibrillæ may
be observed arising from it, to finally join a neighboring bun-
dle of nerve fibres.

In making a final review of the facts elicited by these inves-
tigations into the development of the nervous tissues, de-
scribed in the preceeding pages, we first notice, that these tis-
sues are not developed in the true sense of the word from pre-
existing cells, as was formerly supposed, but, on the contrary,
are developed by the aggregation of pre-existing minute gran-
ules, which either collect around a pre-existing nucleus, as in
the formation of the ganglionic bodies; or arrange themselves
into rows, as in the formation of the fibrillæ of the axis cylin-
ders.

The principal anatomical elements, then, taking part in the
formation of the nervous tissues, are a mass of pre-existing
minute granules with a special material binding them to each
other,—the intermediate substance, as I have occasionally
called it,—and a large number of pre-existing nuclei. The
granules, together with the intermediate substance, may be
regarded as the protoplasm of these primitive formations.

Nevertheless, in more minutely comparing the development
of an organic cell with that of the nervous tissues, we cannot
fail to recognize a certain analogy between the two processes.

This exists in the formation of the *wall* in the one instance, and the formation of the *sheath* of the axis cylinder, as well as that of the ganglionic body in the other instance. As the wall of the cell, namely, is formed by a condensation of the protoplasm, taking place at its surface, so, I believe, the sheath of the axis cylinder and that of the ganglionic body, are formed by a condensation of the intermediate substance, which not only connects the individual granules of the nervous fibrillæ with each other, but also surrounds the fibrillæ themselves. Thus the ganglionic bodies of the nervous system may still be regarded in the light of organic cells, which, multipolar in form, would send the ramifications of their processes, in the form of nervous fibrillæ, to the various peripheral organs.

The view has been held, and perhaps still is, by some anatomists, that the great nerve centres, the brain and spinal marrow, were the first parts of the nervous system formed, and that the nerves, regarded as simple processes, or prolongations arising from these centres, were growing outwardly into the tissues, finally to arrive at the periphery. In the preceding pages it has been shown, that this is by no means the case, but, on the contrary, that the peripheral nerve fibres are sooner developed than those of the centre; and, furthermore, that the nerve fibres arrive at their full development sooner, than the ganglionic bodies. As regards the tissues of the different parts of the nervous system, we find that they attain their full development first in the sympathetic ganglia, especially in the spinal; next in the spinal marrow, and last in the brain. This order of development might be expected, for it truly corresponds with the different grades of functions, namely, the *vegetable, animal* and *mental.*

EXPLANATION OF PLATES.

Fig. 1.—Human embryo, 6 mm. in length ; nat. size.

Fig. 2.—Human embryo, 9 mm. in length, nat. size; *a*, anterior, *b*, lateral and *c*, posterior view.

Fig. 3.—Human embryo of 16 mm. in length.

Fig. 4.—Elementary forms of striated muscular fibres, from the upper extremities of the embryo, represented in Fig. 3.

Fig. 5.—Primary muscular fibre from the tongue of the same embryo, with its fibrillæ separated from each other.

Fig. 6.—Spindle shaped bodies (cells) of the primary muscular fibre of the auricles of the heart of the same embryo.

Fig. 7.—Primary anatomical elements of the brain of embryo, Fig. 2; *a*, nuclei and granules; *b*, mother cells, containing a brood of nuclei; *c*, primary elements of blood vessels.

Fig. 8.—Nervous matter of the brain of embryo, Fig. 3; *a*, white substance; *b*, grey substance; *c*, nuclei with nervous filaments adhering to them; *d*, different forms of clear mother cells, described in the text.

Fig. 9.—*a*, minute portion of the pia mater of embryo, Fig. 3, with nervous matter adhering; *b*, groups of clear mother cells; *c*, large mother cells, containing a brood of nuclei of a greenish lustre.

Fig. 10.—Minute portion of spinal marrow from embryo, Fig. 3, showing the formation of nervous fibrillæ.

Fig. 11.—Bundle of nervous fibrillæ from the brachial plexus of an embryo, about nine weeks old.

Fig. 12.—Nervous bundle from the root of a spinal nerve of an embryo, three and a half months old; it illustrates the formation of the axis cylinders.

Fig. 13.—Nerve fibres from the brachial plexus of the same embryo, showing the formation of the nerve medulla and the tubular membrane; *a*, primitive axis cylinder; *b*, nerve fibre with single contour; *c*, nerve fibre with double contour.

Fig. 14.—Minute portion of gray substance from the upper part of the spinal marrow of an embryo, three and a half months old, showing the formation of ganglionic bodies.

Fig. 15.—Ganglionic body from the dorsal region of the spinal marrow of the same embryo.

Fig. 16.—Nervous matter from the brain of a fœtus, four months old, showing the formation of ganglionic bodies, and the process of multiplication of nuclei by germination.

Fig. 17.—Communicating ganglionic bodies from the spinal marrow of the same fœtus.

Fig. 18.—Minute portion of spinal marrow of a fœtus four and a half months old.

Fig. 19.—Thin transparent section of the cortical layer of the cerebrum of a fœtus seven months old.

Fig. 20.—Thin section of the same layer of the cerebrum of a fœtus at full term.

Fig. 21.—Ganglionic bodies from a thoracic sympathetic ganglion of an embryo, about nine weeks old.

Fig. 22.—Group of sympathetic ganglionic bodies from a spinal ganglion of the same embryo.

Fig. 23.—Group of sympathetic ganglionic bodies from spinal ganglion of an embryo, from eleven to twelve weeks old.

Fig. 24.—*a*, sympathetic ganglionic body from the thoracic ganglion of an embryo of four months, showing the formation of its capsule; *b*, nuclei, entangled in nervous fibrillæ during the formation of the capsule.

Fig. 25.—Sympathetic ganglionic body from a spinal ganglion of the same embryo, also illustrating the formation of the capsule.

Table I.

Table III.
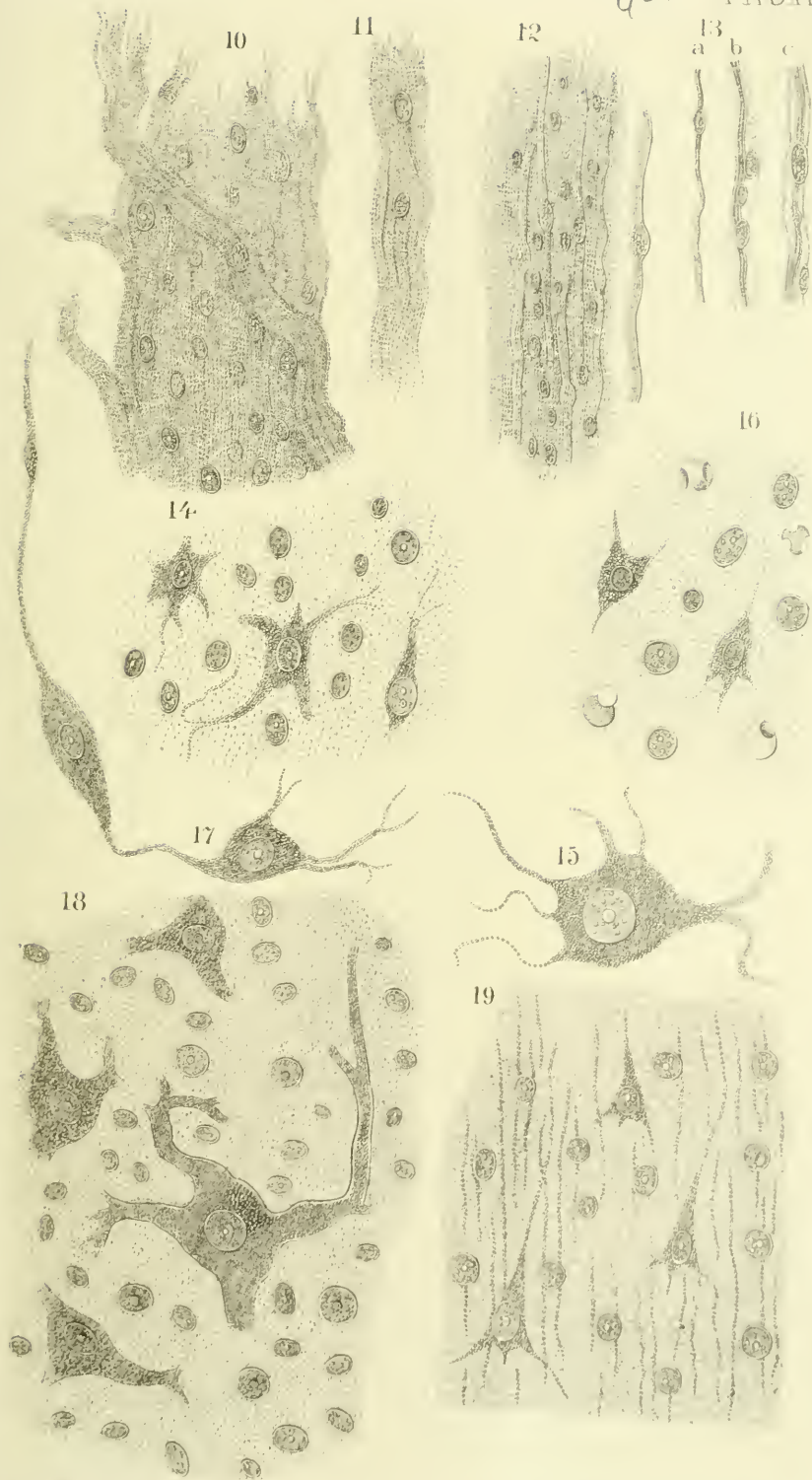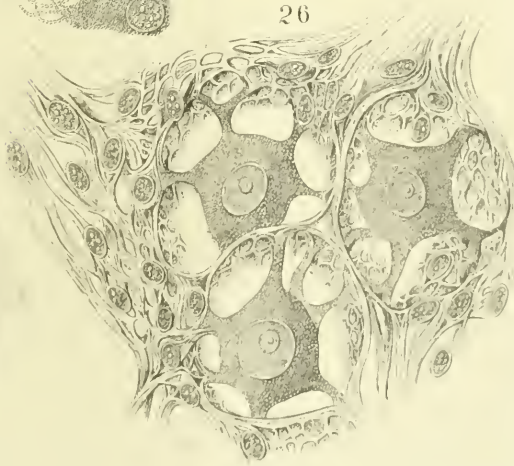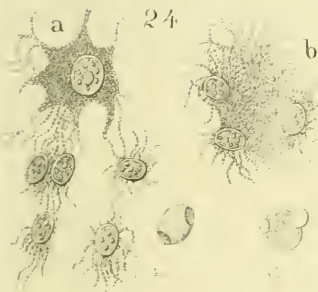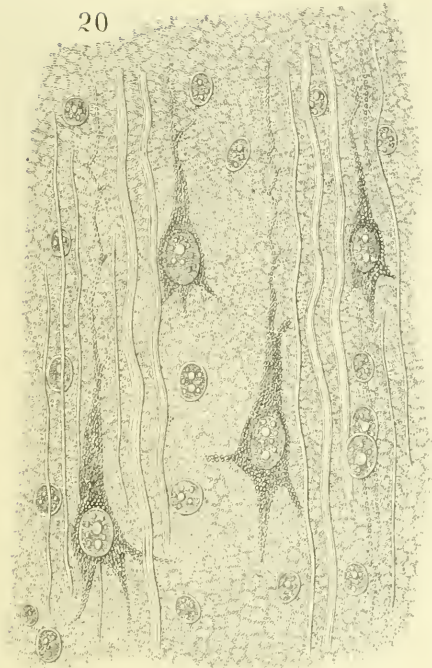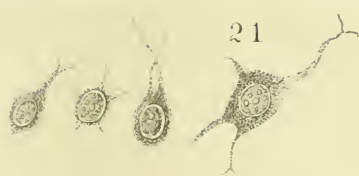
Fig. 26.—Thin transparent section of the first cervical sympathetic ganglion of a fœtus six and a half months old, showing the structure of the capsule.

Fig. 27.—External surface of a capsule, showing the network or plexus of nervous fibrillæ, also the bundles of fibrillæ arising from it, and joining a neighboring nerve bundle.

The above figures are magnified 420 diameters, with the exception of the first three.

Art. II.—[1]THE ABUSE AND USE OF BROMIDES.

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MR. PRESIDENT AND GENTLEMEN: The time allowed by custom for the reading of a paper before a medical society, will not permit me to treat the subject of the abuse and use of bromides as fully as its importance deserves. I shall only be able to consider the salient points of the topic, almost restricting myself to what I have observed in this branch of therapeutics.

The paper will consist of two parts. The first devoted to a study of bromism, or intoxication by the bromides; the second to a succinct statement of my own method of using the bromic salts in the treatment of epilepsy and other neuroses.

Bromine (from βρωμος a bad smell,) was discovered in 1826 by a French chemist, Balard, and to him we also owe the production of the bromide of potassium.

Bromide of potassium appears to have been soon tried by physicians, but it was not until 1840 that its physiological and true therapeutical effects were first apprehended. This was by a German, Otto Graf.[2]

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1 Read before the New York Medical Journal Association, April 25, 1877.
2 De Kali bromati efficacitate interna experimentis illustrata. Lipsiæ 1840. (From Voisin's Essay.)

In 1850, Huette,[1] of Paris, read to the Societie de Biologie a remarkable paper, which may be considered as the basis of the modern use of the bromides. Huette observed and accurately described the general sedative effect of the bromides; their depressing action upon the sexual organs; the anæsthesia of the palate and throat, the mental torpor, the disorders of mobility, and the cutaneous anæsthesia produced by the drug. He also determined (against a number of physicians) its uselessness in late syphilis. Huette may, consequently, be rightly considered as having been the first to describe mild bromism.

With respect to the use of bromides in neuroses, and especially in hysteria and epilepsy, it is generally admitted that Sir Charles Locock[2] was its originator and advocate, and that Brown-Séquard did the most to systematize and render successful the bromic treatment of epilepsy. For further historical considerations I would refer to Auguste Voisin's excellent essay published in 1875.[3]

From this time (1857) the bromides have been used by an increasing number of physicians in an almost endless list of diseases and symptoms. Among these may be mentioned, Hysteria, Epilepsy, Infantile Convulsions, Puerperal Convulsions, Sexual Excitement, Chorea, Tetanus, Delirium Tremens, Insanity of active form, Melancholia, Cerebral Excitement and Insomnia, Somnambulism, Vomiting, Headache, Diabetes, etc., etc.

This general use of the various bromides (of potassium, sodium, ammonium, lithium, camphor, etc.,) was largely empirical; the medicine being prescribed because of its quieting effects, and without strict regard to its physiological action.

From 1867 to the present time numerous researches upon the effects of bromides upon the healthy organism have been made by competent observers in various countries,[4] and since

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1 Recherches cur les propriétés physiologiques et thérapeutiques de bromure de potassium. Mém. de la Société de Biologie. 1850, Vol. II., p. 19.

2 The Lancet. 1857, I, p. 528.

3 De l' emploi de bromure de potassium daus les maladies nerveuses. Paris, 1875.

4 For *resumé* of physiological action consult H. C. Wood's Treatise on Therapeutics. Philadelphia, 1874; p. 278, et seq.

the publication of these papers there has been, I believe, a more rational and moderate use of bromic salts.

The most important conclusions reached by these physiologists respecting the *modus operandi* of the bromides, are two in number. According to some, (Brown-Séquard, Amory) the bromides act by causing contraction of arterioles and consequent diminution in the amount of blood in the nervous centres; while according to others, (Eulenberg and Gutmann, Laborde, Wood) they affect the nervous tissues directly. All agree, however, in considering the physiological result of the action of the bromides, to be lessened irritability of the nervous centres, especially in the motor tract.

I cannot, of course, now enter upon a discussion of this question, which is really only one phase or face of one of the greatest questions in medical philosophy; viz., whether biological processes are more dependent upon vascular (vaso-motor and hæmic) changes, or upon varying degrees of cellular activity, but I may be allowed to give it as my opinion that the bromides act mainly in the second way referred to; viz., by an action upon the anatomical elements (ganglion cells, chiefly,) of the central nervous system. This belief is based upon physiological experiments in animals, clinical observations in man, and largely, also, by the phenomena of bromism; which last are, it seems to me, quite inexplicable by the first, or vascular theory of the action of bromides.

Chiefly, in consequence of the prevalence of the empirical notion that the bromides are called for whenever there is excitement, and partly, also, because of the extreme application of certain theoretical views concerning the physiological and pathological importance of changes in the amount of blood in the brain and spinal cord, there has been, and is still, I believe, a great abuse or over-use of the various bromides, and it is not seldom that we meet with patients who have been kept in a condition of impaired nutrition and nervous atony for months or years, by means of these medicines, and with others (less numerous) who present the toxic symptoms of the drugs, who have bromism, so-called.

The remarks which follow upon the abuse of the bromides, are naturally divisible into three sections: 1, concerning the

general description of mild and of severe bromism; 2, respect-
the complication which bromism may cause in diagnosis; and,
3, with reference to the legal aspects of bromism.

I.   Bromism of varying degrees.   In a number of cases, I
have observed the following symptoms superadded to legit-
imate symptoms of disease; general debility, with weak pulse
and coldness of the extremities; a tendency to stupor; slight
difficulty in speaking, partly due to an aphasiform state; the
bromic breath and acne.   These persons were weak, anæmic
individuals, who had been given the bromides for the relief of
certain head symptoms, which were quite gratuitously supposed
to be due to cerebral congestion.   In some of these cases, moder-
ate doses of the drug had been taken for long periods of time,
with frequent temporary relief to some symptoms.   Yet all
the while the patients' general condition had been kept below
par, in spite of tonics and selected food.   I have observed the
same mild bromism, without any real improvement, in some
cases of hysteria and hystero-epilepsy.   Again, in melancholia,
a disease in which cerebral nutrition is quite surely lowered
and perverted, I have known injurious effects follow the
prolonged use of bromides.   In addition to the instances
enumerated, there is a large class of patients who, without
having any definite disease, suffer from nervousness, imperfect
sleep, queer sensations about the head, and who constantly
over-estimate their symptoms, and to whom the physician or
druggist says, in an off-hand manner, "take a little bromide."

It may be said that often the giving of the bromides in the
above manner does not produce positive ill effects; but to this
I would reply, first, that from what we know of the physio-
logical effects of the bromides, such dosing must produce a
general depression, or lowering of vitality, which few patients
can tolerate; and, second, that, on general principles, physi-
cians are in duty bound to give no superfluous or non-indi-
cated drug to their patients.

Bromism may be much more severe than depicted in the
above statement; it may attain the dignity of a distinct
morbid state, with a clear symptomatology, a well-known
course, and I am disposed to think, a central lesion.   Huette,
in 1850, gave a partial picture of this severe intoxication, and

Prof. William A. Hammond has furnished us with a fuller representation of all its graver details.[1]

The chief symptoms of this condition are:

Cerebral: there is a gradually increased stupor, and dullness of intellect; language is impaired; failure of memory and difficult articulation; the memory is much weakened generally, hallucinations, delusions, and even delirium may supervene.

Spinal: general debility becomes marked paresis, and a staggering gait, like that of an intoxicated person, is developed; the facial and other muscles are tremulous; the reflex functions of the palate and throat are abolished; general cutaneous and mucous sensibility is much dulled; the pupils are wide and sluggish; the facial expression idiotic or maniacal; the menses reduced or arrested; the virile power reduced, etc.

Vaso-motor and trophic: the heart beats feebly; the arteries carry less blood and show less impulse; the peripheral circulation is sluggish and the extremities cold; the breath is foul and quite characteristic; the skin of the face and body is covered with acne; the skin and mucous membranes are dry; the saliva scanty and sticky; sometimes ulcers or a rupia-like eruption shows itself on the extremities.

These symptoms may be so aggravated as to simulate dementia, mania, or general paralysis of the insane; and even death may ensue from extreme debility.

I desire particularly to insist upon the resemblance between bromism and general paralysis of the insane. In both we observe tremor of the facial and lingual muscles, producing a peculiar vibratory speech; in both there is an uncertainty in the performance of certain movements, as walking or using the hands for fine work ; in both there is a failure of intellectual force and of memory. Even somewhat exalted notions may be present in bromism, though this is rare. In general paralysis we have other important symptoms, such as contraction and inequality of the pupil, sexual excitement, peculiar epileptiform seizures, remarkable remissions in the symptoms,

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1 On some of the Effects of the Bromide of Potassium when administered in Large Doses. Quarterly Jour. of Psychological Medicine, II. 1869. p. 46.

and often good physical health, with tense arteries; all these symptoms being wanting in bromic intoxication. Severe bromism is, I am happy to say, very seldom produced, except during the early stage of the treatment of obstinate epilepsy, chiefly for the reason that the doses given for other affections are insufficient to bring about such a result, in the majority of persons. Dr. Hammond believes that bromism is rarely produced by doses less than thirty or forty-five grains of the bromide of potassium daily, and my experience would lead me to a similar estimate.

Occasionally we deliberately produce severe bromic intoxication. This is done in some severe cases of epilepsy, though even in these we seldom go further than creating a state intermediate between the two conditions I have described. Again, bromism has been proposed as a cure for the morphine habit, or mania. Dr. Geo. M. Schweig,[1] of New York, has published a most interesting case in illustration of this procedure. The medication is certainly very heroic; but it is doubtful if any treatment not imminently dangerous to life, is not acceptable in such a terrible malady as the opium habit. Dr. Schweig's paper is, furthermore, an admirable study of the severe effects of the bromides.

II. Bromism as a complication in diagnosis.

The following case is related by Voisin.[2]

A patient who had been under treatment for epilepsy became, as his physicians thought, insane, and was sent to Paris to consult Voisin. The patient was found at a hotel in a state of violent mania, beset by frightful hallucinations of hearing, and shouting loudly. Later there was stupor, loss of memory, of affection, and of appetite; the walk was oscillating, and all movements were irregularly performed. Titillation of the nares and throat showed complete loss of reflex action, the hands were tremulous, and the facial and lingual muscles were the seat of fibrillary contractions. The pupils were equal, and the symptoms had developed very acutely after the use, during some months, of potassium bromide in doses of 90 to 120 grains. The medical officers of an asylum in which

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1. Cure of the Morphine Habit. New York Medical Journal, May, 1876, p. 495

2. Op cit. p. 68.

Voisin placed this patient, thought he was mistaken in his diagnosis of bromism, and in his favorable prognosis; they looked upon the case as one of general paralysis of the insane.

In a week after the cessations of the bromides and the use of vapor baths, purging, black coffee, etc., the symptoms subsided, and in thirteen days the patient was sent back to his home in the country, quite well.

In a case which I saw in consultation, the addition of bromism to other symptoms led to the diagnosis of cerebral lesion of the gravest kind, when really only the basal dura mater was involved. The following is a summary of this to me, instructive example of the evils of the purposeless giving of the bromides in large doses.

I was sent for to see Mrs. X. in a country town, near New York, on October 18th, 1875. She was under the care of a very intelligent practitioner, and had been seen by a prominent oculist of New York. I learned that this lady, then aged fifty-five years, had enjoyed good health during her adult life. In 1863, in Europe, she had an attack of mydriasis on the right side, without diplopia, or ptosis, or lesion of the fundus, or headache. This disappeared in three or four months. In 1870, having been well during the interval, while again in Europe, experienced internal strabismus of right eye with diplopia, but no pain. An oculist of Naples performed tenotomy of the internal rectus without relief. She had not then (and has never had) neuralgic pains in the legs, osteoscopic pains, sore eyes, or sore throat; never was dizzy or faint. In 1872 had pain in the head for the first time, in the shape of neuralgia of the right supra-orbital nerve. This pain has been present ever since with great variation in intensity. Patient was often awakened by severe pain at 3 or 4 o'clock A. M. The pain soon affected the whole of the right temporo-frontal region, with some extension into the eyeball and orbit. About one year ago, (1874) and often since had tingling in all the superficial branches of the right trigeminus. No irritation of the acoustic nerve. During the past year the sight of the right eye gradually failed, and slight exophthalmus appeared. Returned to America about one month ago and was fairly well on board ship. Soon after landing, the

local head and brow pains became much more severe, the pain apparently shooting through the right anterior lobe of the brain. There was no ptosis, but the eyeball was fixed in internal strabismus, without dilatation of the pupil. Bromide of potassium was then given internally in doses of 60 and 90 grains per diem, blisters were applied behind the ears, and morphia exhibited. Patient became weaker. On Oct. 1st, ptosis appeared, there was only perception of light in the right eye; vision normal in the left eye. The bromide of sodium was then substituted to the potassium salt, and given in doses ranging from 90 to 180 grains per diem. Patient grew weaker and weaker, was stupid, used wrong words, staggered while standing or walking; hands were tremulous. Oct. 12, bromides stopped and the iodide of potassium given in 10 grain doses three times a day. About this time slight anæsthesia of the right brow was discovered.

I found the patient, on the 18th, in a state of hebetude, speaking a little thickly and slowly, and quite often using the wrong word. She is perfectly intelligent. There is an abundant flow of buccal saliva and nasal mucus, but no acne. The left side of the face and the tongue are normal. Smells with both nostrils. On the right side there is ptosis, and on raising the eyelid the eyeball is found immovable nearly in the median line, its pupil of medium size and fixed; only perception of light on this side. The ophthalmoscope shows simple atrophy of the optic nerve, there is no choking of the disk and no trace of hæmorrhages in the retina. The fundus of the left eye is normal, and its vision is good; field not impaired. The seat of pain is as described above. The brow, temple, and fronto-parietal region on the right side are partly anæsthetic. There is no palsy of the face or extremities, no anæsthesia of fingers, no referred sensations (numbness, etc). The walk is titubating but not hemiplegic. Patient repeats that she has never lost consciousness. Her pulse is regular, beating 80 in the minute, and her buccal temperature is $98°9$ F. The attending physician and the consulting oculist had concurred in diagnosticating a tumor in the right middle fossa of the skull, involving the brain.

My own conclusion was that we had to deal with an inflam-

matory affection of the dura mater in the right middle fossa of the skull, compressing the nerves, etc., which pass through the optic and anterior lacerated foramina, and not involving or affecting the brain substance. The cerebral symptoms present seemed to me to be those of bromism, partly by their intrinsic characters and mode of appearance, and partly because they were not those which a lesion at the base of the brain, on the right side, could produce. Furthermore, I rejected the idea of a cerebral lesion because of the absence of hemiopia and of lesion in each eye, of hemiplegia on the opposite side, both of which symptoms a tumor in the middle fossa must of necessity produce by pressure upon (*a*) the right optic tract and (*b*) the right crus cerebri.

As to the nature of the inflammation, I gave no opinion ; the social position of the patient, her blooming family of children, and her own medical history previous to 1870, being, opposed to a syphilitic theory. Still I considered that we were in duty bound to give her the benefit of the doubt, and I urged the attending physician to continue withholding the bromides, to give the iodide of potassium in gradually increasing doses, to relieve the pain by hypodermic injections of morphia, and to support the patient with food and stimulants. It will suffice, for the present purpose, if I state that after the half ounce of iodide of potassium per diem was reached and passed, improvement began and progressed rapidly. The medicine was carried up to ℨvi. a day, and held at that dose for some time, then gradually decreased ; doses of ten grains being taken as late as the spring of 1876. The symptoms of supposed cerebral lesion passed away in a few days, and the local symptoms gradually disappeared except the atrophy of the optic nerve. I met this lady a few months ago, and she seemed in perfect health, with the exception of slight imperfections in the movements of the right eyelid and eye-ball, and of loss of vision in the eye.

It would be easy for me to relate other cases, illustrating the proposition that bromism may embarrass diagnosis, but my space is limited, and the two examples given above are perhaps sufficiently demonstrated.

I should add, however, that apart from the above special

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symptom-groups, the use or abuse of bromides may give rise
to a condition of general debility, and to a weakness of the
heart, which are not then by any means as serious as when not
produced by the bromides.

III.　Bromism in its medico-legal aspects.

I am not aware that bromism has ever been brought into
the courts as a matter for study and decision, but it may
eventually be so under several circumstances.

First, with respect to the responsibility of the physician
administering the medicine which so debilitates a patient,
physically and mentally, as to expose him to various mishaps.
For example, I know of a case in which the patient, suffering
from acute bromism, fell asleep in a railway station, and was
robbed of four hundred dollars, so great was the stupor
porduced by bromides, by thieves who undoubtedly won-
dered at the man's indifference to their manipulations.　Dr.
Hammond[1] relates, in his essay on Bromism, the case of
a gentleman, one of his patients, who was arrested on the
street for drunkenness, and locked up over night in spite of
the doctor's remonstrances and explanation.

Second, as to the patient's responsibility for criminal acts
committed while brominized.　It is perfectly possible that
such a patient shall take from a store articles not paid for,
through defective memory ; that he shall be mistaken in the
identity of persons, and thus be led to be improperly familiar
or abusive ; or that he shall enter a house or room not his
own, etc.

Third, with respect to the legal capacity, both for ordinary
business and for testamentary disposition, of brominized per-
sons. · Each case will, of course, have to be studied by itself,
but it must be admitted that in some cases of bromism, the
stupor, loss of memory, and aphasiform difficulty are so great
that the patient is, for the time being, as truly *non compos
mentis* as if he had a natural secondary dementia.

A decision will be the more difficult to reach, because in
mild and in moderately severe bromism the judgment and
general intellection are remarkably well preserved, behind a
vail of striking superficial symptoms, as impaired articulation,

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1 Journal of Psychological Med. l. c.

stupid expression, staggering gait, partial weakness of memory, muscular weakness and tremor, etc. Again, in some cases, it will be necessary to make a close analysis of the patient's antecedents, in order to clearly ascertain how much of the mental impairment depends upon the medicine, and how much upon the disease for which the medicine was prescribed or taken.

Fourth, with reference to the production of death through bromism. This idea is suggested by reading a case of fatal bromism related by Clarke and Amory,[1] in which a nurse, literally applying orders given him by a physician, continued giving enormous doses of bromide of potassium in spite of progressive weakness. When seen by Dr. Clarke, the patient was past recovery, and sank in a week.

There is a possibility that this procedure may some day be repeated with criminal intentions; *e. g.,* for the purpose of getting rid of a burdensome and incurable invalid.

I shall now proceed with the second part of this paper; viz., a statement of my own mode of using the bromides in the treatment of epilepsy and other neuroses.

In prescribing the bromides for Epilepsy, I have been guided by ideas, which can, perhaps, be best expressed in the form of terse propositions.

1. In view of what we know of the physiological and toxic effects of the bromides and in accordance with either of the two generally received hypotheses of their modus operandi, anæmia and debility, or congenital feebleness, contra-indicate prolonged use of the bromide.

2. The bromides are, on the contrary, well borne by persons of fairly full habit and good nervous power.

3. The bromides are indicated in cases of abnormally great irritability of the nervous system, in its motor (muscular and vaso-motor,) and ideational tracts.

4. Epilepsy is so serious a disease, one which, if not interrupted, kills the patient, or reduces him to dementia, that we are justified in using unusual and heroic measures in its treatment. Hence, the contra-indications named above are to be

1 The Physiological and Pathological Actions of the Bromide of Potassium, Boston, 1872, p. 62.

much less regarded in the management of this formidable neurosis.

5. As a corollary to the last proposition, I may state that I consider epilepsy to be the only disease for the cure of which we are justified in deliberately producing a degree of bromism.

My method of prescribing the bromides in a common case of "idiopathic" epilepsy, is the following:

I employ one of two solutions, made according to a standard formula.

℞	Potassii bromidi	-	-	ℨi
	Ammon. bromidi		-	ℨss
	Aquæ font.	-	-	-	ℨvij

M

S. To be given by the teaspoonful:

℞	Sodii bromidi	-	-	ℨj
	Ammon. bromidi		-	ℨss
	Aquæ font.-	-	-	℥vij

M

S. To be given by the teaspoonful.

These simple solutions, which I have found much more palatable to most patients than those made with infusions or syrups, contain forty-nine doses; i. e., each teaspoonful contains ten grains of the potassium or sodium bromide, and five grains of the ammonium bromide.

The solution is given several times a day, nearly always so divided as to give by far the largest dose in the evening. This is Brown-Séquard's rule, and the principle involved is to keep the system thoroughly under the influence of the drug during the night.

I direct for an adult male epileptic, that a teaspoonful shall be taken before each meal, and two teaspoonfuls at bed-time, largely diluted. In the case of delicate males and of females, I at first prescribe only a teaspoonful before two meals, and two teaspoonfuls at bed-time, and in some young persons or very small and tender adults only, one dose before breakfast, and then at bed-time. The patient taking these initial, or trial doses is carefully observed, the sensibility of the palate and

throat frequently studied, and information obtained from the patient and his friends as to the absence or presence of stupor. Guided by these signs, or their absence, I cautiously increase the bromide, still keeping the nocturnal dose the largest, until slight bromism is produced, as evidenced by absence of reflex movements in the throat, and slight stupor. I pay but little attention to acne. During the rest of the treatment, I aim to give the patient just as little bromide as shall prevent attacks of epilepsy, yet I nearly always find it necessary to keep up slight bromism for months.

The precise amount required per diem in a given case can only be determined by careful observation of that case, and is not to be deduced from general experience. At times, remarkable idiosyncrasies are observed which inexplicably render the patient very susceptible or very rebellious to the bromic influence. Very many of my patients take, month after month, one dose (fifteen grains of the two bromides) before each meal, and three doses, (forty-five grains, at bedtime); a total of ninety grains. As extremes illustrating peculiarities, I may cite the case of a girl of twelve years, who, for weeks took one hundred and fifty grains per diem without bromism, and that of a young lady of twenty, who was decidedly influenced by one teaspoonful before breakfast and two at bed-time; a total of only forty-five grains per diem. In the latter case, had I given the usual doses taken by adults, I should have produced severe bromism.

With respect to children I find that they tolerate the bromides (and iodides as well) in relatively large doses, and little patients of mine often take sixty grains of the bromides a day; while to mere infants I give (after careful trial of smaller doses) twenty to forty grains a day.

It appears to me very important to thoroughly dilute the bromides, in order to facilitate their absorption; I usually direct that the dose be taken in a wineglassful or half a tumblerful of water. Furthermore, I give the medicine on an empty stomach.

With respect to the practice of giving a very large dose at bed-time. Theoretically, upon physiological grounds, it appears right to obtain the greatest bromic action in those hours

when the reflex power of the motor part is probably heightened, and when epileptic seizures often occur; and, again, as a great number of hours must elapse before another dose can be taken, it seems right to give an extra large amount to keep up the medicinal effect. Empirically there can be no doubt of the great importance of this rule. Brown-Séquard's extraordinary success in the treatment of epilepsy was in part due to this, and I have several times seen patients who had been taking a large amount of the bromides in three equal doses without much improvement, and who have had fewer attacks immediately after subdividing the same amount in such a way as to give a large dose at bed-time. For example, thirty grains three times a day did a little good, but fifteen grains before each meal and forty-five grains at bed-time checked the disease much more.

Another of the reasons of Brown-Séquard's success was his positive direction that under no circumstances should the bromides be discontinued; and I have always studiously followed his teachings in this matter. The bromides may be *diminished* but never *stopped* until the word *cure* can be pronounced. Even during intercurrent acute diseases, as colds, fevers, accidents, the bromides should be given regularly, though in reduced doses, partly because the nervous system resists less in that condition, and partly because such attacks of illness, or accident, interrupt the epileptic habit. The omission of the bromides for a very few days may allow a fit to occur, and thus destroy the good work done by months of patient care.

How long must the bromides be taken in epilepsy? This is a question to which we can as yet give no answer. Brown-Séquard and Voisin give it for at least three years *after the last attack*, and I think that this is a minimum of time. I have twice been grievously disappointed by the return of attacks after an immunity of over two years, and others have known recurrence to take place after even a longer period.

Some patients who have had epilepsy for many years are partially demented, and take the bromide unkindly; they become irritable, feeble, and have nearly as many attacks as without the drug. In such cases parents will often ask you if it is worth while to give the bromides systematically, and to bear

with the bromic symptoms. I generally answer this question negatively, yet state to the parents that as the patient may die in a paroxysm it is our duty, on general principles, to do anything which shall diminish that risk.

It will be inferred from the foregoing that I rely upon the bromides of ammonium, sodium, and potassium, for the treatment of epilepsy, and this is in one sense so.

No medicine, it is now generally admitted, has such power over the epileptic habit, and does good in so many cases, as the bromides, and it would seem as if the day for trifling with such doubtfully efficacious medicines as zinc oxide and sulphate, copper sulphate, belladonna, strychnia, setons, diet, etc., had passed away. With the bromide of calcium, lithium, zinc, and arsenic, I have had little or no experience. The last named is loudly vaunted by Clemens, of Frankfort on the Rhine.

I would not, however, have it understood that I employ *only* the bromides in the treatment of epilepsy. On the contrary, what measure of success I have is owing in part to the fact that I made a large use of other means, together with the bromides; and this seems to me so important that I shall take the liberty of digressing a little to specify what this adjunct treatment is.

In the first place, I employ means which tend to counteract the unpleasant effects of the bromides.

The acne may to a certain extent be prevented by administering arsenic from time to time, either in the shape of the solution of arsenite of potassa, or of arsenious acid. Sulphur ointments, mercurial plaster, alkaline lotions, may also be employed.

The general debility or slight paresis produced by the "continuous dose" (Clarke) of bromides, is corrected by strychnia, by nux vomica and zinc oxide, and by quinia. Drowsiness and the more serious symptoms of bromisms are relieved by inhalation of nitrite of amyl, by stimulants, and by quinia. The anæmia and general depression of the vital functions produced in the course of the management of a case of epilepsy, I meet by care in giving the patient nutritious diet, by giving cream or cod-liver oil, and by administering such

medicines as iron, quinia, phosphorus, strychnia, with nitro-muriatic acid, wine, beer, or whisky, and by regulating the patient's hygiene.

Important hygienic rules in the treatment of epilepsy, are the avoiding of large meals at night, regulating the function of the bowels, kidneys, and skin, early rising, and great moder-ation in sexual gratification.

In the second place, I employ, in some cases, a few medi-cines which act more directly upon the morbid state of the nervous centres. These are belladonna, cannabis indica, oxide of zinc, strychnia, sulphates of zinc and copper, etc. My favorite is the first named, and I have known the best effects to follow its association with the bromides. For example, a patient passed into my hands after having been a long time under the treatment of a distinguished physician with a moderately good result; under a given quantity of the bro-mides she had attacks about fortnightly. I did not increase the bromides or change the method of taking them, but at once gave extract of belladonna in doses of gr. $\frac{1}{8}$ three times a day. The patient acquired a dryness of the throat, and the attacks were reduced in frequency to once a month, once in two months, three months, and she has now been thirteen months without an attack. Of course the belladonna was not con-tinued during all the two years of treatment. At first it was used for two or three months in succession, and afterward given from time to time.

During the many months or years of the treatment of a case of common epilepsy, I ring the changes on the medicines above enumerated; nearly always giving something in ad-dition to the bromides. And I may be allowed to repeat that the bromides, though often changed in amount, and sometimes in kind, are never withheld.

The treatment of cases of epilepsy in which a definite causa-tive lesion can be made out, is of course somewhat different. I refer now to epilepsy due to syphilitic lesions, to peripheral disease, to cranial and neural injury, etc. In these varieties, I use the bromides to combat the epileptic habit, to prevent discharges (using Hughling Jackson's phraseology), and at the same time meet the special indication by using mercury and

iodide of potassium, by correcting visceral disease, by removing some external irritating cause, or by an operation like trephining, neurectomy, etc.

In other neuroses, I have used the bromides sparingly, and never continuously.

Hystero-epilepsy and hysteria have not seemed to me much benefited by the bromides. As far back as 1857, Locock remarked that the bromide of potassium was especially successful in hysteria of distinctly ovarian or uterine origin. I would not condemn the use of the bromides in hysteria, but would protect against its being given in such a manner as to produce bromism.

Delirium tremens is probably shortened by the free use of the bromide, while Home Physician to the New York Hospital I treated, under the directions of the Attending Physician many cases of this disease with doses of forty and sixty grains, repeated two and three times in the course of the evening. It was our belief that in cases uncomplicated by visceral disease, we thus rapidly produced sleep.

Insomnia I think, is often treated by bromides, upon the purely hypothetical indication of causing anæmia of the brain—an indication reposing upon insufficient physiological experimentation, and upon belief in the notion that the bromides directly produce cerebral anæmia. Many cases so viewed might be much more quickly relieved by chloral, or by a glass of ale, or by correcting indigestion. A case of well-marked insomnia needs, it seems to me, to be investigated in the broadest manner, without failing to keep in mind that this symptom may depend upon a number of pathological conditions. As to the immediate cause of sleep, I believe it to be due partly to the waste of tissues generally, and the presence in the blood of an accumulation of the products of retrograde metamorphosis (Preyer's theory), and partly to the exhaustion of the cerebral tissue itself. The anæmia which is observed in the brain during sleep is, it appears to me, a concomitant, or consequent phenomenon, in obedience to the general law that a tissue in repose contains less blood than one in action.

Insanity is often, I believe, erroneously treated with the

bromides. I have several times seen patients with melancholia made weak and wretched by large doses of the bromides which failed to make them sleep; and in mania I have known precious time wasted in vain attempts to get sleep by these medicines. I would only employ the bromides in insanity to meet a few indications, such as a tendency to epileptiform attacks, or abnormal sexual excitement, or great nervousness not caused by delusions.

I have not used the bromides in Chorea and Neuralgia. It is vaunted in migraine, but in some half dozen cases in my practice it has given absolutely no relief to the attack. Extreme irritability of the bladder with pain, in a female, was very greatly relieved by a retained vaginal injection of $\ni$i bromide of ammonium. The idea was to produce anæsthesia of the vagina and vulva. Upon a similar principle we all use gargles of bromides in neuralgic or myalgic sore-throats, and in the cough of laryngeal irritation, with fair success. Vomiting in pregnancy, after morphia, ether, etc., may be arrested by a judicious use of the bromides. It has been proposed to give bromide of potassium an hour or so before administering ether, or giving opium or morphia, with the view of preventing nausea and vomiting. This practice has seemed successful in my hands also.

Hay-asthma, or hay-cold, is a disease for which multitudes of medicines have been tried, without much good result. Last year, I induced two or three persons suffering from this disease to employ a strong gargle of bromide of ammonium, and to wash out the nasal passages with a weak solution of the same salt several times a day during the attack. The result was so gratifying that I am disposed to ask physicians to give a fair trial to this treatment during the coming summer and autumn. The gargle to be of the strength of ʒi or ʒij of bromide to ʒj water; the solution for the nares much weaker, of from 10 to 30 grains of bromide in ʒj water.[1]

1 New York Medical Record, Nov. 11, 1876. p. 757.

Art. III.—MENSTRUAL NEUROSES.

By Montrose A. Pallen, A. M., M. D.

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of the City of New York, Surgeon to Char-
ity Hospital, Etc., Etc.

(*Read before the New York Neurological Society, April 2d,* 1877.)

Case I. *Cerebral anæmia, loss of consciousness, volun-
tary motions, and sensibility—epileptiform convulsions.*

MRS. B., aged twenty-eight; widow, no children; petite
brunette; and, during the intermenstrual period, appar-
ently healthy. The uterus is normal in size and position; no
endometritis, no leucorrhea. The ovaries are not enlarged
and not sensitive. She dates her trouble from the death of
her husband, whom she nursed during a prolonged attack of
dysentery during the intensely hot weather of Canton, in
China. Upon the demise of her husband, she was menstru-
ating, and the shock induced by the sudden and unlooked for
catastrophe, in consequence of intestinal hemorrhage, threw
her into a series of "fainting fits," which continued for more
than thirty-six hours. This took place in September, 1871.
From that date until February, 1875, she never passed a men-
strual period without similar attacks, and had consulted vari-
ous medical gentlemen in China and in Europe, with no amel-
ioration of her symptoms. She returned to New York in
January, 1875, and was borne off the ship in a very prostrate
condition. I saw her in the evening of the day of her arrival,
and then learned the above history from her brother, who was
a physician. As this was the last day of the menstrual flow,
and the symptoms had always subsided upon its cessation, I
refrained from all treatment whatever, desiring to base the
therapeutics upon such diagnosis as a future study of the case

might develop.   One week afterwards the lady was subjected
to a most rigid examination.   Touch, conjoined vaginal, rectal
and hypogastric palpitation, as well as the sound and specu-
lum revealed nothing.   The heart murmurs were feeble, and
the apex beat somewhat lower than usual—venous murmurs
were heard at the base of the neck.   The diagnosis thus far
was enfeebled heart walls, with possibly a little dilatation of the
left ventricle.   Subsequent examination of the urine, showed
excessive phosphatic deposits, but no casts or albumen, there-
fore the ventricular enlargement could not be due to Bright's
disease of the kidney.   A subsequent study of this case one
week afterwards, developed nothing more.   The diagnosis
then was anæmia of the medulla oblongata, in consequence of
the menstrual molimen, which in turn reacted upon the car-
diac and respiratory nerves, developed when menstruation was
proceeding.   Digitalis, manganese, iron and quinine were or-
dered, together with daily faradization of the entire spine,
ovaries and uterus.   On the ninth of February, she expected
the catemenial discharge, and we awaited its return with great
solicitude.   She was ordered to keep the recumbent posture,
to drink freely of milk punch, and under no circumstances to
raise her head from the pillow.   Hitherto these fainting spells
had frequently been ushered in by marked convulsions of an
epileptiform nature, the history of which went very far to-
wards the formation of the diagnosis of anæmia of the medul-
la.   Our hopes were disappointed, as she had a slight convul-
sion almost without premonition, followed by profound syn-
cope.   Anticipating this result, I had provided myself with
the nitrite of amyl, and immediately administered eight drops
by inhalation.   In a few minutes the pallid cheek became
flushed.   She opened her eyes and stared at us in a semi-
conscious state, in which condition she remained for fully a
half hour, when twelve drops of the amyl were dropped upon a
handkerchief and administered by inhalation, until there was
marked injection of the conjunctiva and swelling of the veins
upon the temporal regions.   She now spoke somewhat inco-
herently, but soon complained of a sense of pricking of the
face and fullness of the head.   In forty minutes she had re-
gained her consciousness, and was apparently free from her

usual syncopal symptoms. I then examined her by touch
per vaginam, and found that menstruation was freely going
on, (more freely, as she afterward stated, than it had be enfor
many years,) possibly in consequence of the hyperæmic action
of the nitrite of amyl, which I directed her brother to admin-
ister if the convulsive or syncopal symptoms should reap-
pear. On the next day I saw her again, and the report was
that she had had two slight fainting spells during the night,
which were relieved in a few minutes by ten drop inhalations
of the nitrite of amyl. During this day and for sixty hours
subsequently, she inhaled every five hours, successively, eight,
six, and four drops of the nitrite of amyl, without the reappear-
ance of any of the former symptoms.

March 5th. Patient expects menstrual flow, as she has
every symptom of the molimen, such as heaviness and fullness
of the loins and pelvis, but none of the usual feelings of dizzi-
ness, floating objects before the eyes and tinnitus aurium.
The nitrite of amyl was ordered in five drop inhalations every
four hours, unless syncope came on, when twelve drops were
to be administered. The flow did not come on, but patient
awakened in the morning of the 6th with decided uterine pain,
which was followed by the expulsion of a clot as large as a fil-
bert, and succeeded by the menses. No faintings or convul-
sions. Nitrite of amyl continued as on yesterday.

March 6th. Menstruation in full process; no faintings or
convulsions. Nitrite of amyl in three drop inhalations every
three hours.

March 7th. Menstruation less, but patient has had one
slight fainting spell, as she refused to inhale any of the amyl
in the morning, having conceived a disgust for it. The inhal-
ation of eight drops, however, promptly restored her to con-
sciousness.

March 8th. Menstruation ceased, and six ounces of cham-
pagne ordered every six hours.

March 9th. Patient apparently well, but nauseated from
the previous inhalations of amyl and the large quantities of
champagne used on yesterday.

March 10th. Patient drove out, and feels very comfortably.
At this date there are no heart murmurs or are there any ve-

nous sounds at the base of the neck, and the digitalis, iron, manganese and quinine were suspended. Routh's chloro-phosphide of arsenic in ten drop doses after each meal were substituted therefor. The faradizations of the spine, from this date until April 3d, were administered on every second day instead of every day.

April 3d. Menstruation at hand. Nitrite of amyl ordered as before,—no syncope and the patient slept well.

April 4th. Menstruation appeared. No syncope; no bad symptoms. Nitrite of amyl in five drop inhalations every four hours.

April 5th. Same as yesterday.

April 6th. Same as yesterday, except that the inhalations were given only twice, and in three drop doses.

April 7th. Menstruation ceased; treatment suspended. From this date until May 1st, the patient was faradized only once a week, and the arsenic was suspended on the 25th of April.

May 1st. Menstruation expected; no treatment.

May 2d. Menstruation appeared, but no bad symptoms.

May 3d. Menstrual flow proceeding freely; no bad symptoms.

May 4th. Slight vertigo, for which three drops of the amyl were given.

May 5th. Menstruation ceased; no treatment.

From this date until May 29th, no treatment was given, as the patient was apparently free from heart debility; was ruddy and florid, and very exultant over her condition.

May 30th. Menstruation appeared with no bad symptoms, and continued until June 4th. The patient was dismissed as cured. In February, 1876, she was married, and has had no bad symptoms since.

CASE II. *Paralysis of the right arm, supra and infra-clavicular region, and right breast.*

Miss A., aged 16, fair complexion, light hair, blue eyes, well developed and nourished. She had menstruated first when she was twelve years old. For the preceding year she had been confined closely in a boarding school, and had had five or six menorrhagic attacks, which lasted from six to ten

days. Her menstruation had usually existed but three days, was painless, and produced but very slight constitutional disturbances. She awakened one morning prior to an expected flow, and found that her right arm was paralyzed completely, with regard to sensation and partially, so far as motion was concerned,—the flexors and extensors of the fore-arm were immobile, but the whole member could be lifted by a conjoined fixature of the deltoid and triceps. Further examination showed anæsthesia of the right mamma, and the cutaneous surface as far as the sixth rib on the anterior portions of the right chest. I saw her some eight hours after she had first discovered the lesion. She was agitated and alarmed to an excessive degree, but there was no increase of temperature, slight acceleration of pulse, no headache, a slight dilatation of the right pupil, but no conjunctival or intra ocular hyperæmia. The attack was apparently reflex, and due to some derangement of the molecular action of the cord, in consequence of menstruation. That the paralysis was not due to hemorrhage in the cord above the origin of the cervical nerves making up the brachial plexus, was apparent, as the lack of constitutional disturbance, the absence of pain, the previous history of the case, and the preceding menorrhagic attacks, pointed to pelvic origin. The prognosis was favorable, although the diagnosis was not completed until later in the day after the use of faradic electricity, to determine the contractility of special muscles, and of acupuncture to discover if the anæsthesia was universal. There was also a careful examination made of the pelvic contents. The ovaries were enlarged and tender, and the uterine body was swollen and sensitive. The treatment was a good drastic cathartic of aloes, calomel and hyoscyamus, together with hot vaginal douches. The purgative was followed on the next day by a slight menstrual discharge, but with no amelioration of symptoms. I refrained from further medication, as I believed that as soon as the pelvic fluxion, coincident with menstruation, would subside, that one or the other form of the paralysis would likewise disappear. Matters did not improve for four days more, and the menses were so slight, and the uterus was still so sensitive, that I determined to relieve the engorgement, if possible, by the abstraction of

blood from over the sacrum. Six wet cups were applied and twelve ounces of blood taken, and extract of cannabis indica ordered in half-grain doses, until the hemp intoxication would be manifest. On the following day she could move her fingers, and within a week, motion was pretty well established in the entire fore-arm, although the anæsthesia persisted. I then gave her the galvanic current every day for ten minutes, for more than three weeks, with a slight improvement in sensation, and as complete a restoration of mobility as could be expected where anæsthesia existed. Four weeks after, the next menstrual period was at hand, and a return of the paralytic symptoms was anticipated. Fortunately the flow set in very briskly, and on the third day it amounted to a decided hemorrhage, and the anæsthesia disappeared therewith. Here was a dilemma; the menorrhagia was excessive, and telling upon her general strength, and if checked, might invoke the reappearance of the paralysis. However, I ventured to give the aqueous extract of ergot in five grain doses, combined with one-third of the extract of the cannabis indica every six hours. After twenty-four hours, the drugs seemed to modify the flow, but on the second day the anæsthesia of the breast reappeared, and in spite of the galvanic and faradic currents, persisted until the next menstrual flow, when it subsided. The patient had had no more paralysis of motion when I last saw her, some two years since, but I learn by letter that if there should exist any retardation of the menstrual flow, the anæsthesia of the breast reappears, to again disappear as soon as the blood issues from the uterus in any excessive quantity. The uterus, during the inter-menstrual periods, was normal, not sensitive nor intumescent, but became swollen, œdematous (?), and enlarged, as soon as the catamenia set in, very much beyond the usual hypertrophy attendant upon menstruation. She was under my observation from June, 1872, to September, 1874.

Case III. *Hæmorrhage from the nipple, mastodynia and intercostal neuralgia—Asthma and bronchial hemorrhage.* Mrs. D., aged 22, has borne two children, and up to date of observation, August, 1875, has never been irregular or troubled with her menstruation. She is a very dark brunette, remark-

ably strong and weighs over one hundred and sixty pounds. Her youngest child, if living, would be twenty-two months old. She has been menstruating ten months regularly and painlessly. Her menstruation was due in the latter part of July, and not making its appearance she fancied that she was again pregnant, particularly as pigmentation of the forehead, eyelids and breasts was very marked. On the third or fourth of August she took a cold bath, which was followed by acute pain in both mammary glands, with a decidedly knotty feeling of the left, as if one held a bunch of grapes in a thick napkin. The veins on the breasts are swollen and turgid, the pain over the region of the heart intense, but no abnormal cardiac sounds, and there is decided emphysematous respiration over the base of right lung. She spat about three tablespoonfuls of frothy blood and mucus during the examination. There is no increase of temperature and the pulse beats 120 per minute. The uterus is hard, retroverted, swollen and very sensitive, and the ovaries cannot be mapped out, although there is a very tender spot in the region of the left. The diagnosis was obscure, as she might be pregnant, and had contracted an acute capillary bronchitis in consequence of the cold bath, yet this was doubtful as there was no increase of temperature, and the frequency of the pulse might be due entirely to purely neuric causes. In the afternoon of the same day I was hastily summoned, and found that the orifices of the milk ducts were dribbling blood from the left nipple, and she was expectorating pure dark blood. Here was a case of so-called vicarious menstruation. I immediately opened one of the turgid veins upon the breast, and let out about eight ounces of blood, which relieved the mastodynia, the pain in the chest, and in a few hours there was no more bloody expectoration, nor was there any farther bloody dribbling from the nipple. She was ordered three drops each of the tincture of the root of aconite and veratrum viride every two hours, of which she took three doses, causing vomiting an hour after the last one, and her pulse came down to 70.

On the next day, menstruation came on, and with it a subsidence of the asthma and the other pulmonary symptoms. She had no return of these phenomena, but menstruated

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within the next three weeks apparently as healthily as ever. I have seen one other case of hemorrhage from the nipple with intense mastodynia, in a young lady whose menstrual flow was suppressed by getting her feet wet in the snow. The nipple poured out blood at intervals for more than a day, and then subsided. She must have lost two ounces of blood during the progress of the discharge. Her next menstruation came on properly, but the breast continued tender and enlarged for several months.

CASE IV. *Suspension of menstruation on third day, followed by nasal epistaxis; Cerebral hyperæmia.*

Nov. 2d, 1876, I was requested to see Miss——aged 23, who was suffering from intense headache and vomiting. Her pulse was full, strong, and slow, beating 58 per minute. The face was flushed, the conjunctiva injected, and she stated that the pains about the temple and in the back of the head were most agonizing. She had menstruated regularly on the third day preceding, which had stopped, as was usual, in the morning of the day I saw her. As this had been her menstrual habit from the inception of the catamenial law she paid no attention to it, but the nose bleeding which always had been rather free, when the vaginal flow ceased, did not come on; and, instead of it, the headache supervened and increased in intensity, to such a degree as to be unbearable. She was ordered a hot mustard foot-bath, dry cups the entire length of the spine and hot turpentine stupes to the hypogastrium which did not give any relief. I then ordered leeches on the the temples, which were objected to, as her mother thought they would leave marks, whereupon I bled her from the arm about eight ounces when the headache passed off. She then went to sleep quietly, and on the next day the menstruation reappeared as usual. This lady was in perfect health otherwise and has so continued ever since. The appearance of nasal epistaxis takes place upon the cessation of the flow on every third day, and on the fourth day the catamenia again reappear as usual. Here then was a case of cerebral hyperæmia, in consequence of peripheral pelvic irritation, relieved by the abstraction of blood from the general circulation, which nature habitually rectified by nose bleeding. I cannot

explain why menstruation should cease every third day, to be resumed on the fourth, why the local uterine hemorrhage should be supplemented by systemic discharge, other than upon the ground of certain changes developed in the cerebrospinal axis. If the uterine hemorrhage of menstruation be entirely due to fatty degeneration of the mucosa and the bloodvessels ramifying through and underlying it, then there must take place some ischæmic paralysis of uterine structure, a species of uterine tetany of a very remarkable nature, more particularly as the health of the patient has never been impaired save in the instance just related.

The citation of the above cases clearly indicates that normal physiological menstruation is a neurosis indicative of anatomical change , degeneration of tissue and hyperplastic formation. It is coincident with, if not dependent upon ovulation, and the latest theory of independent uterine action without ovarian causation is not susceptible of proof. Periodical fluxions to the pelvic viscera, and so-called vicarious menstruations, as well as hemorrhages from the generative passages, have been known to take place where no *uterus, or only a rudimentary organ* existed. There is no well authenticated case on record where menstruation took place with congenital absence of, or undeveloped ovaries. That hemorrhage from the uterine cavity does take place after removal of both ovaries is a fact, but in the twenty-seven cases tabulated by Dr. John Goodman, of Louisville, up to 1872, there is one quoted from Peaslee's work on *Ovarian Tumors*, where the woman " *menstruated regularly from the cicatrix and vagina*" after the removal of both ovaries and the entire supra-vaginal portion of the uterus. Here is testimony that even the uterus is not necessary for so-called menstruation, but that the mere cervical cicatrized stump poured out the menses(?). Menstruation, or rather the periodical bleeding, implies a process of desquamation and fatty degeneration of the lining membrane of the body of the uterus, involving not only the mucosa but the vascular distribution as well. The case of uterine and ovarian removal proves only that the phenomenon of pelvic monthly periodical blood discharge, *once set up*, may continue when neither ovaries or uterus exist, but it is not clearly indicative that menstruation

is solely and peculiarly a uterine function inseparable from ovulation. One fact deducible from it, however, is that the stimulation of the vaso-motor system coincident with pubertic pelvic development once aroused, abides in the ganglionic system, similar to those molecular changes which take place in all of the ultimate cells presiding over automatic life.

From the cases enumerated by Goodman, as well as the four others gathered by Dr. Ely McClellan, of Louisville, and quoted by Dr. A. Reeves Jackson, of Chicago, in the October, 1876, number of the *American Journal of Obstetrics*, we do not feel warranted to believe that the ovulation theory of menstruation is absolutely the entire factor, nor do they establish the theory of uterine unity. So many cases are recorded where the function ceased entirely after double ovarian extirpation, that we are compelled to seek cause for menstruation in neuric origin, that the ovaries as well as the uterus are designed to complete the work as media, just as the liver is constructed to eliminate bile or the brain to evolve thought. The perversion or failure of developmental action in embryonic, fœtal or pre-pubertic life, as well as pathological change after puberty, necessarily induce those alterations of menstrual function, which, for the want of a better term, I call *menstrual neuroses.*

All menstrual derangements are symptomatic of pelvic change, either in the ovary, the oviduct, the uterus, the vagina, the vascular system or the connective tissue surrounding and binding them together.

The generative system is also supplied with plexuses of nerves of remarkable intensity, and notwithstanding we have as yet not been enabled to demonstrate the existence of trophic nerves which govern and equalize the fluxionary movements, erectility, and subsidence peculiar to the female pelvis, we are positive of their presence. Emotional and physical causes arouse molecular nerve action in the woman's pelvis, evidenced by the sudden cessation of menstruation, or an equally sudden uterine hemorrhage, or the production of pain. We constantly meet with these phenomena in an apparently healthy woman, and can only explain the causation upon the basis of *ganglionic irritation* induced by molecular disturbance in the centripetal or centrifugal system, so intimately allied with the

trophic nerves. The subsequent morbid changes of structure are easily seen in the aplasia, kakaplasia or hyperplasia revealed to the chemist or microscopist.

Menstruation is peculiarly active in developing every variety of cerebral and spinal irritation, and sometimes involves psychical life to such a degree as to impair consciousness, perception and volition.

Until we actually trace the nerves and ganglia of the female generative circle to and from the cerebro-spinal centres, we must accept facts without being able to explain them satisfactorily, although we are positive that such intimate physical relations exist. Many psychical phenomena peculiar to woman's life depend upon impressions, stimuli or irritants conveyed to the cerebro-spinal centres from the pelvis, chiefly when menstruation is absent, suppressed, defective, excessive, obstructed or retained.

Amenorrhœa is present when there is no uterus or simply a rudimentary one; when there is absence or atrophy of the ovaries; when leukaemia or chlorosis exists; and, it is symptomatic of pregnancy as well as of that change of life denominated the menopause.

Dysmenorrhea is symptomatic of difficult excretion of blood from the uterine cavity, and is found in every variety of flexion, version or stenosis of the organ.

Menorrhagia indicates a plus quantity of blood pouring from the uterine cavity in consequence of erosions, varicose vessels, ulceration, increased arterial supply, obstructed venous discharge, or general hypertrophic increase of the whole texture.

These morbid phenomena are explicable upon altogether different grounds from those enumerated as menstrual neuroses.

Neurological Correspondence.

Messrs. Editors: I take pleasure in forwarding some of the papers read before the various New York medical societies, together with a *resumé* of debates and abstracts of papers, when the entire document could not be procured.

This, I believe, embraces about all that has occurred bearing upon neurology, excepting a course of six lectures recently delivered in this city by Eugene Dupuy, M. D., on epilepsy, contractions and paralysis. These lectures are now undergoing thorough revision by their author, and will appear in a small volume, probably in the course of the summer.

Another important event in neurology was the meeting of the American Neurological Association in this city, on the 6th June; but as you already have a full account of the proceedings of that society, I have not referred to it in the following pages.

The Medico-Legal Society.

Jacob I. Miller, Esq., of the New York Bar, at a meeting of the Medico-Legal Society, read a paper on "Experts as Witnesses." The paper called out a very full discussion, and as this is a subject which interests what may be styled the higher plane of the profession, we will take the liberty of presenting pretty full extracts of both essay and debate:

It is the object of evidence, says Dr. Miller, to establish facts for judicial determination. Such evidence consists of written documents or deposition of living witnesses. These are of two kinds; ordinary witnesses, who depose to facts, and experts, or skilled witnesses, who depose to matters of science. The former tell what they know; the latter tell what they think—what they believe. The first state facts; the latter tell what the facts mean.

Experts were known to the Roman law, and during all the

history of the common law.   Indeed, notwithstanding the unsatisfactory nature of much of the expert testimony produced in our courts, it is difficult to see how the use of this class of testimony can be avoided.   Courts and juries are not presumed to be skilled in all the scientific questions which are involved in the cases which they are called upon to try.   They cannot understand the evidence produced.   They need an interpreter.   The rule in such cases is well stated in Cooper *vs.* The State, 23 Texas Rep. 336.   The court said: " It is a familiar general rule of evidence that witnesses must speak as to fact, and cannot be permitted to give their opinions.   It must be left to juries to draw inference from the facts.   But to the general rule here stated, there are exceptions.   In certain cases, certain persons may give their opinions to the jury. I cannot better state the principle on which the exceptions to the general rule repose, than by quoting the language of the judge who delivered the opinion of the court in the Jefferson Insurance Company *vs.* Corteal, reported in 7 Wend. 73.   In that case, Judge Sutherland said: ʻOn questions of science, or skill, or trade, persons of skill in those particular departments are allowed to give their opinions in evidence; but the rule is confined to cases in which, from the very nature of the subject, facts disconnected from such opinions cannot be so presented to a jury as to enable them to pass upon the question with the requisite knowledge and judgment.   Thus, a physician, in many cases, cannot so explain to a jury the cause of the death or other serious injury of an individual as to make the jury distinctly perceive the connection between the cause and the effect.   He may, therefore, express an opinion that the wound given or poison administered produced the death of the deceased, but in such case the physician must state the facts on which his opinion is founded.ʼ "

Physicians may appear in two capacities; when they depose to facts they differ in no respect from ordinary witnesses. Their testimony is then confined to their knowledge of the facts of the case.   They may also be called as experts; and it is in this capacity that this society was interested.   In both cases they are obliged to respond to a subpœna, but in the former only are they *compelled* to testify.   Either party to a

litigation is entitled by law to the testimony of any person who can enlighten the court and jury upon the facts of the case; but a physician's opinions, his knowledge of his profession, his skill, are his own, to be kept or imparted at his pleasure. While, therefore, he might be compelled to come to court, and would be punished for contempt of court if he refused, he could not be compelled to give his opinions, to act as an expert, except, perhaps, in the case of the government, which has a right to command the services of any individual owing allegiance to it. His knowledge is his own, and he parts with it upon his own terms.

The position of the medical expert, therefore, is one of dignity. His appearance is voluntary. He speaks to science. He produces the connecting link between cause and effect; between the act of the accused and the allegations made against him. As the jury are usually ignorant of the matters about which he speaks, the physician should be a person of the highest skill and of the strictest integrity.

Properly speaking, an expert is not a witness for either side. He does not depose to the facts which form the basis of the judgment. He is not concerned with the result. He is not to answer the question is he guilty or not guilty?—is he sane or insane? These are for the jury. He is to say whether, assuming certain facts to be shown, they, as a matter of science, would indicate what is necessary to meet the allegations against the accused. If the jury does not agree with the expert, there is no conflict. No reflection is cast upon him. But he does not always remember this; *he sometimes acts as if he were a retained witness, a testifying advocate.*

Expert testimony should be correct. Otherwise disaster befalls not only the parties to the cause, but the whole medical profession. In so marked a degree has this appeared in some cases that judges have not hesitated to tell the jury that the expert testimony was wholly unreliable, and that they need not be guided by it. It would not be difficult to raise a doubt about the sanity of any man in New York. Our safeguard is the right of the opposing counsel to inquire into the qualifications of the person offering himself as an expert. This is not a full protection, for the court itself does not know

or is not presumed to be skilled in any science, except that of the law. He is, to a large extent, dependent upon the witness himself respecting his qualifications. If the court does his duty, he will not allow a physician to testify as an expert, unless he is especially qualified by experience to speak upon the subject of science under consideration. Because a man is a physician, he will not be allowed to testify to everything which comes within the scope of medicine.

After citing the opinions of many learned judges in various cases, the writer draws the conclusion as to the qualifications of an expert, and says that he who would testify as an expert should be peculiarly fitted for the work of giving opinions on the subjects and have more than ordinary knowledge on the subject. No precise limits as to knowledge can be set; for as science is constantly progressing, the extraordinary knowledge of to-day becomes the ordinary knowledge of to-morrow. A few advance spirits make discoveries which soon become the common property of the world. The standard of the expert must change with the progress of his science. But as all sciences have progressed to such great heights, it is obvious that any one who would extend his knowledge beyond these limits, must be content with a narrower range. And, hence, the sphere of the expert in any one science is proportionally narrowed. The tendency is to specializing. No one physician expects to master all branches of his profession ; and hence, no one physician can expect to act as an expert in every class of cases which come under the cognizance of members of his profession.

While the court can inquire somewhat into the extent of the knowledge and experience of the witness, offering himself as an expert, it cannot discriminate between the various schools of medicine.

The reason is obvious; the court is not presumed to know anything about medicine or surgery. Therefore, when a medical witness offers himself and declares his ability to testify and shows that he has had the requisite experience, the court cannot reject him. He must be allowed to give his opinions. His knowledge can be tested in a cross-examination; and in all cases he must state the facts upon which his opinions are based.

Professional books are not competent evidences. They are treatises upon certain subjects stating general principles, but having no relation to the particular case under consideration. The jury want an explanation of that case; professional books cannot give it. A witness, however, may refer to such books as containing the generally accepted opinions of the profession, and as sustaining him, (generally,) but cannot read them as part of his evidence. The author not being present, or under oath, cannot be cross-examined and his knowledge tested.

Medical experts cannot be asked questions, as experts, which can be answered equally as well by other persons. If the jury can know the questions as well as he, the expert is not needed. Hence, whatever lies within men's general knowledge, is not a subject for an expert. See Woodin vs. The People; also, Van Zant vs. The Mutual Life Ins. Co., Kennedy, 39 N. Y., 255.

A witness may not be asked whether the accused was sane or insane at the time when he is alleged to have committed a crime, if there is a conflict as to the fact. He may not be asked whether a testator had capacity to make a will; a but hypothetical case may be put to him, and his opinion asked thereon. The jury will see how far then it corresponds with the case in hand, and, therefore, how far their judgment shall be guided by it. Wendell vs. the Mayor of Troy, 39 Barb., 338; People vs. Lake, N. Y., 358; Goodrich vs. The People, 3 Park. 622; White vs. Bailey, 10 Mich., 159; Shelton vs. The State, 34 Texas, 666; Woodbury vs. Obear, 7 Gray, 469.

If, however, the physician has attended the accused, and had frequent opportunities to witness his conduct and converse with him, then he can give an opinion as to his sanity or insanity, based upon such observation. And after an opinion has been given, a cross-examination will be allowed upon hypothetical questions predicable of the facts then proved in the case, in order to test the correctness of the opinions of the witnesses. People vs. State, 12 N. Y., 358.

In order to be an expert in the true sense of the word, the witness must have had experience in the particular science under investigation. Mere opportunities for observation are not sufficient. He may not have improved the opportunity. He may

have considered the profits which should arise from dealing in certain articles, rather than their scientific value. Page vs. Parlor, N. H. Rep., 59. The court, after stating in what cases witnesses are allowed to give their opinions, adds:

"It must, however, be first shown that they are skilled or scientific men, or at least that they have superior *actual* skill or scientific knowledge in relation to the question, before their opinions can be competent. Mere *opportunity for observation is not sufficient.*"

We have thus seen what are the qualifications of medical witnesses, and what is the sphere of their duties. It would almost seem as if their testimony should produce more satisfactory results. And yet, within the last ten years, trials have taken place in this and neighboring States, in which the medical witnesses did more harm than good, and more to mislead the jury than to instruct them. Why is this so? Incidentally two causes have been mentioned: (1), incapacity; (2), partiality of witnesses. Some knew no better; some did not wish to know better. But there are cases wherein skillful and well-meaning men fail to meet the demands of the case. It is not because they do not understand the general subject under consideration; not because they are unwilling to do their duty, but because they fail to comprehend the demands of the case, and to apply correctly the knowledge which they possess. This most frequently happens in cases of insanity. This is the question for the jury to decide. But the jury are not presumed to know enough about insanity to judge correctly in reference to it. They do not understand the evidence. They do not comprehend its scientific bearing. The aid of the medical witness is invoked. He is expected to tell them what? What his notion of sanity is? Oh, no! He may be one of those people who think that everybody is more or less insane; that there are no perfectly sound bodies—but all contain within them the seeds of disease and death. So there are no sound minds. Or, he may think that the legal test is not a sound one, and wishes to adopt one more scientifically correct, as he thinks. No! He is expected to use the word insane as the law uses it, and in the same sense. That is "solely whether he was capable of having and did

have a criminal intent. If he had it punishes him; if not, it holds him dispunishable, and it applies a test by which the jury is to ascertain whether the accused be so far insane as to be irresponsible. That test is the capacity to distinguish between right, and wrong as to the particular act with which the accused is charged. If he understood the nature of the act, if he knows his act is criminal, and that if he does it he will do wrong, and deserve punishment, then in the judgment of the law he has a criminal intent, and is not so far insane as to be exempt from responsibility. On the other hand, if he is under such delusion as not to understand the nature of his act, or if he has not sufficient memory and reason and judgment to know that he is doing wrong, or not sufficient conscience to discern that his act is criminal and deserving punishment, then he is not responsible. U. S. *vs.* McCne, 1 Curtis, Reps. 1.

If the witness does not use the word in that sense, he misleads the jury, and causes them to err. Just here has been the mistake which medical witnesses have so frequently made; and after the prisoner has been acquitted, have been astonished at their own acts. And then they talk of the "criminal insane" and the "insane criminal!" If the medical witnesses would appreciate exactly the question which they were called upon to answer, and answer it honestly and conscientiously, much of the confusion which hangs about this subject would be removed. They would not startle at their own conclusions, or aid in securing results which shock the common sense. Let them, before they testify, inquire what the law demands of them, and strive to meet the law's requirements, and not palm off on the court and jury what Maudsley thinks or Ray thinks; or perhaps what the witness thinks insanity ought to be. But we have often heard it said here by some physicians, that in their judgment the legal test of insanity is not scientifically correct, and they cannot adopt it—that they have a better test of their own. If they are unwilling to do what the law requires, they have an alternative; they need not testify at all. They will not be compelled to give their opinions. The court does not want their peculiar notions. If the test is not correct apply to the legislature to

have it changed. But adopting another test is not changing—it is defeating the law.

From what has been said, it would appear that the office of medical expert is one of dignity and importance, for it relates to questions of medical science and skill. If expert testimony is not to become a synonym for useless testimony, more care must be exercised on the part of those who offer themselves as interpreters of evidence. They must have their opinion first hand. They must, in short, be what they profess to be, skilled witnesses, skilled by experience.

At a late meeting of the Medico-Legal Society, a communication was received from Dr. John H. Packard, of Philadelphia, chairman of the obstetrical society of that city, asking its experience of the law extending the principle of confidential communications to physicians, and whether, in the opinion of that body, it is advisable for their brethren of Philadelphia and the State of Pennsylvania to take steps to secure the passage of a like act.

The question was referred to the permanent commission for consideration, who reported that the experience of the Medico-Legal Society, and it is believed, of the legal and medical profession of this state, touching the working and efforts of the law referred to, proves the wisdom of the law, and commends it to their earnest approval; and that an affirmative answer should be given to the inquiry whether it is advisable that steps should be taken to secure the adoption of a like law in the state referred to.

Regarding the statutory provision in New York, they say that since the revision of the statutes, in the year 1828, the following has remained unaltered:

"No person duly authorized to practise physic or surgery, shall be allowed to disclose any information which he may have acquired in attending any patient, in a professional character, and which information was necessary to enable him to prescribe for such patient as a physician, or to do any act for him as a surgeon."

The rulings of the court upon the meaning of this language, have been uniform, but the might of judicious authority seems to be that it is the priviledge of the patient or party to the

action, and not of the physician or witness, and that if no objection be interposed by the party, the physician may be compelled to testify.

But the protection and privilege it affords are deemed necessary to its full discharge and to that perfect confidence which should exist between physician and patient, needed in the treatment of maladies, either bodily or mental, and that the law finds an equally cogent justification in considerations of public decency, and in the preservation of the peace and happiness of families.

The exposures prevented by the statute would generally cause only pain, with no corresponding good; and over all such cases, the veil of confidence, protected by law, should be drawn.

A debate arose after the presentation of the report, during which one of the legal gentlemen present, Mr. Riddle, remarked that the rule in New York concerning information obtained by physicians and surgeons in the course of professional attendance on a patient, and which was necessary properly to prescribe for or treat the latter, is that such information is confidential or privileged, but the privilege is that of the patient, who may waive it and allow the physician or surgeon to disclose the information on the witness stand. This waiver may be implied as well as expressed. If the patient is present, or represented at the examination of the physician or surgeon, and has the right to be heard but makes no objection, he is deemed to have waived his privilege, and the evidence is received and considered; but if he is not present or represented, then the physician or surgeon cannot disclose the information, because the patient's absence cannot be construed into a waiver of this privilege. On the probate of a will, the physician was permitted to disclose communications made to him by the deceased in his illness, to show his want of testamentary capacity, on the ground that the person whose privilege was this confidential communication was dead.

In view of the importance of the matter discussed, we would respectfully suggest that societies of states not already having such a law, look into this question, and take steps looking towards the passage of a law similar in its intent to that passed by this state in 1828, above quoted.

At the meeting of the Medico-Legal Society, held May 2d, 1877, Dr. W. G. Stevenson, of Poughkeepsie, N. Y., read an elaborate and valuable paper on " CRIMINALITY," of which the following is a very full abstract:

The evolution of humanity ever has presented problems to be solved, and phenomena to be interpreted; and eager minds in all ages, have striven to obtain the key which would unlock the mystic laws of creation, and reveal to man the rule which directs his actions and interprets his responsibilities. Ignorant alike of his origin and destiny; unable to comprehend the relation he sustains to external nature or to interpret her phenomena; impotent to resist the influence of the unseen but mighty forces which constantly displayed themselves in his presence, man recoiled in terror, and in abject fear prostrated himself before the power which exhibited itself in such majestic grandeur, but with such terrible reality. He ascribed to these blind forces personalities, and supplicated them as Gods, to gain whose smiles or shun whose frowns an ignorant humanity propitiated by offerings and sacrifices.

Thus was erected the shadowy structure of mythology, whose foundation stone was ignorance, and whose frame-work was composed of the capricious imaginations of an infant age.

But as by observation and experience, man learned to know the truth and thereby to dissipate the superstition of the past, so, by the accretion of knowledge, he began slowly but surely the demolition of this delusive structure, and it tottered to its fall. Investigation has abundantly demonstrated, as a fact of primary importance, that nerve tissue is essential to the production of at least the higher phenomena of animal life; and that these phenomena vary in degree and character in proportion to nervous development.

From the lowly forms of animal life, whose nerve centres produce only reflex action in its simplest expression, upward through every series of progressive development, with the super-additions of nerve centres which add to the complexity of nerve function, there is accumulated evidence that every such additional centre but furnishes a source of new power, potentially capable, within certain limits, of modifying the action of subordinate centres, yet, nevertheless, incapable of wholly negativing their specified functions.

Thus, the organic nerve-cell presides over nutrition, so far as the simple vegetative growth of an animal is concerned, its action is all sufficient; but, in order to connect simple organic growth with the phenomena of animal life, an additional centre force is needed, and there are evolved the reflex centres of the spinal cord which unite vegetative growth with animal action. The sensory centres being next evolved, there is the possibility of sensory, added to organic and reflex action.

But yet there is no consciousness of life, of motion or sensation. Evolution goes on, and the brain, the conscious centre of that energy which presides with such mysterious power over the thoughts and actions of man, is produced.

To fully understand the functions of the brain, necessitates a knowledge of the functions of the spinal and sensory centres; for there are acts performed by man which bear the semblance of conscious volition, and yet, when correctly interpreted, give evidence only of the automaton action of the reflex centres of the spinal cord and sensorium.

Although a few cases are known in which limited nerve action resulted without the existence of the proper nerve elements, as fibres and cells with their prolongations, yet it is an accepted fact that nerve force only exists within the boundaries of the nervous structure, and that this nerve force is generated not in the cells, but also in the fibres, as even when rest, oxygen or strychnine restores nervous energy after its complete exhaustion in a limb removed. Nerve force is not generated by any volitional effort.

As magnetic, frictional and statical electricity are only different forms of expression of the same energy, so a simple impression, sensation, ideation, emotion and volition are but different forms and expressions of the same nerve force, and come from the special molecular structure of the organ through which they are made manifest. This unity of nerve force precludes its intense expression in more than one way at a time. So that if there is great bodily fatigue, mental work is impossible, and *vice versa*; neither can volition hold full rule in the presence of deep emotion.

Nerve energy is transformed into motion as evidenced in muscular action. It is also transformed into heat, but it is

not known whether this is an immediate or secondary result. There are a few instances recorded which seem to show its transformation into light, and it is well known that in certain animals electricity is the result of its metamorphosis.

From these data, the conclusion seems authorized that at least a partial correlation exists between the physical forces and the energy resulting from nerve action,—partial correlation—because, while the evidence may permit the conclusion that nerve force is transformed into motion, heat, light and electricity, it does not authorize the conclusion that these can be reconverted into nerve force.

It is important to remember the fact that the character of nervous and mental phenomena is determined by the condition of nerve centres, whether the condition is one of development of the centres themselves, or of modification by disease, or by foreign substances.

The existence of the spinal cord alone predicates the possible existence of automotor reflex action, which, though unconscious, gives evidence of the use of means to a special end. Add to the spinal centres the medulla oblongata, and there results the involuntary and unconscious co-ordinate muscular movements of respiration, swallowing, coughing, and simple exclamation; conscious sensations of pleasure and pain, of taste and hearing, come by the addition of the annular protuberance, while the tubercular quadrigemina alone give visual power; the cerebellum co-ordinates the muscular movements of the eye, while the cerebrum not only determines the nature of the mental life, but it alone is able to bring varied sensations of nervous actions within the domain of consciousness.

The existence of these several centres is therefore necessary in order that the many acts of human life may be performed, and as a necessary corollary, it is found that in proportion as any centre is undeveloped, diseased or modified, nervous or mental action will be changed, limited or cease to exist.

That mind is influenced by and dependant upon the physical condition of the brain, is again evident, when it is remembered the effect caused by poisoned blood on all mental expressions.

Hashisch, opium and alcohol, for example, weaken the will,

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exalt the automatic action of the brain, disturb perception, exaggerate self-consciousness, distort the emotion, dethrones reason and cause moral terpitude.

Diminish the normal blood supply to the brain, and the mind instantly changes its character; restore the needed amount and the mind promptly responds to the altered condition.

Old age, injury, fatigue—anything which impairs the normal nutritive supply of the nervous centres of thought, directly modify intellectual and moral manifestations, and the conclusion is irresistible that every psychical manifestation has a physical antecedent; that cause and effect are as certainly established within the realm of mind as of matter.

To explain the how and the why of mental action as a result of physical conditions, is impossible. So, too, is it to explain how electricity comes from the union of metals with an acid; or how life springs from a seed; or perfume from a flower. They are ultimate facts, and as such they are beyond the boundaries of explanation. But however complex the problem, the fact remains that definite conditions invariably produce definite results; and there is no more reason why a divorce should be given in the one case than in the other.

In solving the problem of criminality, it was the purpose of the author to rely chiefly upon the evidence of nervous physiology, and while he would not be able to present in detail the many facts at hand relative to mental phenomena, he would nevertheless endeavor to cite such as may be needed to sustain the position that *criminality is a neurosis*, and originating in either an inherited or an acquired condition of the brain; whether it be a condition of positive disease, or of mere non-development of certain faculties, the existence of what would preclude the possibility of crime.

The doctor then considered the question of the equality of man in things which pertain to his acts in life, for if equality exists, it is necessarily evident that we may the more easily formulate a rule by which to measure human action; whereas, if equality does not exist, it is apparent that we must measure each person's acts by some standard of his own, with special reference to his individual organization and surroundings, and

come to a negative conclusion, both in a physical and intellectual sense. Each individual of the human race possesses a certain brain organization which will admit of intellectual *and moral* development to a certain degree; but no discipline, no method of education, no power of the will can advance it beyond this limit! Thus far and no farther, is the dictum of creative energy!

With respect to the brain, one is capable of receiving and assimilating certain impressions of a specific nature by which a definite character of mind is formed, while another is so organized as to receive and assimilate impressions different in quantity and quality; and there results a mind of peculiar endowment and force. And here it is important to observe that the reception of impressions by the brain does not of necessity predicate the nature of their assimilation; for as the different elements of food to the general system are selected and assimilated by different tissues, and produce bone, muscle, nerve and blood, so the nature of the assimilation of mental impressions varies according to the potential nature of the brain tissue, whose duty is to transform impressions into ideas and modes of thought.

Individuals differ from each other intellectually, because of the potential character of brain tissue which in one gives great inborn capacity to assimilate impressions, while others are unable because of an inferior or weaker inborn capacity of brain structure to receive more than the simpler impressions from life's experience, and thus the mode of thought and action correspond with such statical energy of organization.

Nations and peoples differ from one another in their ideas of law and civil government, religion and morals; and these differences have existed from the earliest monumental history of man, and as strongly mark the present diversity of races as do the color of the skin or form of the skull. These intellectual variations result from the different brain capacities of the several species, and thereby absolutely preclude an intellectual equality except through amalgamation by which the superiority of one is depressed while the inferiority of the other is elevated—until a temporary equilibrium is produced.

Considering that sufficient proof is offered that these dif-

ferences do exist, the author discussed at length how these differences were transmitted from parent to offspring, in which he indulged in quite an extended journey through heredity in its influence in forming the character of instinct, perception, intellect and will; and also the interesting question as to its influence over the appetites and passions including all the moral impulses, either natural or morbid, of man's nature, and the pathological conditions to which this physical and mental life are subject.

This brings us into the immediate presence of the criminal, and evokes an analysis of those physical and mental anteced-ents which give the bias to his individual character, and which so irresistibly impel him to crime.

Testimony is conclusive in establishing the heredity of many neurotic diseases, such as a simple nervous tempera-ment; neuralgia, chorea, hysteria, hypochondriasis, inebriety, *criminality*, insanity, whether perceptional, emotional, voli-tional or maniacal, or as general paralysis, idiocy or dementia.

Assuredly it is a fact which none deny that the offspring of nervous, insane, epileptic, inebriate, consumptive, scrofulous or criminal parents, are more liable to develop some special form of disease or character, than those whose parents are free from any vitiating cause. They have organizations which render it not only more possible but more probable for ancestral vice to appear, but the special form this vice may assume is not necessarily determined by the parent.

Many neurotic diseases, like physical forces, are correlatives of each other. They are metamorphosed oftentimes in their transmission, so that what was neuralgia in the parent is chorea, or hysteria in the offspring, or chorea or hysteria may be transformed into epilepsy and thus into insanity, and the insanity may in a third generation develop phthisis, dipso-mania or criminality, or conversely criminality or drunken-ness may engender epilepsy or madness; and thus through the entire category of nervous manifestations testimony is added to sustain the fact that cause and effect are as invariable in the intellectual and moral as in the physical world; and that through heredity the physical, intellectual and moral forces of the ancestor largely determine the physical intellect-ual and moral forces of the offspring.

In this connection, it is important to remember that the conditions or factors which so influence character may lie dormant in one generation, existing only as potential elements which may spring into activity at any time when influenced by favorable surroundings.

There is a diminished stability of organization; an equilibrium more easily disturbed, and which, when moved from its balance, is less liable and less capable of resuming its original position.

A neurosis existed in the family line of the Cæsars which developed epilepsy in Julius Cæsar; made a valetudinarian of his nephew, the emperor Augustus; disfigured with disease the face of Tiberius; caused Caligula to be pale, sleepless, and delirious, and gave Claudius a weak physical organization, while the excessive cruelty of Nero may find some excuse in the fact that the morbid nerves of the Cæsars had in him caused insanity.

Thus is seen the immediate and direct results of a bad inheritance; and while we cannot foresee the type of degeneration which will follow ancestral vice of character, yet we can prophetically declare that some form of physical, intellectual or moral degeneration will come from it and thereby determine the possibility of crime.

Accepting then the physiological fact that conscious mind and brain are intimately united as cause and effect, and that the character of the mind depends primarily upon the nature and degree of molecular brain development, which development is measured according to the inherited capacity of the brain tissue, which capacity differs in each and every person, we must believe that each individual has through his inherited organization a special power of actual and possible brain energy, and that in order to justly judge of his actions and his responsibilities, his own individual organism must furnish the only reliable standard by which to determine facts.

By this rule only can the demands of justice be satisfied in dealing with that large class of humanity known as criminals, whose deeds have furnished so many pages of the world's history.

To the question, "why is one man a criminal?" the answer

is, for the same reason that another is a moralist, or an honest
law-abiding citizen. Either an inherited organization, having
morbid antecedents gives the bias to development and action,
or a constitution, originally well endowed, is so modified by
morbid influences as to render possible, subsequent moral
alienation.

Criminals are such, either because they inherit a brain
structure potentially incapable of generating moral faculties,
or through morbid influences the development of mind does
not evolve sufficient moral strength to guide and control the
lower propensities of man's nature.

In either case, however, criminals are generally speaking,
diseased elements or members of the body politic, which are
born of it, belong to it, and are of necessity, correlated with
it in every stage of human evolution. Positively diseased in
that they bear evidence of bodily infirmity, neurotic disease
largely predominating either in the milder types or as epi-
lepsy, inebriety and insanity. These together with scrofula,
and tubercular development, determine with certainty the fact
of bodily and nervous degeneration.

Negatively this position is strengthened by the fact that,
being perfectly conscious of punishment received for past
offenses as well as a certain assurance that retributive justice
will be executed for every offense in the future, they yet close
their eyes to all results and apparently without dread of the
coming day of wrath, rush wildly, heedlessly, remorsely, into
the seething vortex of criminality, and pause not until the
strong arm of the law interposes for the protection of human
society. But with all this, there is moral obliquity—a per-
versity of thought and desire with violent passions, which
being uncontrolled by higher moral faculties (not because the
perversities are greater than in other men, but because the
moral force is less) there result immoral, criminal acts, which
are correct exponents of the organic cause by which and
through which they alone exist.

It is no more difficult to accept the belief that brain devel-
opment in the evolution of mind may stop short of com-
pletely unfolding the moral faculties, than it is to believe that
it may, as it certainly does in many cases, stop before high

intellectuality is attained, and the moral sense being the highest as well as the last in the gradation of mental evolution, it is the first to exhibit evidences of degeneration, which unless checked or neutralized by some controlling influence, increases in each succeeding generation, until at length, the morbid action culminates in its positive and irreparable destruction.

And what then?

With only a feeble moral sense, or perhaps, with its total obliteration, what incentive is there in the human mind to shun wrong doing?

None at all! but man is left to battle with the storms and tempests of life without a helm, or pilot to guide his frail bark to a port of safety; but tossed about upon the angry billows of temptation, is it indeed strange if, by and by, his craft is stranded on the shoals of petty crimes, or goes down to destruction in the dark cold gloom of murder?

Evidently then, in some instances at least, criminality is not the immediate offspring of uncontrolled or vicious passion, or impulse which might be restrained by an effort of the will; but it represents a symptom of degeneration of the nerve centres, and the perpetrated crime itself, is but an outward expression of the inner condition of man's development, akin to that condition of mind which generates low and vulgar ideas, having their counterpart in brutal instincts and words.

In truth, it is undoubtedly connected with diseased or non-developed nerve structure in the higher centres, as in epilepsy, etc., which may be to a degree, and in some persons is remedied or modified by proper treatment and education; but when, as is too often the case, it results from a vice of organization, incapacitated by a bad descent, from evolving sufficient sensibility, it then becomes an irresistible fate. The cause is in past physical antecedents, and as a sequence of this cause the criminal takes his place in human society. Yes; he is a legitimate member of society—in that his individuality sprang from, and is correlated with ancestral taints of character over which he has no control, and although free under civil law to choose his following as a member of society, to shun its wrongs and hold its virtues, yet being bound through heredity by the forces of his organization, he occupies a natural

position among the vast throng of mortals whose individual acts, whether of virtue or of vice, when aggregated, constitute the history of humanity.

Is it not then an important question, to inquire relative to man's acts in life, and does not conviction follow after duly reflecting upon the complex nature of the problem to be solved and the innumerable number and variety of elements, which, as factors, make up the grand total of human life, that ere judgment is given upon any case of criminality, passion and prejudice should be eliminated and manhood exalted by giving reason empire over emotion, holding steadily and firmly the scales of justice which, on the one side, holds the act committed, and on the other, the organization which impelled the crime, and only by proper balancing of which, can right be vindicated or justice measured to man?

Criminals belong to us as a legitimate inheritance, and are exponents of our civilization; their crimes, like insanity, varying and assuming different forms under different stages of social evolution.

The doctrine that crime can be diminished through fear of punishment, is erroneous, as criminal statistics demonstrate. Were crime the result of personal spontaneity, this might be true, but since it springs from the secret forces of organization, it is but a symptom of moral alienation, resulting from an inherited vice of character, over the origin of which the possessor had no control. How can he, in the name of common sense, do differently than he does? Nay; he cannot; his acts are as the "chastity of impotency, or the silence of the mute— a necessity." He who has no moral faculty, can not be held responsible for moral wrong-doing; and he who is morally dead through the vice of an inherited nature, is absolutely beyond the jurisprudence of penal retribution.

From all this, the conclusion is drawn that man, in his acts, is not beyond the realm of law, and that his responsibilities in life are to be measured according to the standard of his personal organization, the perfect knowledge of which gives the key which will unlock and interpret the mysteries of human life and action.

THE NEUROLOGICAL SOCIETY.

The annual meeting of the society was held on the evening of Monday, May 7th, at which the newly-elected officers assumed their duties. They are as follows:

President, Edward C. Seguin; *First Vice President*, Eugene Dupuy; *Second Vice President*, Edward C. Spitzka; *Secretary*, Geo. W. Wells; *Corresponding Secretary*, Max Herzog; *Treasurer*, Edward C. Harwood; *Counsellors*, Prof. W. A. Hammond, Prof. J. C. Darby, Drs. J. C. Peters, Clinton Wagner, J. G. Kiernan.

After preliminary business, Dr. Peters retired from the office of president, and delivered " the reins of government " in the hands of his successor in the following terms:

VALEDICTORY ADDRESS OF DR. J. C. PETERS.

GENTLEMEN: In resigning the post of president of this society to my distinguished successor, I wish, more formally than I have yet done, to return you my sincere thanks for the kind partiality which you have always shown me, and for the generous and able manner in which you have always seconded my efforts for the welfare of this association, and have filled up all my shortcomings.

It is unnecessary for me to bespeak for Dr. Seguin the continuance of the same kindness, and an equally active co operation with him, for you know that he is willing to do all in his power to thoroughly promote your prosperity and to elevate the standard of he proceedings of this society.

I need not impress upon you the great importance of societies, devoted solely to the advancement of topics of great special interest, and none can be more important than a banded brotherhood, occupied exclusively in the study of diseases of the nervous system.

These comparatively small special societies accomplish more scientific and better practical work than those of more general scope.

We need only look to the high position of the Pathological and Opthalmological, the Gynæcological, the Obstetrical, the Dermatological, the Medico-legal, the Public health, the Laryngological societies, etc., etc., in order to induce us to strain every

nerve to maintain our well-earned position fully abreast of, or even well in advance of, some of them.

The composition of this society has a broader and higher scope than that of any other special society, except, perhaps, the Pathological society. A large proportion of the practice of every physician is laid among diseases of the nervous system, and it has been our endeavor not to limit our membership to mere neurologists, but to include many able general practitioners, who are familiar with the nervous diseases of the heart, lungs, liver, gastro-intestinal and urino-genital organs, etc., as well as those who have made a life study of the brain, nerves and sympathetic system. They can bring a large quota of experience in diseases of the nervous system, which rarely falls to the lot of the pure neurologist; and they can both be vastly benefited, as I have had ample proof, by close contact with and free discussion with each other.

It was for this reason, I presume, that I, whose highest ambition has been to be a pure physician and therapeutist, have been sandwiched in the presidential chair between two of the most widely known neurologists that the country has ever produced.

I am most happy to hand over this society to my successor, not only in a prosperous condition, but with a great vista of usefulness clearly opening up before it, and which I am sure that Dr. Seguin will both lengthen, widen and embellish.

Again thanking you, I gladly resign the chair unto the able man we have so wisely selected to fill it.

Dr. Seguin in accepting the responsibilities and trials of a successful society, spoke as follows:

Remarks of E. C. Seguin, M. D.

Gentlemen: It is a pleasant custom that a newly-elected presiding officer should be allowed to stand face to face with those who have honored him with their votes, and return thanks. In my case this custom is the more appropriate because I feel the need of assuring you that your action in this matter was quite unexpected, and I am sure very undeserved by anything I have done.

With your friendly assistance, I shall endeavor to faithfully

and efficiently discharge the duties of the office to which you
have assigned me.

Our retiring president, Dr. Peters, has just ably reviewed
the labor done by the society during the past year, and the
record is one with which you may well be satisfied. But
much as you have done, there yet remains much more to be
achieved.

We are physicians who, under the name of Neurologists,
cultivate more especially one of the many fair fields in the
great domain of medical sciences; and while we probably all
lay claim to being physicians in the broad sense of the word,
we are justly proud of the subject we have undertaken to study
as exhaustively as possible.

This field of Neurology is broad and rich, and lies next to
other equally valuable parts of the same great domain. We
are thus in intimate association with Psychology, and even
Moral Philosophy, with Forensic Medicine, with Public Hy-
giene, with Ophthalmology, Gynecology, etc.

The soil of our chosen field is that noblest part of animals,
the Nervous System; the system which serves more than any
other to mark the progress of evolution in the zoological series,
and whose surpassing physiological attributes make man what
he is. And under this general title of nervous system, we not
only include the anatomical structure and common physio-
logical properties of the nerves, the spinal cord, and the brain,
but we even bring the finer functions of the cerebrum—the so-
called Psyche or soul—under the edge of our analysis. We
study, with our various methods, complex moral as well as
simpler physiological perversions.

To cultivate such a field, one which has already been the
theatre of great and brilliantly productive labor, we need
skilled workmen; men not only possessed of book-learning,
keen in criticism, and fluent in speech, but also men expert in
the use of the instruments with which the field is to be
cultivated.

The instruments I refer to are clinical study, pathological
anatomy, anatomical investigation, and physiological experi-
mentation. A furrow in our field, to be made productive,
must be fully tilled by these four great agencies, and in this

connection I may be allowed to lay stress upon the necessity in which we are of aiding one another, partly because few of us can hope to attain great skill in the use of more than one or two of these instruments, and because, as it seems to me, our work is all the better, yet not less creditable, for being trimmed and perfected by other laborers. I see before me, in the ranks of the Society, the disciplined minds and the skilled hands needed in the field of neurological medicine. You have among you microscopists, experimental physiologists, men apt in clinical observation, medico-legal experts, keen critics, and able writers. By individual labor, each in your special branch, you must produce much valuable matter, but by conjoined efforts, in proper associations, you may really advance medical science, and give this Society an honorable reputation at home and abroad.

At the risk of imposing on your patience I shall now briefly refer to a few of what I take to be the great neurological questions of the day, or, in pursuance of my allegory, the great and fertile furrows in our favorite field.

It will be generally admitted, I think, that a subject just now of surprising interest, is that of the localization of psycho-motor functions in limited parts of the cortex of the cerebrum. Although only six years have elapsed since Fritsch and Hitzig first showed that the cerebral cortex was excitable, and only four years since Ferrier began his remarkable experiments and publications on the so-called motor centres of the brain, the subject has assumed vast importance, and already commands a large literature. Experimental researches pro et con, have been published in all civilized countries; and among these investigations, I may mention Dalton's and Putnam's in favor of Ferrier's hypothesis, and Dupuy's against it, as American contributions. In the clinical and pathological fields, the dispute is very active, and we find such men as Brown-Séquard annd Charcot opposed to one another, at first in debate in the Socíete de Biologie, of Paris, later in the journals; the former great authority publishing a long series of articles in the London *Lancet*, tending to overthrow the hypothesis of the existence of psycho-motor centres in the cerebral cortex, while Prof. Charcot and his pupils, and numerous observers in various countries,

are constantly sending to the medical press well-studied medical and surgical cases, with post-mortem examinations in support of the theory. It strikes me that nothing could be more creditable to this society, than that it should furnish during the coming year, experimental and pathological data bearing upon this question; all worked out with the exactness and fulness demanded by positive medicine.

Another question, which, to my mind is just now of importance, is that respecting the setting up of cerebral symptoms by peripheral or vascular disease, and by diathetic conditions. The problem resolves itself into a number of serious questions, each one of which is a fruitful subject for work. Allow me to enumerate some of them:

How frequently and by what mechanism do errors of refraction and insufficiency of ocular muscles produce headache, pressure and pain in the cervical and occipital regions, dizziness, etc.? Drs. Weir Mitchell and Thompson, of Philadelphia, have laid the foundation for a thorough study of this matter.

How frequently, and in what manner does utero-ovarian disease set up back-ache, cervical spinal neuralgia, obscure cerebral symptoms, and even melancholia?

How often and by what mechanism do irritations in the male genital organs produce paraplegiform symptoms, hysteroid symptoms, spinal neuralgia, etc.?

How frequently do such diathetic conditions as oxaluria, or lithæmia give rise to headache, dizziness, insomnia, indisposition to mental exertion, extreme depression of spirits?

What is the precise relation between such symptoms as have been enumerated and general long-continued mal-nutrition, or over-waste?

I attach so much importance to these questions because if we solve them, we shall almost have the key to the more general problem whether hyperæmia is or is not a frequent proximate pathological cause of headache, dizziness, sense of pressure, or emptiness or fullness in the head, blurring of sight, indisposition to mental exertion, nervousness; in short the various symptoms which some patients not inappropriately designate in a group as "bad head." I am afraid that at the present time hundreds of cases, which are really very complex in pa-

thology and ætiology, are grouped together under the (to me) somewhat crude designations of hyperæmia and anæmia of the brain, and subjected to a treatment which is designed by deduction from certain hypothetical views instead of by induction from selected experience, and from well-performed experiments. I trust that the subject will be brought up before the society that we may do something to throw light upon these obscure morbid conditions, which are so common in our American life.

Naming a third great subject will suffice. I refer to one in which the society has already taken great interest; *viz.*, the pathological anatomy of mental diseases so-called. There are several gentlemen before me, well fitted by special training to continue their exact and scientific clinical and pathological researches into insanity, and I hope they will frequently lay the results of their labor before the society.

Once again, gentlemen, I tender you my sincere thanks for the honor you have done me.

At a previous meeting of the Society, Dr. Jas G. Kiernan, of N. York City Asylum for the Insane, Ward's Island, read a paper on Katatonia, a new form of insanity with peculiar motor symptoms.

At this meeting the paper was discussed.

As a preliminary, Dr. Kiernan gave a full resumé of his paper, containing the points of the paper, as follows:

REMARKS OF DR. JAS. G. KIERNAN.

The disease is characterized by an irregularity or, as it has been called, an insanity of tension, whence the name Katatonia. It is of a cyclical character; maniacal, melancholic and cataleptoidal conditions alternately, with more or less perfect convulsive attacks, pathetic delusions of grandeur, voluntary rhythmical movements of the fingers, and a tendency to talk and act theatrically.

The patients attacked by the disease are under the age of thirty, and inherit a strumous diathesis; they are, comparatively speaking, temperate in the use of stimulants, but are, as a rule, addicted to masturbation or sexual excess. The disease was first clinically demarcated by Dr. Kahlbaum, of Gorlitz,

Prussia, but two years before, Meynert described it "as a peculiar form of melancholia attonita, characterized by a series of fluxionary excitations, (toned down by coexistent cerebral pressure,) microscopic exudations, ventricular dropsy and perhaps premature ossification of the sutures. From these would result forced and theatrical activities on the part of the patient.

"The convulsive element representing the irritative factors getting the upper hand, the cataleptoid the triumph of the depressing factors.

"The ideas of grandeur following upon stupor were the results of ideas previously caused by fluxionary conditions." These views of Meynert were confirmed by the macro- and microscopical examination of a case coming under observation.

The patient whose spinal cord and brain were examined was thirty years of age, unmarried, moderate in the use of stimulants. He had melancholia, manaical, and cataleptoid alternations, made well marked rhythmical movements, and had pathetic delusions of grandeur.

The disease had existed for two years, and the patient died of phthisis and tubercular enteritis. The post mortem and microscopical examination was made with the assistance of Dr. Spitzka.

Lungs were found to be tubercular, as also were the intestines and peritoneum; other abdominal and thoracic organs normal; head-scalp thin; cranium thick and not adherent to the dura mater, which was normal. Subarachnoid space filled with a number of brownish flakes of a gelatinous consistency, most of these drained away with cerebro spinal fluid, but a few were quite firmly adherant to the underlying pia: minute blackish or dark-brown grains were disseminated through these probably exudative products. Pontico chiasmal lamina perfectly healthy, clear, transparent; cerebello-medullary lamina opaque, whitish dense bands in it; *pia* along the larger, and in some instances along the finer vessels with minute pale, yellowish, whitish and reddish bodies, supposed to be tuberculous. In the sylvian fossa, over the Island of Riel, there was a fusion of the leptomeninges.

A whitish spot, measuring $1\frac{1}{2}$ inches in every direction on the under surface of the A. basilares, otherwise the large veins were filled with dark continuous corgula, or with chains of whitish connected thrombi, such as occur in the ultimate agony when prolonged in exhaustive diseases. The fine network of vessels was well injected, and this was especially well marked over the Island of Riel.

Convolutions few, simple and typical. The white substance of the centrum ovale of Vieussens, of the pedunculi, cerebellum, ganglia, and tegmentum, as well as of the medulla and pons, showed numerous puncta vasculosa, all of a strikingly venous character, in every direction the veins, and these alone, were filled with blood. This was also true of the cortex, and nowhere better pronounced than in the gyri-operti of the insula Reilli; the claustrum, which I have never before seen the seat of any marked injection, was filled with distended venous channels and puncta venosa. (Wherever, in the numerous post mortems I have made, a marked injection occurred, even in animals which had died asphyxiated, the color of the puncta was purple—*never blue*.) The gray ganglia at the base of the fourth ventricle, which depend for their color on the degree and kind of injection, as well as on the pigmentation of their cellular elements, appeared semi-transparent and cerulean in tint. Spinal cord; membranes healthy, no deviation from the normal standard whatever, cord itself decidedly anæmic.

Ventricles. A mucoid substance covered the parts at the base of these cavities, particularly well marked at the calamus scriptorious of the fourth ventricle. Over the stria cornea of the left side, the ground glass appearance was visible. This passed gradually into the mucoid substance on either side.

Dilatation of the posterior cornua of the lateral ventricles with extension backwards, adhesion of the walls so extensive on the left side as to cause the complete separation of the apex of the posterior horn from the body of the ventricle, giving it the appearance of a cyst in the occipital lobe.

There was a beautiful venous injection of the ventricular lining. The mucoid matter on the floor of the fourth ventricle was found to consist of an accumulation of round cells, not surpassing a red blood corpuscle in diameter; some nucleated;

others not; all were perfectly colorless. Interspersed among them were larger elements, identical in every respect with white blood corpuscles. Isolated bodies of an oblong shape, with a distinct nucleus and pellucid protoplasm, were noticed. All these were imbedded in a granular mass, which showed a formation of imperfect fibrils. The arachnoid exudation consisted of the same matter, together with a fair proportion of red corpuscles, large flakes of pigment and round spheres, of a protein nature. The pia mater of the convexity exhibited numerous small nodules, most of which were molecular, others calcareous, and a few contained large and small polynucleated cells. These nodules were peri-adventitial, and hardly visible to the naked eye.

The cortical substance of the Island of Reil showed a marked increase of the neuroglia nuclei. The ganglionic cells (both pyramidal and fusiform) were normally contoured, processes well developed, protoplasm healthy, (in some cases diffusely pigmented), and nucleus round and clear. Free lymphoid bodies were accumulated in the pericellular spaces in prodigious numbers. In one instance, no less than twenty-three of these cells could be distinguished clustering around one pyramidal nerve cell of the third layer. Frequently, the nerve cell was altogether hidden from view by such cell groups. In this respect, the Island of Reil presented marked regional differences. It was found that areas varying from a line to an inch in diameter, were the seat of this appearance, while a similar larger or smaller adjoining area was either less involved or perfectly normal in this respect. The transition from the affected to the healthy areas was sudden.

The coats of all the vessels were entirely healthy, presenting no deviation from the appearance of cerebral vessels in sane subjects. The arteries were empty, the veins and many capillary districts filled with blood corpuscles; these latter were individually distinct, not compressed or fused by crowding, as has been described in the case of the stasis accompanying general paresis.

This engorgement was most marked in those areas in which the accumulation of lymphoid bodies was furthest advanced. The peri-adventitial spaces were filled out with similar bodies in the case of the vessels referred to.

The same appearances in a lesser degree were noticed in the operculum, and the convolutions bordering the anterior part of the great longitudinal fissure. The remainder of the cortex cerebri appeared perfectly healthy. The accumulation of lymphoid bodies was still more marked in the nucleus lenticularis than in the claustrum and Island of Reil.

The cerebellum, olivary bodies, nuclei of the cranial nerves corpus striatum, thalamus, and corpora quadrigemina presented no deviations from the normal standard. Spinal cord: The nerve cells of the gray cornua were perfectly healthy; a delicate granular material filled the dilated pericellular spaces; central canal open. The white columns showed everywhere an increase in the number and thickness of the connective tissue septa, and of Trousmann's cells. With this the medullary sheaths had undergone a slight degree of atrophy, while many axis cylinders were hypertrophic.

These conditions were most marked in the antero-lateral columns of the cervical portion of the cord, although the posterior were not free from it. Here it was limited to the peripheral portion, and a small area at the base of the posterior intermediate sulcus. The anterior pyramids of the medulla oblongata exhibited the same change as the spinal cord.

Conclusions. 1. The pia mater presented signs of an old tubercular process which had become latent.

The encephalon was the seat of a passive venous engorgement, which had been of long standing. No mechanical obstruction to the venous outflow could be found as the cause of this engorgement, and we must therefore suppose it to have depended on vaso-motor anomalies.

3. The gelatinous exudation of the arachnoid and pia can not be considered an inflammatory product, but rather as a simple filtration of molecular matter and blood discs through the walls of the distended venous channels.

4. The accumulation of lymphoid bodies by diapedesis around the ganglionic cells was in a like manner the result of the vascular stagnation. The fact that certain cortical areas were more severely affected than others, is to be attributed to peculiarities in the distribution of certain venous channels.

5. This accumulation of lymphoid bodies, of whose identity

with blood corpuscles (both red and white, particularly the former) I am fully convinced occurs to such an extent only in one other cerebral condition; namely, that which accompanies the severer forms of typhus fever. The similarity between the pathological appearance of the cerebral cortex in katatonia and typhus is truly striking; the chief difference is that while in the former certain parts of the cortex are chiefly if not exclusively affected, in the latter the whole encephalon is involved equally. It should not be forgotten that a few of these bodies (one or two in the pericellular space of one out from twelve to a hundred pyramids) occur in health, but so rarely that they have to be sought for, and are not, as in this pathological condition, so numerous as to actually conceal the nerve cells from view.

In lesser degree such an increase of the lymphoid bodies takes place in many forms of insanity associated with atrophy; their origin here is, however, different.

6. No destruction or degeneration of the essential nervous elements, the cells and fibres, was to be found, for no importance can be attached to the diffuse pigmentation of a few of the pyramidal cells. Many subjects who have never manifested any symptoms of mental alienation show the same condition.

7. The condition of the spinal cord and anterior pyramids observed is to be considered as a mild grade of sclerosis, approximating senile sclerosis in character. In a patient of this age, such a change is unquestionably pathological. I am inclined to consider it as a degeneration due in part to malnutrition, partly to disease of the motor tracts, in consequence of the long continued and oft repeated cataleptoid conditions. In this it offers a parallel to "Charcot's sclerosé laterale," as found in an old case of hysteric contracture, where the connective tissue hyperplasia was not the cause of the contracture, but the result of the consequent long continued disease of the motor periphery.

Vaso-motor anomalies frequently occurred; the first sound of the heart being at times absent, the neck pulse more frequent than the wrist.

The prognosis was stated to be good by Dr. Kahlbaum, but

the conclusion drawn in the paper was opposed to this. The
duration of the disease is from two to five years. The treat-
ment was mainly symptomatic and restorative. The chief
cause of the disease was the inheritance of a strumous diathesis,
the pathology and genealogy of the patient leading to this con-
clusion, and to the belief that infantile tubercular meningitis
was more frequently recovered from than was generally sup-
posed. The occurrence of Katatonia in epidemic form, as
claimed by Kahlbaum, was denied, the epidemic being at-
tributed to morbid impulse, simulating a few cases of Katatonia
that had occurred. An explanation was offered of similar epi-
demics by Dr. Hammond.

DR. EDWARD C. MANN asked the reader of the paper
whether the initial symptoms were of short duration; and if
so, was there not a manifest tendency to pass into a state of
dementia?

DR. KIERNAN replied that in the majority of cases the initial
symptoms were of rather long standing. They do not exhibit
a tendency to pass into dementia very suddenly. Most of them
do if the disease lasts five or six years.

DR. SPITZKA said that having made the autopsy in the case,
and being responsible for the pathological deductions, he could
merely agree with the clinical points presented by Dr. Kiernan.
Since the disease was a new form of insanity, he had devoted
particular attention to the microscopical analysis, in order that
Katatonia might eventually stand on as firm a pathological as
it does stand on a clinical basis.

In this connection, he continued, it must be confessed that
there is no special change in the nerve elements which could
be called characteristic of this disease. As has been heard in
the *resumé*, the same preternatural accumulation of lymphoid
bodies observed in Katatonia is found in the brains of those
dying from typhus fever. Perhaps this may serve to explain
the similarity of the symptoms sometimes observed in the
stadium decrementi of essential fevers, to a prominent phe-
nomena of Katatonia, namely, the atonicity.

There was one peculiarity, however, in the topographical dis-
tribution of the lesions in this particular case, in which it differs
from the similar condition of typhus fever. In the latter instance

the cerebral cortex is *generally* affected, but here the change was concentrated in the Island of Reil and its operculum. That this localization may bear some relation to the peculiar symptoms on the part of the speech, such as the slow, precise, and theatrical (though disconnected) manner of talking described by Dr. Kiernan, is probable.

He would say *en passant*, that while he was diametrically opposed to Ferrier's deductions, and while he considers any sharp, abrupt demarcation of "centres" in the cortex as unphysiological, he must predicate a relation for the Island of Reil and the contiguous territory to both articulate and symbolical speech, for reasons discussed at the meeting in February. The venous stasis and lymphoid accumulations constitute two antagonistic factors, the former paralyzing, the latter stimulating the nerve cells of the Island of Reil, and thus producing the clinical paradox of an inhibited activity; verbigeration.

As to the slight connective tissue hyperplasia observed in the spinal cord, he attached no importance to it, and considered it a secondary, perhaps, also accidental complication.

At the previous meeting he suggested the propriety of employing nitrite of amyl in these patients, and in the course of the same week was invited by Dr. Kiernan to witness the effects of this drug, effects which were at first very striking.

Among others, one patient was introduced, who on being urged to answer three or four simple questions, answered slowly and with difficulty. He suffered from extreme depression and depressing delusions. He assumed a puzzled look when asked to tell how long he had been in the asylum, and passed into a cataleptoid condition, exhibiting wax-like immobility of the highest degree, his pupils being also widely dilated.

Ten (10) drops of nitrite of amyl were administered, and his whole condition changed like a flash. His pupils became contracted, he became communicative *and perfectly* rational, spoke freely of his past condition, and himself expressed astonishment at the change manifested in so short a time.

Subjectively a feeling of well-being was felt, and the depressing delusions were denied to have ever existed. These

obvious and beneficial effects were not as transient as were anticipated, but persisted for many hours, and all that was necessary to insure a continuous effect, was to give the dose several times in the course of a day. In this case delusions returned, but they were of a positive character, not depressing.

He was well aware that the strong way which he proposed the administration of nitrite of amyl will be objected to by some. For instance Dr. Dupuy* has stated that the increase of blood in the brain of animals, after the inhalation of this drug, is not of arterial character, but that the blood is chocolate colored, and that the blood therefore must be supposed to be chemically changed.

Against this the speaker could urge but a single experiment, in which he found the blood brightly arterial, and the pulsations excursive, as well as the influence of the drug in his own person. The discrepancy in the results of the experiments may be explained by the difference in the doses employed, as he used such quantities as should be therapeutic in the human being. In the numerous inhalations which Dr. S. had indulged himself, he had not noticed any such injurious effects as would occur from the employment of a drug so dangerous as Dr. Dupuy claims nitrite of amyl to be.

Whatever be the explanation adopted for the action of amyl nitrite, it seems to act as a vaso-motor stimulant. By this is not meant that it increases the tone of the circular coat of the arteries, but that in any manner it increases both the *rapidity* and the *amount* of the cerebral blood-current; for all the symptoms, noticed after inhalation of the amyl-nitrite are symptoms of cerebral hyperæmia. Since the vascular condition in Katatonia is essentially a stagnation of the blood-current, with a diminished amount of arterial blood in the brain, nitrite of amyl, a drug which produces exactly the opposite condition, is clearly indicated.

Dr. Seguin said that he used amyl-nitrite on patients subject to melancholia in private practice, and apparently with beneficial results.

Dr. Dupuy said that he did not deprecate the use of amyl-

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* Six lectures on the Pathology and Treatment of Nervous Symptoms. [Soon to be published.—G. W. W.]

nitrite as a therapeutic agent altogether, but still insisted that it changed the nutritive processes in a pathological manner. The track of the blood vessels in a dog's brain became marked by a white line of exudation corpuscles, and that the blood was of the color of venous blood. He asked Dr. Spitzka whether there were no lesions of the nerve cells, as he recollected seeing a plate representing the microscopic appearance exhibited at the last meeting.

Dr. SPITZKA replied that while the illustrations referred to represented pathological conditions, the latter did not effect the cells *as such*, but that there was an extreme accumulation of lymphoid bodies (white corpuscles) around the cortical pyramids. He did not believe that destructive or any palpable changes occurred in the first stages of several forms of mental alienation, or at all in some acute forms.

Dr. SEGUIN asked whether Dr. Spitzka had referred to exanthematic or abdominal typhus as resembling Katatonia in the cortical changes.

Dr. SPITZKA replied that while he did not feel prepared to discriminate between these forms in this respect, the investigations to which he referred, were made in cases of the former species of typhus. About a year ago, Popoff made these investigations in Recklinghausen's laboratory, some curious misinterpretations, however, diminished the value of his observations. These were left unchallenged until the beginning of this year, when Herzog Carl, of Bavaria, showed that any such occurrences as an invagination of the ganglionic protoplasm by white corpuscles, producing nuclear segmentation, as described by Popoff, were out of the question, and the results of imperfect observation.

Herzog Carl also found that a similar condition obtains in some cases of cardiac disease, especially where obstructive to the return circulation. This bears out our attribution of the leucocyte diapedesis to the venous stasis.

In connection with Prof. Pallen's paper on menstrual neuroses, read before the Neurological Society, in April, which appears in another portion of this JOURNAL, the following remarks were made:

Dr. PUTNAM JACOBI being called upon, remarked that she

had recently had occasion to pay a good deal of attention to
the theory of menstruation, and quite agreed with Pallen that
the so-called ovulation hypothesis, was insufficient to account
for the facts. " I have endeavored elsewhere to show, said
she, that this famous modern hypothesis, according to
which the uterine hemorrhage constitutes a mere epiphenom-
enan of processes concerned in the development of the
ovule,—really rests on a slender foundation of fact," at the same
time she confessed that she failed to understand what Dr.
Pallen means " by neuric causes," in this connection, nor the
"nerve influences involved in cell growth," etc. She under-
stood him to exclude from the theory, both the cell growth in
the ovaries, and still more decidedly that determining the
development of the uterine mucous membrane, for he em-
phasizes the fact that, on the one hand menstruation is known
to continue after extirpation of both ovaries, on the other
hand, even in the absence of a uterus and hence independent
of the fatty degeneration of the so-called menstrual decidua.
The numerous examples of vicarious menstruation, even when
the uterus is apparently intact, examples to whose list Dr.
Pallen has himself contributed this evening, all confirm the
opinion that the real cause of menstruation is constitutional
and not local. Local conditions determine the habitual direc-
tion of the menstrual flux, and also its periodicity, but the
fundamental cause lies deeper.

For my own part I have been led to the unqualified
adoption of the antique theory of a nutritive plethora in
women, as the real cause of this periodical elimination of nu-
tritive material,—material formed in excess of the needs of
the individual economy, and in preparation for the demands
of reproduction. This theory is well known to have reigned
from Hippocrates down, until the discovery of the ovule in
1827, or rather of its spontaneous dehiscence, in 1847. Pre-
vious to this time it was admitted as a logical necessity that
the sex upon which devolved the greatest cost of reproduction,
should be endowed with a special richness of nutrition in
order to render it capable of meeting this demand without
annihilation. This logical necessity still exists, and it is as
absurd to suppose that the formation of an ovule would suf-

fice for reproduction, unless special provision existed for the
accumulation of extra nutritive material,—as to suppose that
a tree could develope branches from buds or seeds, for whose
nourishment were provided no special currents of sap. The
menstruation is the exact analogy of the accumulations of
nutritive material along the stem of a plant, which constitute
the nodes, each of which is destined under favorable circum-
stances to develope into a branch. It is a great mistake to
consider normal menstruation as a phenomenon of animal, in-
stead of as it really is, vegetable nutritive existence. The
accidents of nervous irritation constituting the so-called "men-
strual neuroses," nervous dysmenorrhea, epilepsy, chorea,
hysteria, etc., depend ultimately upon a denutrition of nerve
centres, owing to a disturbance in the equilibrium, normally
maintained between the nutritive provisions made for them,
for muscles, and for the development of the embryo. When
the nutritive balance is disturbed from any cause, the nervous
system is the first to suffer, while the reproductive nutrition,
being the most rudimentary, is the last to be given up.

After shock, the assimilative capacity of all tissues is
probably lessened ; less nutritive material is taken up into the
blood, and from that again less is withdrawn by nervous and
muscular elements. If assimilation diminishes out of pro-
portion to the diminution of absorption, there is a double re-
sult ; the tension in the blood vessels is raised at the same mo-
ment that the capacity of resistance to pressure in the nerve
centres is diminished. Hence, as the vascular tension reaches
its maximum just before the menstrual hemorrhage by which
it is lowered, the irritability of nerve centres also reaches a
maximum, and is manifested in the so-called "menstrual
neurosis." In Dr. Pallen's first case where syncope from
cerebral anemia is said to have occurred, it would seem that
the anemia was caused by vaso-motor spasm of the cerebral
blood vessels.

She should be interested in knowing whether the nitrite of
amyl was prescribed because the patient was menstruating, or
because she had epileptiform attacks. For the latter, the rem-
edy has acquired a decided reputation; but she believes she
was the first to suggest its use in various nervous disorders of
menstruation, especially pain.

The disturbance of nutritive equilibrium, to which she has alluded, is somewhat curiously shown in the history of peasant women with powerful muscular development, and regular and abundant menstruation. The balance is *plus* in the muscular and reproductive nutrition, *minus* in that of the nerve centres, and these are unable to resist the rise of tension in their blood vessels, caused by the development of the menstrual waves.

Dr. Pallen stated that his reason for considering menstruation entirely of neuric origin, was, in the natural history of the function, that it was, to use Dr. Mary Putnam Jacobis' own word, " an epiphenomenon." It did not make its appearance until the developmental action of the ovaries and uterus had, in a measure, been completed, and that it was interrupted by pregnancy, or disease or emotional cause, and that after about twenty-five years in the woman's life, it ceased altogether. Were it not dependent upon "ultimate-cell" cause in the cerebro-spinal centres, transmitted through the ganglionic to the vaso-motor and trophic nerves of the generative circle, in consequence of the development of the utero-ovarian structures, then it ought to be a permanent function of automatic life, such as bile-secretion or urine-secretion, which commenced at birth and continued through life. That menstruation could be modified by purely neuric cause, either emotional or molecular and reflex, was another proof of its epiphenomenal nature. The strongest proof, however, was in the anatomical structure of the generative circle, composed of masses of blood-vessels, erectile tissue and plexuses of exceedingly sensitive nerves, which were actually erect during the menstrual fluxion, hyperstasis and congestion, as much as were the male generative organs out of the pelvis whenever certain portions of the cerebro-spinal centres were irritated, either from pathological or emotional causes. If erection of the male organs was dependent, as they are, without any doubt, upon neuric stimuli then their anatomical and physiological analogues in the female had to obey the same set of governing principles, as nature does not call upon similar functions in the two sexes to respond to dissimilar causes.

We can trace from the earliest embryological states, from the curving upwards or downwards of the ducts of Müller to the

complete development of the fœtus, a most marked analogy of the male and female sexual organs; between

> The testicles and ovaries,
> " vasa deferentia and fallopian tubes,
> " cremasters and round ligaments,
> " glans penis and clitoris,
> " scrotum and labia-majora,

even to the minutest distribution of follicles and glandular textures, as well as the membranous portion of the male urethra and the vagina. It is too late to continue these analogies any farther, and as he expects to elaborate the subject in another paper* about to be printed, he merely mentions them to illustrate why he is disposed to regard menstruation to be of neuric origin, purely an epiphenomenon attached to a certain period of a woman's life, to indicate that she is prepared to procreate, and at a time after infantile weakness has passed, and before senile decrepitude has commenced.

He should be very glad to hear Dr. Putnam Jacobis' views more elaborately expressed, with regard to her theory of deranged menstruation in the peasant women of France, as from what she states to-night, he could hardly accept her views as a physiological cause, although they may thoroughly explain pathological results.

FROM PHILADELPHIA.

EDITOR JOURNAL:—Neurological matters have been quite active in Philadelphia for several months, especially in the various discussions concerning

Noise as a Disturber of Health. The relation between noise and disturbances of health, especially of the human system, has received some attention by various authors. It is probable, however, that, neither upon the annals of law or medicine this subject has received such attention as has been elicited by the case of Harrison et al., *vs.* St. Mark's Church, Phil. It will not be possible, in a correspondence, to review strictly every aspect of the case, but rather to look at it from

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* Dr. Seguin's series of American Lectures.

a medical standpoint, grouping together the data furnished by
the trial, in relation to the topic above announced. In order
to obtain an intelligent idea of the opinions recorded, it will
be necessary for the reader to have presented to him a con-
densed *resumé* of the main features of the case.

The complainants are several gentlemen, living in a section
of Philadelphia, "covered by handsome and expensive resi-
dences," of peculiar value, by reason of the supposed free-
dom from nuisances. These gentlemen have expended large
sums of money in buildings which are adapted to no other
purpose than dwelling-houses.

The defendants are a Protestant Episcopal Church, who
have a church edifice on the north side of Locust street, be-
tween Sixteenth and Seventeenth streets, and who have, within
the year, placed four large bells in the tower, these bells being
a part of a chime. The church is built of brown stone, and
the beauty of the architecture and the careful attention paid
to the grounds were considered to make the neighborhood
more attractive. No bells of any kind were in and about the
church until June, 1876. It is upon frequent and prolonged
ringing of these bells that the difficulty depends. The hours
and length of time of ringing are as follows:

Sunday, 7 A. M.   Rung 15 minutes.
    "   10½ "   " 30 "
    "   4 P. M.   " 30 "
Also daily, at 9 A. M.   10 "
    "   "   5 or 6 P. M. 10 "

On festivals and Saint's days the services are more frequent
than on ordinary week days. The average stroke is 80 per
minute. Now, the complainants testified that, by reason of
the frequent, prolonged, overpowering noise, produced by these,
harsh, loud, high, sharp, clanging, discordant bells, amounts
to an intolerable nuisance. They claim that it shakes the
walls of the houses, disturbs sleep, especially that of children
and infants, distracts the mind from any serious employment;
it lessens or destroys social and domestic intercourse, and
much of all that which goes to make up the peace and happi-
ness of home life. It is also claimed that this is a nuisance, not
only to the well, but that positive harm is done to the ill,

*especially to those whose nervous systems are delicately or-
ganized.* Moreover, it is averred that the nuisance is not con-
fined to the time of the bell ringing, but the expectation of its
beginning produces a nervousness and excitement, which to all
is painful, and to some, intolerable.

The defendants deny each and every statement of the com-
plainants, and what interests us most, they affirm that the
sleep of no one is disturbed—not even children and infants,
that the mind is not distracted, that social and home happi-
ness are not destroyed, that the health of a single person has
not been injured or impaired in the slightest degree. More-
over, the defendants claim that in many instances invalids have
expressed positive gratification, and their recovery has been ma-
terially assisted by the chime, and that the alleged nervousness
or excitement produced by the expectation that the bells will
ring is not, in truth, a natural consequence of the use of the
said bells, but is due wholly to a morbid, mental, or physical
condition of the person or persons (if any) by whom such ex-
pectation is entertained. Here are two diametrically opposite
views of the effect of this particular noise upon the human
system of the inhabitants of the neighborhood. Moreover,
each party has secured the affidavits of physicians to support
their peculiar veins, and we find with proverbial certainty that
the doctors disagree as to the effect of noise upon the health.

Some of these affidavits from both sides will now be pre-
sented. Dr. ADDINELL HEWSON deposed: " I have given con-
siderable attention to the treatment of nervous diseases. I
have had, and have now numerous patients in the immediate
vicinity of St. Mark's church. I have frequently been visit-
ing the latter during the time of the ringing of the St. Mark's
bells. I have experienced no inconvenience from the noise
so made in conversing with my patients, and on inquiry of
those patients have been assured that it was no annoyance to
them. In one instance, that of a lady patient residing within
one hundred and fifty feet of the church tour, and who was an
exceedingly nervous patient, I had the assurance that she had
utterly ceased even to notice their being rung. I can cite also
two other instances of patients, invalids, who took satisfaction
and pleasure in listening to the ringing of these bells." * * *

"I have had the averment of patients in the immediate neighborhood that the early morning ringing failed to disturb their repose and sleep after it had ceased to be a novelty. I have had nervous patients, who were very ill, residing in the immediate vicinity of churches in this city, whose bells were larger, louder, and harsher, rung at earlier hours and much more frequently than those of St. Mark's, who were not, in my opinion, in any way disturbed or injured by the ringing of those bells." I extract from the affidavit of Dr. S. LITTLE such sentences as bear upon our subject: "I have never observed any injurious effect upon my patients from the ringing of these bells. People soon become accustomed to noise, even disagreeable ones, and fail to notice them. * * * I, as a physician, cannot see any reason why these bells should be interfered with, except in specified cases of very serious illness." Also from Dr. VAN PELT'S affidavit: "I have had patients in the immediate vicinity of St. Mark's church. I have never observed any ill-effects upon them from the ringing of St. Mark's bells." He also mentions several particular cases of nervous patients living near other large bells, and who were not in the least disturbed by the ringing of the same.

On the other hand, Doctors Alfred Stillé, S. Weir Mitchell, Elwood Wilson, William V. Keating, S. D. Gross, Francis G. Smith, H. Lenox Hodge, J. Cheston Morris, William Pepper, James H. Hutchinson, Albert H. Smith, Edward Hartshorne, John H. Packard, have signed their names to the following certificate: "In every community there is a large class of persons in the enjoyment of average health, whose constitutions demand a quiet dwelling-place as indispensable to their well-being and happiness, and to their usefulness to their families and to society. In addition, there are the sick, both acute and chronic, to whom quiet is essential for speedy and proper recovery, or for the prevention of the aggravation of their disorders. In the practice of our profession, we are constantly reminded of the truth of the foregoing, and recognize, and deplore the injury often done our patients by the noise incidental to, and, probably, necessarily connected with, a large city.

We have become cognizant of the annoyance and discom-

fort, and some of us even of the likelihood of injury to health to members of the afore-mentioned classes, if the practice be continued, caused by the loud noise of the St. Mark's bells recently erected; and believing it to be our duty when an evil arises—especially when it is not in any sense a necessary evil— to use our influence to abate it, therefore we earnestly and advisedly call the attention of the rector, wardens and vestry of St. Mark's church, to the evil influence exerted by the *early,* the *frequent,* and the *prolonged* ringing of their bells, *believing it to be prejudicial to the health of some and to the comfort of many of the residents of the neighborhood,* some of whom have specially sought the locality on account of the quiet which it has heretofore enjoyed."

The special affidavits of some of the representative medical men of the city are worthy of careful perusal. Thus S. WEIR MITCHELL, M. D., well known at home and abroad as a leading neurologist, being duly sworn, says: "I am a doctor of medicine and have been so for twenty-four years. For several years past I have given particular attention to the treatment of nervous diseases, and am frequently called in consultation with others upon this branch of my profession. In the month of November last (1875), I heard many complaints among my patients and others of the noise caused by the bell-ringing at that church, which distressed them in various ways according to their various temperaments. Some were those who found it difficult to sleep on going to bed at night, and who naturally relied upon a morning sleep, more or less late, to make up for the loss of the night sleep. This morning sleep, of course, they lost by the early seven o'clock bell-ringing. It is more than probable that in a majority of such cases a sleep in the afternoon or evening might take the place of the lost morning sleep, but this was denied them by reason of another bell-ringing in the afternoon at five o'clock, and on Sundays at half-past four in the afternoon, and at seven in the evening. These taken together made the treatment of a certain class of diseases in that neighborhood almost an impossibility, and I was compelled to request one of the vestry of the church to discontinue the early morning bell-ringing. The request was

(Italics mine. C. C. V.)

granted in a note from the rector." The doctor now pro-
ceeds to give a scientific explanation of the effect of noise in
general upon the health of an individual. He says: "It was
not only the actual noise of the bell-ringing (and the reason of
the noise and clangor of these particular bells is easily ac-
counted for upon familiar principles of the loss of sound),
but it was the *expectation* of their beginning which had a
notable effect upon a certain class, in producing a painful
nervous irritability and excitement. It is no answer to this to
say that this is imagination, and mere nervousness. Mere
nervousness is, perhaps, the most difficult and subtle disease
with which modern science has to grapple; and it is diseased
imagination which fills our mad-houses. Every one knows
that a person of average health going to bed with the inten-
tion of starting on a journey at an unusually early hour next
morning, will often sleep very restlessly, and sometimes not
sleep at all. It is the *expectation* which deranges the normal
condition of the brain. Of course, there are persons whose
temperament ignores the operation of such influences, and
who can truly say that neither the actual noise of the bell-
ringing nor the expectation of it at all disturbs them. But
what is true as to this class of placid temperaments is not
true as to another class, which fluctuates with the varying
health of the individual of every community, and the differ-
ence in annoyance is immense as between the well man and
the sick, moreover, there is also a difference in this connection,
not only between those who are ill and those who are well, but
a difference and a natural one, between men and women. The
same rules of hygiene obviously do not apply equally to men
who, from business or other causes, are away from their homes
the greater part of the day, and to women whose vocations
confine them more to their homes. Among the latter, it is
very common, particularly in summer, to lie down and sleep
during the hot hours of the afternoon.

To invalids, the afternoon or early evening sleep may be just
as important as the night sleep, and in case of infants particu-
larly so. It would be folly to prescribe a narcotic at half-past
four, when a person was sure to be thrown broad awake at five,
and so of the other hours; and it therefore may come just to

this—that the medical treatment of the neighborhood must
be regulated by the hours of the defendant's bell-ringing. As
to the bell-ringing at seven in the morning, it is scarcely neces-
sary to say more than a word. There are those, but, perhaps,
not a large class in this neighborhood, who habitually rise at
daylight or soon after; there are others, whose habits of life
are just as carefully measured, who rise much later, and nei-
ther class has a right to claim a peculiar merit over the other,
though this is often done by the former. But when habits of
life are once formed, it is difficult, and sometimes dangerous to
change them after a certain age; and to insist that certain per-
sons shall not be allowed to sleep after seven o'clock in the
morning may be admirable in a boarding-school, but absurd
among grown people living in an artificial state of society.
To those of my own profession, used as we are to habitual
violation of the laws of nature, and to go at once from a night-
watch to our breakfasts and our daily duties, there are still
times when we could snatch an hour or two of morning sleep
which would better fit us for our business. One thing may
possibly be said of this particular bell-ringing—that during the
season when it causes greatest suffering, *viz.*, the summer, the
class likely to be most annoyed by it is absent from the city;
but the answer is, first, that many, especially women and chil-
dren, are detained in town during this time by sickness or
other causes ; and secondly, that although this section of the
town contains many who are rich, it also contains many who
are poor, and who cannot fly from, but must endure the pain.
When it comes to the question of early Sunday bell-ringing,
all that I have said is intensified. There is a large class of
God-fearing Christians, as well as others, who begin their day
of rest by an hour or two of extra rest. To many men, this is
not luxury, it is necessity. The pressure of modern social life
necessarily produces, especially among professional men, a
degree of brain-tire—of loss of power to use the brain, of
which the results are terribly alike, beginning with insomnia,
irritability, nervous excitement, cerebral derangement, and run-
ning the gamut of mischief down to paralysis and death.
While it would be absurd to say that any one who was waked
out of a deep sleep by a Sunday seven o'clock bell-ringing

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would get a paralytic stroke and die, I do say that a man whose brain has been sorely worked during the week, and whose brain-tire was habitually lessened by one, two, three or more extra hours of sleep on a Sunday, is pushed well on his way to disease by having that natural medicine withdrawn. For, as a distinguished modern author has put it, 'The mere procuring a regularly recurring oblivion of distressing impressions is no slight boon, and makes the sufferer more capable to bear his waking burden.' Of course, there are intellectual prodigies, whose brain development, combined with great physical power, and an ordinarily utter absence of nervous excitability, enables them to do without those "let ups," so to speak, which to some are a partial and to others an absolute necessity. But such men are abnormal. Nor is it an answer when some say that this bell-ringing never disturbs them, and that in particular, after a period of severe mental or physical strain, they have slept through it all. The instances are notorious of soldiers who have slept while on the march, and even during battle. In our own time, nearly every one who served actively during the late war can recall similar instances. But these things do not disprove general propositions; they are simply striking exceptions. And that insomnia is not only one of the most dangerous, but also one of the most painful of diseases, is shown by the fact that those who are suffering from it would willingly exchange it for almost any other form of disease. The multiplication of needless noises in modern life is beginning to attract scientific attention in Europe as a cause of discomfort, (and therefore a superinducement to disease) in well people and a present injury to sick people. The last steamer brought over a number of the London *Lancet*, in which was an article on the subject, referring especially to unnecessary bell-ringing. And while it is true that no one can as yet, expect to escape in our great cities, for noises connected with manufactures and travel, but which the civilization of the future will certainly arrest, it is not too much to require that such noises should not be unnecessarily multiplied, but be limited to secular work, and to the apparatus or machinery by which cities have their wants supplied."

THE VIEWS OF PROF. J. M. DA COSTA.

Being duly sworn the doctor says: For years actively engaged in the practice of my profession. Partly in consequence of living not far from St. Mark's, partly in consequence of knowing many who dwell in its more immediate neighborhood, some of whom I attend professionally; I have had the subject of the church bells prominently brought to my notice. I have heard the bell-ringing greatly complained of. It is by nearly every one regarded with disfavor, and by many considered an intolerable nuisance. The early morning bell roused persons from their sleep; the afternoon bell prevents those to whom it was important, from habit or from indisposition, to obtain some repose, from so doing. I make these statements as they have been made to me by a number of patients. The bad effect of unwelcome noise to the sick and well alike is a matter easily ascertained. It renders, for instance, an attack of migraine, an unendurable punishment; it aggravates delirium; it may make the difference in the sleeplessness of a fever, between recovery and death. In certain inflammatory or irritative disorders of the brain, the effect of noise is most painfully witnessed. Do we not constantly see houses with tan before the door to lessen the sound of passing vehicles? But what now will be the use of this, if the loud, discordant peals are almost at all hours to ring through the air? In certain chronic conditions of the brain, noise becomes an irritation that takes away strength and impairs vitality. I attended some years since, a lady with beginning softening of the brain, to whom noise was so terrible and exhausting, that she prayed devoutly that she might not live to see another 4th of July. What would be her daily sufferings if now alive, and within easy reach, as she would have been, of the bells of St. Mark's? Every stroke of the loud clangor would have rung discomfort or been a pang of distress. On those who are well, the effect of noise varies much, according to the temperament. Some it does not annoy, or they become accustomed to it; others it annoys extremely, and they never become accustomed to it. It makes them irritable or greatly increases irritability. If not good sleepers, and the noise deprives them of, or curtails their rest, the irritability works into

a positive injury. On little children, so dependent on sleep, and on sleep in the day time, for their health, the bad consequences of being awakened or prevented from sleeping by loud sounds are self-evident; and I have often thought that the little ones in the neighborhood of the loud bell-ringing were likely in the spring and summer days to be among the greatest sufferers. Then there are men in all large communities, well in body, but with minds constantly on the stretch, whose habits and mode of life may have made them particularly sensitive to noise, and whose occupations are sadly interfered with by such disturbances. This class embraces many of the most thoughtful professional men, the original thinkers and writers in science, the higher order of men of letters. These laborers are naturally the ones that make a community great; and many of them can do their best work only when unperturbed, when their nervous force is not dissipated by jarring interruptions of unwelcome sounds. How true this is, is seen by the telling petition drawn up, and I think presented to Parliament, by a large meeting of men renowned in science, literature and art, praying to be relieved of the organ-grinding and other nuisances of sound, and setting forth that these interruptions interfered with their vocations. Chas. Dickens was one of the committee, and spoke very feelingly of how much his work was thus retarded or spoiled. Persons of this kind naturally seek a quiet neighborhood, and to disturb them heedlessly is to inflict injury on a community. That with the recognition of the greater and greater strain on the nervous centres produced by the conditions of our present civilization, this question of noise, and especially as connected with church-bells, is beginning to attract much attention, is readily proved by referring to the medical literature of the day. Thus the "London Lancet," a world-renowned exponent of scientific medicine, in an article on noises, notices that the public at last are beginning to insist on the blessing of quiet, which has so long been denied them. The war, however, against noise, which has thus been successfully inaugurated, must not be allowed to flag.

Our working-hours as well as our brief seasons of repose are disturbed in many other ways besides the rattling of the traffic.

The organ grinder, the German band, the coster's yell, are instances of a tyranny over weak or strained nerve that ought to be suppressed. Church bells, which in the country undoubtedly have a charm, become in the crowded city a positive distress to many sick persons. Last year the Queen, at the opening of Parliament, considerately gave orders that the Abbey bells should not be rung, lest they might disturb the repose of a dear friend who was dying within the Abbey precincts. Are there not hundreds of sufferers in London who would be thankful to have a like consideration extended to them? * * * On Sunday the ringing is not so hurtful to the invalid, since nearly all the churches keep the same hours, so that there is at least peace during the hour of service. It would be a great gain to the sick of the metropolis if the church bells were permitted to ring only on the Sunday, and then but for a limited period.

Of course, where sound is melodious and pleasing—music rather than the monotonous vibration—the evil lessens greatly, and the interruptions spoken of may be to some agreeable. But this can not apply to the bell ringing in question. I live in a neighborhood where I have been obliged to hear it. I have, when sitting near an open window, been repeatedly annoyed by it, and do not see how the most vivid imagination could construe the penetrating, harsh bell-tolling into music."

Dr. WILLIAM THOMSON, being duly sworn, says:

" I am a doctor of medicine, and have been so for twenty-two years. I have given special attention to the diseases of the eye and ear, and am frequently called in consultation with others upon these branches of my profession, and have, of course, had occasion to study the scientific laws of sight and sound.

My attention has been particularly called to the noise made by the bell-ringing at St. Mark's church. Soon after the bells were placed there, I was on my way to a consultation on a Sunday morning near the hour of service, and stopped directly opposite the church, in front of Mr. Cadwalader's house, to notice the extraordinary noise which the bell-ringing produced, and thought, as I do now, that it was almost as intolerable a nuisance as could be produced by sound. The noise was not musical, and the difference between noise and music can be

illustrated by rattling a tool-box, and by drawing a violin bow across a tuning fork. The sound of these bells was noise. It was sharp, shrill, harsh, loud and dissonant. The sound of a proper chime of bells, properly cast, properly hung; (both as to elevation and mechanical adjustment) and properly rung, is music, which is defined to be " a combination of sounds in accord or harmony."

The reason why these bells produce noise and not music there, is referable to the simplest principles of the science of acoustics. The bells are placed in a tower at the height, I am informed, of sixty-five feet from the ground. Take this tower as a centre, we find a " well" formed round it, consisting immediately on the west and east of the houses of Mr. Carver and Dr. Hays, on the north by the rear of the Walnut street houses, and on the south by the Locust street houses. Now everybody knows that the action of sound, like that of light and radiant heat, is a wave motion, and " every experiment on the reflection of light," says a distinguished scientist, " has its analogue in the reflection of sound." A lighted candle distributes the rays of light in every direction—vertical, lateral, and in every degree of the circle—except, of course, directly below the flame, where is the candle itself. Observe a chandelier, opposite and between two mirrors, and you will see the lights reflected almost infinitely, because the rays of light have no way to escape, and are reflected from one mirror back to the other, and back again, and so ad infinitum. Although the waves of sound travel more slowly than those of light, yet otherwise their action is the same, because the angle of incidence is equal to the angle of reflection. From the tower of St. Mark's church, as a centre, the waves of sound produced by the bells rush out; a part goes up, a part goes laterally, and a part goes downward. A great part of the lateral, and all the downward waves, strike most nearly the parish school-house, the houses of Mr. Carver and Dr. Hays, and the opposite houses in Locust street. From these, they are reflected exactly at the angle of incidence back to the church, or, it may be, first to the ground below, and then, like a billiard ball, to the church, and so backwards and forwards, the waves following each other in rapid succession. At a greater distance, they strike the rear

of the Walnut street houses; they also strike obliquely the walls of the Locust street houses near Sixteenth and near Seventeenth streets respectively, and are reflected back to the houses on the north side of that street, both below and above. The noise can also be heard elsewhere than in the line of these direct waves. It is now clearly proved that a sound-wave bends itself around opaque obstacles, though as it diffuses itself in the air at the back of the obstacle it is enfeebled in power, the obstacle thus producing a partial shadow of the sound. The sound-waves of this noise would therefore be heard at a much greater distance than I have named, but with less intensity, since this diminishes inversely as the square of the distance.

These are the directions of these sound-waves when the windows are closed. When they are open, of course the waves enter the houses and are reflected back and forward in and among the several rooms, crossing each other and producing a multiplicity of echoes, the intensity of which alone would render them distressing to the ear; while positive pain is caused by the irregular, unrhythmical and discordant sounds caused by four bells placed and rung as these are.

Passing from the scientific to the medical view of the subject, it is familiar that the various nerves of the body have their origin in the brain, which is the seat of sensation. " It is the emotion excited by sugar in the nerves of taste," says the same author, "which, transmitted to the brain, produces the sensation of sweetness, while bitterness is the result of the motion produced by aloes. It is the motion excited in the olfactory nerves by the effluvium of a rose which announces itself in the brain as the odor of the rose. It is the motion imparted by the sunbeam to the optic nerve which, when it reaches the brain, awakes the consciousness of light, while a similar motion imparted to other nerves resolves itself into heat in the same wonderful organ. * * * But the nerves of taste are not competent to transmit the tremors of light, nor is the optic nerve competent to transmit sonorous vibration. For this latter a special nerve is necessary, which passes from the brain into one of the cavities of the ear, and there spreads out in a multitude of filaments. It is the motion imparted

to this, the auditory nerve, which, in the brain, is translated into sound." When, then, the waves of sound reach and fill the cavity of the ear, they are driven against the tympanic membrane, which is stretched across the passage leading towards the brain. This, which closes the drum of the ear, is thrown into a state of vibration, its motion is transmitted to the ends of the auditory nerves, and thence along these to the brain, where the vibrations are translated into sound.

Hence it is easy to see why melodious or rhythmical waves of sound effect the brain with pleasure, and waves of mere noise, particularly dissonance, with pain. And I am not sure but that the brain is not more sensitively affected by sound than by sight. Although an unpleasant sight will cause an involuntary closing of the eyes, yet this is often but momentary, while every one knows the actual suffering caused to a skilled musician by even a false note, and frequently persons leave a concert-room unable to endure the discord of even musical sounds.

Of course there must be cases in which the brain, with its exquisite adaptability, accommodates itself to certain conditions which are exacted of it. But for this there is a penalty. The professional wine-taster or tea-taster loses all or nearly all sense of the pleasure of the taste. Old artillerymen are often deaf, and I have frequently seen men accustomed to the riveting of bolts in boilers, whose deafness I could ascribe to no other cause than their trade.

We all know that however pleasing to the eye light may be over darkness, yet we are not able, without pain, to regard the bright sun or the glare of a calcium light; and in like manner loud and dissonant sounds, by their forcible impact upon the drum-head, give intense annoyance to the mind. It must be also remembered that the ear, unlike the eye, is provided with no protection, like the eyelids, to exclude or moderate the force of these irritants, but that in sickness or in health, asleep or awake, the vibrations of sound have free access to our nervous centres, and are capable of exciting intense pleasure or indescribable and intolerable pain.

Prof. SAMUEL D. GROSS, being duly affirmed, says:

I am professor of surgery in the Jefferson Medical College

of Philadelphia. I have been engaged in the active practice of medicine and surgery since 1828. I have read with great care the affidavits of Drs. S. Weir Mitchell, J. M. Da Costa, and William Thomson, and fully endorse the views therein expressed concerning the injurious effects of the ringing of St. Mark's bells upon the residents in the vicinity of that church. Early last spring, as I was passing with some friends the church on Locust street, above Fifteenth street, when the bell was ringing for the evening service, the noise that greeted my ears was so horrible that every one involuntarily exclaimed, "Who would live in such a neighborhood?" The church seemed almost to shake with the disgusting sound. If such a noise grates harshly upon a healthy ear, it is easy to conceive how injurious its effects must be upon the ear of a nervous person, or upon a person laboring under disease, fatigue, grief, or anxiety of mind, and in need of sleep. The sound of the street organ, the harsh and discordant clatter of the parrot, the barking of a dog, and the song of the mocking-bird, are, if daily or nightly repeated in the same neighborhood, a source of real suffering even to many persons in health. I know of no more annoying sound than the loud and discordant shriek of the newsboy, when one is in need of his siesta or of finishing his morning's slumbers. I have been compelled more than once, on such occasions, to invoke the mayor's services in behalf of invalids as well as in my own defense. The sound of my neighbor's piano, however melodious in itself, may, if of constant recurrence, become a source not merely of annoyance, but of great suffering, interfering with sleep and that mental repose so necessary to comfort and happiness. Even the chirping of our sparrows at the early dawn of a summer's morning is to many persons a serious evil, especially when these birds are congregated in large numbers, as they are in some parts of our city. But I regard none of these as at all comparable to the nuisance caused by the ringing of church-bells, if long-continued and frequently repeated.

C. C. Vanderbeck, M. D., Ph. D., 1,121 *Walnut St.*
Philadelphia, June 6th, 1877.

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American Neurological Association.

THIRD ANNUAL SESSION.

First Day's Proceedings: Reports of officers and of Council.—Inaugural address.—
Dr. Hammond's case of moral depraviiy.—Lead poisoning in frogs by Dr. Mason,
and discussion.—Report of Dr. Jewell on the structure and functions of the ganglia
on the posterior roots of spinal nerves and discussion on the same.—Dr. Schmidt
on the structure of the sympathetic ganglionic bodies.—Dr. Mason, structure of
spinal cord of American bull-frog.—Dr. Seguin, sections of the spinal cord of the
green sea turtle.

Second Day's Proceedings: Election of members.—Dr. Emerson, syphilitic sciatica.
—Dr. Eugene Dupuy, hereditary epilepsy and hereditary deformities, and discussion
on the same.—Dr. Shaw, exhibition of a case of locomotor ataxia in a child.—Dr.
Hammond, odor of the human body, as developed by certain affections of the nerv-
ous system.—Beard, endemic tetanus.—Dr. Rockwell, intermittent hemiplegia, with
discussion on same.—Dr. Gibney, spinal irritation, and discussion.—Dr. Seguin,
post-hemiplegic chorea.

Third Day's Proceedings: Election of officers.—Dr. Shaw, brain Tumors, with dis-
cussion.—Dr. Dupuy, vaso-motor centres, and discussion of same.—Dr. Seguin,
cerebral localizations, discussion.—Dr. Dupuy, hereditary transmission of pecu-
liarities.—Dr. Beard, influence of the mind in disease.

Wednesday, June 6th.—Afternoon Meeting.

The American Neurological Association convened at the
College of the Physicians and Surgeons, in New York City,
June 6, 1877, and was called to order at 2:30 P. M., by the
President, Dr. J. S. Jewell, of Chicago.

Present : Drs. Jewell, Miles, Hammond, Mason, Shaw,
Emerson, Beard, Loring, Cross, McBride, Kinnicutt, Seguin.

As the minutes of the preceding session had been printed
and distributed among the members of the Association, on
motion, their reading was dispensed with.

REPORTS.

Dr. E. C. Seguin, of New York, Secretary of the Council,
made the Annual Report, which was adopted.

Dr. J. J. Mason, of New York, Corresponding Secretary of
the Association, made his report, which was accepted.

Dr. E. C. Seguin, Recording Secretary and Treasurer of the
Association, made his report, which was accepted.

At the recommendation of the Council, the resignations of
Dr. J. W. S. Arnold, of New York, and Dr. F. D. Lente, of
Florida, were accepted.

It was voted that the Recording Secretary present Dr. Will-
iam Detmold with the thanks of the Association for the use
of the room.

Dr. William A. Hammond and Dr. N. B. Emerson submitted new by-laws.

THE COMMITTEE ON NOMINATIONS,

as appointed by President Jewell, was as follows: Drs. Miles, of Baltimore; Shaw, of Brooklyn; Kinnicutt, Cross, and Emerson, of New York.

The business of the first meeting having been completed, the

INAUGURAL ADDRESS

of the session was delivered by the President, DR. J. S. JEWELL.

The following is an abstract of the address:

"GENTLEMEN OF THE AMERICAN NEUROLOGICAL ASSOCIATION:— At this our third annual meeting, it has seemed to me appropriate that I should deliver at its opening a short address, as a means of enabling me to discuss certain matters which can hardly find suitable expression at any other time, or in any other way. The nervous system in its totality, which is professedly the object of our consideration, has striking peculiarities as a field for scientific and practical study. From whatever stand-point it may be considered, more unexplored territory lies within the confines of the spinal cord, medulla, and brain than in any other part of the organism. There is not a part of the body, it is probable, into which it does not penetrate, and hence with which it does not establish relations. This is, on the one hand, for the purpose, so to speak, of being always informed as to the varying conditions of all parts of the organism, and on the other hand, for the purpose of exerting an influence upon the same. While it is, in the strictest sense a special field, it is so happily situated as to give those who enter and cultivate it in a rational spirit no excuse for that narrowness of thought and sympathy so often and so unhappily found among those who cultivate specialties.

"Such, in brief, is the field of our study. In its cultivation for the future there are several things I should be glad to see realized.

"The first is, that in this country, henceforth, more attention and encouragement may be given to a thoughtful study of the healthy anatomy and physiology of the nervous system. No doubt there are many now among us who are endeavoring to keep pace with progress in these fundamental de-

partments of neurological science, and some few are endeavoring to confirm or enlarge the boundaries of actually existing knowledge; but it cannot be denied that thus far very little, comparatively, has been done and made public, in our own country, towards advancing a knowledge of the normal anatomy and physiology of the nervous system. The reasons for its neglect thus far, whether good or bad, have now in a great measure passed away. The time has now come—and with it the opportunities—when we should undertake to make some solid contribution in this department of our work. To excite and encourage, and beyond this to prosecute such researches, should be one chief object of the existence of such a society as ours.

" Then again, we need not less experiment, but more care as to methods and results. There can be no question in my mind that there is, relatively speaking, too little close, accurate thought as compared with the mere observation of facts. The mere discovery of a new fact by sense observation does not insure that the discoverer will ascertain its significance. What I mean to declare is, that mere sense observation has outstripped, and does this very hour outstrip, critical, careful thought. What we need is, not mere observers nor mere thinkers, but more men, who like Bacon, Harvey, Bichat, and others, can not only observe, but, like them, think. I would be glad to see this society in its work avoid with a set purpose an ill-balanced course in this respect.

" With due respect to those who have labored in less favored times, I wish to see less reliance placed on the records of pathological cases, as they exist in the literature of the past. With a better knowledge of the anatomy and physiology of the body, with a broader range and basis of established facts than ever before, and with greatly improved methods for research, we are able happily to lay a surer foundation, let us hope, for trustworthy deductions.

" I wish now to lay before you a few practical suggestions, as to the organization and working of the Society.

" I am now, as I have been from the first, of the opinion that the society is about as large as it ought to be until it has lived a little longer and done more good work. I do not say no new members should be admitted, but for a time let them be

few, and admitted with caution. Then, again, the society
should continue to hold its next few meetings in the East.
The bulk of its members must for a long time be here, and
also the means for rendering them attractive. It should meet
either in this city, Boston, or Philadelphia, or what may be
thought better, in some adjacent place of resort. Then, again,
the time of meeting of the Society should be so changed as not
to conflict with other important meetings, which many of our
members might feel like attending. I would suggest that the
meeting be postponed till the second or last week in June, or
until September. Then in regard to the publication of our pa-
pers and discussions. The only things to be done are either to
publish an inexpensive account of the proceedings and abstracts
of discussions and papers, or to publish a volume of transac-
tions. Of these two plans, the latter is every way preferable.
A subscription of from fifty to one hundred dollars from each
member would completely insure the appearance of the volume.
To render the work of the Society more effectual, I would
recommend a diminution in the number of officers, who should,
as now, be aided by a council in discussing purely business
questions, which should rarely, if ever, occupy the attention of
the Society as a whole; and that the Secretary be charged with
the not very onerous duty of editing the materials, employing
such aid as he may need. I would also suggest the propriety
of the appointment of committees to report at subsequent
meetings on definite subjects.

"Such are a few of the suggestions which occur to me, and
are offered because of their importance. I had intended, at
first, to have made a survey of the general field of neurology
and to have indicated with some fullness and particularity the
direction and tendency of research, and to have mentioned
many of the yet open questions which appear to be within the
field of neurological medicine, but it has seemed to me the
suggestions which I have made would be of more importance
to the Association.

" I will conclude, therefore, gentlemen, in the expression of
something more than a hope, that we may, at this meeting,
make a new departure, and though our number is small, that
we may remember that the utility and renown of this Society

will not depend on its numbers, but on the character of its labors."

Next followed the exhibition of an interesting case, by Dr. WILLIAM A. HAMMOND. of New York.

The patient, a boy of 18, the Doctor said, might be considered as a case of total moral depravity. He would lie and steal to any extent, without any apparent reason; would steal clothes and other articles, sell them for a trifle, then give the money away. The boy had been placed in several houses of correction, but they could do nothing with him. In reply to the question, " What makes you do these things?" he said, " I can't help it." He did not seem to be lacking in intelligence. His mother had for a long time noticed blood upon his pillow. Dr. Hammond considered the case one of epilepsy. Weight of child at birth one pound. His father is an exceedingly neurotic individual.

REMARKS ON DR. HAMMOND'S CASE.

DR. SEGUIN. Do you know anything of his family history?

DR. HAMMOND. His father is one of the most neurotic subjects I ever saw. He has used tobacco to excess,—smoked and chewed, but now he does not smoke. While the mother was pregnant with this boy, she had a great shock, mentally.

LEAD POISONING IN FROGS.

DR. J. J. MASON, of New York, read a paper on this subject which suggested a new field for experiment.

Notwithstanding the important services which experiments upon this animal have rendered, in discovering the mode of action of other toxic substances, not even an allusion to the action of lead upon the system of the frog, can be found in toxicological literature.

The subject was divided into

1. Acute poisoning: 2. Chronic poisoning.

In the former, the poison (acetate of lead) was introduced under the skin; in the latter, by placing the animal in a solution of the same salt.

The acute form of poisoning is charactized by paralysis of the heart with preserved integrity of the motor nerves and muscles; while, in the chronic form, paralysis of the muscles of volition, with their nerves, invariably results, leaving the heart intact.

The muscles always show Erb's *Entartungsreaction* to electricity, a characteristic feature of lead palsy in man. A ready method is here found of inducing this condition of the muscles at will, and its value in the study of pathogeny of lead paralysis suggested to the consideration of the Association.

In view of the little we know on this subject, and of the difficulty of obtaining autopsies in man and failures to induce the same condition in warm-blooded animals, may not much be added to our knowledge by further research on lead poisoning in the frog?

The paper being open for discussion.

<center>REMARKS UPON DR. MASON'S PAPER.</center>

Dr. HAMMOND. I would like to ask if there was any evidence of lead in the nervous system?

Dr. MASON. I could not say as I have made no such examination.

Dr. CROSS. In the human subject lead has been actually found.

Dr. JEWELL. In which form of poisoning, acute or chronic, did the paralysis occur?

Dr. MASON. In the chronic.

Dr. JEWELL. In the acute form without the muscles having lost their excitability, was there paralysis of the heart?

Dr. MASON. Yes sir. I have never noticed paralysis of the heart in the chronic form.

Dr. JEWELL. You spoke of opening the heart, and that it began to act again, was its action spontaneous?

Dr. MASON. Yes. The rapidity of the pulsations was about 24 per minute.

Dr. JEWELL. How does that compare with the action of the heart in the healthy animal, when it is extracted from the body when there is no lead poisoning?

Dr. MASON. I have seen the heart act, out of the body, three or four hours after death had apparently taken place.

Dr. JEWELL. In the case of the acute form of poisoning, was the heart removed from the body?   Dr. MASON. It was *in situ.*   Dr. JEWELL. You found no change in the spinal cord, and applied no chemical tests?   Dr. MASON. No, sir, but I wish to do it.

Dr. Jewell. I have known of a number of cases of paralysis in type setters. About a year or more ago, I had one case of this kind, and he has brought me word of seven or eight more cases of the same kind. Every one of them had paralysis, or paresis, and especially those whose hands were most commonly smeared with the material from the type. In one or two instances it seemed to me that nothing less than a deposit of the poison on the hands and fingers, the nerves could have explained the phenomena present. I have been in doubt in regard to these cases, and hope Dr. Mason will follow the matter up, and especially bring in micro-chemistry to aid him, so as to ascertain definitely whether the lesion is peripheral or central.

Dr. Hammond to Dr. Jewell. You speak of your cases as if they were lead poisoning, were they not cases of antimonial poisoning? I am inclined to think it was antimony instead of lead that caused the trouble. In regard to the central lesion, in lead poisoning, I think there is no doubt about that. Against the local absorption theory, I have had cases of lead poisoning from the use of hair dye, where the paralysis was not in the muscles of the face, but in the wrists, and in the case of painters, in the legs.

Dr. F. T. Miles. There are some cases which are very hard to explain by considering the trouble central, as for instance, paralysis in the lower extremities.

Dr. Jewell. I have no doubt in my own mind, that the trouble is central, but one or two of the cases which I had, it was a little difficult to explain the phenomena present, except by local absorption. It is true that in the cases I have cited, the poisoning was more probably due to antimony than to lead; but the phenomena were apparently local, and it appeared to me the trouble must be in the nerve trunk itself.

Dr. Seguin. I had the opportunity of making one observation bearing upon this point. The patient was recovering from lead paralysis at the time he was carried off by a diarrhea. I made an examination of the muscles, which showed the usual degenerative changes. A section through the cervical enlargement showed slight granular change in the ganglionic cells of the anterior horns. At that time, 1874, I took

strong ground in favor of a central affection, and am still disposed to this view, though not so strongly as before. As regards Dr. Jewell's cases, I think they were of antimonial origin. A remarkable fact in regard to lead poisoning is that it does not attack the interossei.

Dr. Hammond. In my experience printers are very rarely affected.

Dr. Seguin. I have only seen two cases of lead poisoning in printers. Dr. Hammond. It is quite rare.

Dr. Emerson. I had a case in which the muscles wasted were those of the thenar and hypothenar eminences.

Dr. Cross. I have given some little thought to this subject, and it has struck me that it was not possible to produce paralysis by mere contact with the hand, from the fact that you can have lead paralysis where there has been no contact, except by means of the respiratory organs, as in the case of smelters. I have seen cases where the muscles in use were paralyzed, and yet the lead had not come in contact with the outer skin, which seems to me strongly against the argument that it is by contact. I believe the affected region is central and high up. Another fact which favors the view of a deposit in the tissues in the case of lead poisoning, is that under the iodide of potassium treatment the symptoms rapidly disappear.

Dr. Mason. I should have seen how long a paralyzed frog would live. Dr. Seguin.—Was there wasting of the muscles? Dr. Mason.—No sir. Dr. Hamilton remarked that a few years ago, he had made some investigations in behalf of the board of health, bearing upon the subject. He had seen many cases of paralysis in printers, and had found lead paralysis exceedingly rare among them. Dr. Hamilton cited the case of a girl where the paralysis was, he thought, due to the constant use of the muscles. She was employed in dressing type.

Dr. Seguin, in reference to the remark which Dr. Cross made in regard to the respiratory tract, I would say that in paint shops, where the assistant painters are required to burn off old paint, previous to repainting, they are liable to lead poisoning from inhalation of the fumes and smoke. Dr. Seguin cited one case which had come under his notice. In lead man-

ufactories the poison was undoubtedly taken into the system by inhalation. Attention was called to the fact that workmen were inveigled into these manufactories without being warned of the dangers.

Dr. J. C. Shaw.—I have seen several cases such as Dr. Seguin has called attention to, in which the paralysis has occurred within a week after entering a lead manufacturing establishment.

Dr. Cross.—I would say in regard to the respiratory tract as a means of conveying lead poison into the system, that those who work in European lead mines, are directed to drink diluted sulphuric acid, and by this means the proportion of cases thus originating has been reduced. I would suggest to Dr. Mason, if he continues his investigations, to have the muscles, nerves and spinal cord examined microscopically and chemically, for by so doing he might be able to throw some light upon the subject.

D. Mason.—I have examined the nerves, and they appear to be healthy. As stated in the paper, I have not been able to find any change. I have at present, at least, a half dozen spinal cords undergoing the process of hardening for future microscopical examination.

The President, Dr. J. S. Jewell, then made a verbal report to the Association of the progress he had made in the examination into the

STRUCTURE AND FUNCTIONS OF THE GANGLIA ON THE POSTERIOR ROOTS OF THE SPINAL, AND ALSO OF THE CORRESPONDING PART OF THE CRANIAL NERVES.

Dr. Jewell's remarks were a continuation of a paper upon the same subject, read before the Association at the last annual session.

He stated that his researches were far from being complete, but to him were suggestive. An important question was, as to what became of the fibres which appear to rise from the nerve-cells found in the ganglia in question. Do they pass toward the cord or toward the periphery of the body? The Doctor's first opinion was the same as that held by many others, since the time of the early observations of Koelliker— that they pass toward the periphery—but he has since aban-

doned that opinion. His opinion now is, that they do not pass either way, but that they join the axis-cylinders of the fibres of the sensory root, at the so-called, "constriction of Ranvier," as these fibres pass through the same ganglion. This view he was first led to entertain by seeing preparations of these ganglia made by Dr. Amidon, of New York, and after reading the account of these bodies, given by M. Ranvier, who describes what he calls the "*terminaison en T.*" This mode of termination of nerve-fibres in other nerve-fibres, Dr. Jewell has since ascertained, had been described by R. Wagner, of Goettingen.

Dr. Jewell has abandoned his opinion that the nerve cells of the ganglia give off two processes, which connect with either two nerve-fibres, or one other cell and a fibre, or with two cells. He now believes that they are connected with one fibre, and that fibre connects, as already described, with the axis-cylinder of a sensory nerve-fibre, as it passes through the ganglion. But for what purpose does this connection exist? This question, Dr. Jewell thinks, is fully answered by making sections, in the living animal, of the posterior or sensory root, at one time between the cord and ganglion, and at another on the peripheral side of the ganglion. In either case Wallerian degeneration sets in, but in a curious manner. In the case of section on the central side of the ganglia, the degeneration is toward the cord, not toward the ganglion, while in the case of the section on the peripheral side of the ganglion, the degeneration of the nerve-fibres takes place toward the periphery, and not toward the ganglia. These observations show conclusively that the ganglia exert a conservative influence over the fibres of the sensory nerves throughout their whole length, from the periphery to their implantation in the spinal cord. Here, then, we have a highly probable determination as to the function of the nerve-cells in the ganglia on the posterior roots of the spinal nerves. They exert an influence on the nutrition of the fibres of the sensory root. Their function is trophic. They are part of a *trophic nervous system.*

Another question arose as to whether these same ganglionic bodies do not exert an influence on the non-nervous tissues of

the body, through the sensory nerves with which the spinal ganglia, on the one hand, and the ultimate anatomical elements of the tissues, on the other, stand in such intimate relations. Dr. Jewell announced that it was his conviction that such is the case. The nutrition of most parts of the body is, to a certain extent, and in a certain manner, under the control of the mechanisms found in the ganglia. Dr. Jewell did not want it understood that it was his opinion, that the nutrition of the body *depends* on either the spinal ganglia or any other part of the nervous system, but that it is to a certain extent under its control. Disease of the ganglia, and also of the spinal cord, may lead to such changes in nutrition as to produce the so-called idiopathic inflammations we so often witness in the skin and other parts of the body, and indeed any cases involving nutritive change, side by side with changes in blood-supply, which cannot be fairly connected with a local injury, mechanical, chemical, or otherwise. As regards the vascular changes which follow in the wake of irritative tissue change, of presumed neurotic origin, they are to be explained on quite different grounds, since they occur through the agency of a different class of nervous mechanisms. It was Dr. Jewell's opinion that the spinal cord contains in connection with the medulla, and possibly the brain, a central mechanism, which may be properly called trophic, and that the spinal ganglia probably bear the same relation to it that the ganglia on the fundamental chain of the sympathetic, so-called, do to the central vaso-motor mechanisms of the cord and medulla. Dr. Jewell stated that it would not be possible for him at present to go at length into the reasons for his beliefs, though he would certainly do so before long. He had simply desired to report progress.

Dr. Jewell expressed a desire to have the subject of his paper discussed.

REMARKS UPON DR. JEWELL'S PAPER.

Dr. HAMILTON inquired the way in which atrophy occurred.

Dr. JEWELL replied, using the blackboard to illustrate his remarks, for instance as in progressive facial atrophy, a certain nerve-fibre, I will suppose, terminates centrally in the cord by two or more filaments; I suppose this fibre to be connected,

directly or indirectly, with several distinct groups of cells. One of these groups is supposed to have a trophic function, another has a sensory function, another group some other function; now it is possible to have this trophic group the seat of a disease which shall exert an influence along the fibre, with which it stands connected, while its sensory function remains intact, because its sensory group of cells remains normal. My belief is that the sensory nerve-fibre conducts all kinds of sense impressions, after the same fashion as the wire of the telephone which is used to convey several messages in the same direction or in opposite directions, at the same time. This supposed trophic tract in the cord, I expect to see sometime demonstrated. Only in the way I have indicated can I explain certain local inflammations or local atrophies which take place without apparent cause, and seem to follow in the wake of nervous disease. Dr. HAMILTON.—I simply spoke of this because of its interest.

Dr. MASON.—You believe that to be a motor fibre? (Pointing to Dr. Jewell's diagram.) Dr. JEWELL.—No sir, for sensory fibres go to the muscles. Dr. SEGUIN.—There is one fact which makes it unnecessary for Dr. Jewell to suppose that sensory fibres go the muscles. I have one case of progressive facial atrophy in which I cannot admit that there is any muscular atrophy. The changes are chiefly in the skin and bones. Dr. HAMMOND.—There is a difference between simple atrophy and degeneration. Dr. CROSS.—You cannot tell whether there be anatomical atrophy, by means of the electrical current. Dr. SEGUIN.—The reactions in my case were so absolutely natural, that I could not admit the existence of any pathological atrophy. With respect to Dr. Jewell's theory, Dr. Mitchell has advanced the view that in cases of alteration of nutrition, the lesion is an irritative one, and this view has also been held by Dr. Brown-Séquard and others. In Dr. Brown-Séquard's laboratory, I have repeatedly seen Guinea-pigs whose sciatic nerves had been cut, remain indefinitely without ulcerations of the feet, but if these animals were neglected, and the feet allowed to remain in filth and urine, frightful ulcerations ensued.

Dr. Hamilton.—Some of the most interesting changes occurring in joints, are those found in locomotor ataxia.

Dr. Seguin.—I believe that M. Charcot has tried to show, and in one case has demonstrated that the anterior horns were the seat of lesion when arthorpathy had been present. In those cases the alteration of nutrition is not connected with the lesion of the ganglia.

Dr. Hamilton.—The cases which I referred to were those in which there is no paralysis. Dr. Jewell.—In regard to the character of the disorders in the nervous system that give rise to inflammations, I have no doubt that they must be irritative.

* * * I have said that there is probably a central spinal apparatus which is brought into play, for the regulation of the production of heat, which is summed up possibly in the medulla oblongata; that this apparatus probably exerts an influence on the nutrition of the body. The central apparatus, if it exists, I have no doubt is in the spinal cord. Some idea of this mode of action may be obtained by analogy. You can excite the secretions of certain glands by irritating their nerves, even after the nerves going to them have been cut. Now the changes going on in a gland in the act of secretion, appear to me to be of the nature of nutritive acts, but they are under the control of the nervous system. In my own mind there is a strong conviction that there is a portion of the nervous system set apart for the control of the nutrition of the body.

Dr. Seguin. It seems to me that there is one condition that escapes from Dr. Jewell's mind. As I understood him, the supposed irritation at this point in the sketch, causes an influence to go along the nerve trunk, and in that way sets up a change in nutrition. Opposed to such a theory as that is the fact that the nerve trunk in four days after injury, loses its power to conduct irritations, that is after complete section. Unless we suppose regeneration to take place, conduction could not occur.

Dr. Jewell. In reply I would say, that such cases as I refer to, would not be represented by that supposed by Dr. Seguin. I am not referring to divided or mechanically injured nerves so much as to those which arise from the cord, at some point, where the gray matter with which the fibres stand re-

lated, is in a state of *irritative disease.* I suppose an influence to be exerted from the seat of disease; just mentioned, through the fibre, on the nutrition of the parts to which the fibre in question goes, which causes disturbances of the nutrition of the part. *Irritative* disease of a nerve trunk, or of a spinal ganglion, I suppose to be be followed often by similar results. It is not necessary in such cases to suppose Wallerian degeneration, or any other form, which destroys the conducting power of a nerve.

Dr. MASON. There is one point I have not settled, in regard to your remarks. How does it make the matter any more clear to conceive of an irritation starting in a single cell T, in your diagram, than to think it starts in the lungs, or that it starts in any number of cells, or without any nervous impression? It is just as easy for me to think of an irritative influence starting in one place as in another; it must start somewhere.

Dr. JEWELL. I do not see how a local so-called idiopathic inflammation can be excited in a protected part, as it often is, unless by means of some disturbing influence conveyed to it by the way of its nerves. If certain parts of the nervous system are capable of exerting an influence on the nutritive activities of related parts of the body, after the manner of certain other parts of the nervous system, on the muscles or secreting organs of the body, I do not see why it is not easier to imagine local irritations arising in this way in non-nervous parts, than *de novo.* Certainly, a nervous impulse passes along a nerve to a muscle, which makes it contract, or along a nerve which modifies the action of a gland, and it is in some such way, that I suppose, the nervous system to act in modifying the nutrition of a part. Why refuse to believe it possible for the nervous system to exert such an influence? This whole matter of a " heat regulating" function of the nervous system requires it to exert such an influence.

First Day—Evening Session.

The Association was called to order by President Jewell, at 8 p. m.

Present: Drs. Jewell, Miles, Shaw, Emerson, Mason, Kinnicut, Beard, Rockwell, and Seguin.

The first paper was by Dr. H. D. Schmidt, of New Orleans, who was unable to meet the Association in person the present session. The paper was read by the Secretary, Dr. Seguin. The communication was accompanied by a number of thin microscopic sections, of different ganglia of the sympathetic nervous system, and also some sketches of ganglionic bodies.

The object of Dr. Schmidt's communication was not only to offer an opportunity to those persons willing to devote the necessary time to the examination of the specimens, of convincing themselves of the truth of his former statements, but moreover to direct the particular attention of the Association to the structure of these bodies, each of which seems to represent a complete nervous apparatus in itself, and, also, to the importance attached to a true knowledge of the particular mode in which the sympathetic nervous system operates. As the investigation of the structure of these bodies is one of the most difficult in histology, the Doctor considered it necessary that the investigator, in order to recognize this structure, should be able to represent in his mind the exact form in which sections of a complicated body of a certain form, made in different directions, would appear to the eye. Dr. Schmidt called attention to the fact that in examining the ganglionic bodies, lodged between the double contour nerve-fibres of the *plexus gangliformis* of the pneumogastric nerve, the meshes of the net-work forming the capsule, would be found much coarser, a fact which rendered these bodies very suitable for research. Attention was called to the fact that from each sympathetic ganglionic body, two different kinds of processes arise; the larger of these, having the appearance of true axis-cylinders, are most probably transformed into double contour nerve-fibres, while the finer and shorter ones contribute to the formation of the nervous net-work of the capsule, from which the fine fibrillæ, forming eventually the sympathetic nerve-fibres, arise. Thus we have two different kinds of nerve-fibres arising from one and the same ganglionic body. Now the question arises, does this arrangement bear any relation to the rhythmical or peristaltic action of the involuntary muscles? And

further, is one kind of nerve-fibre motor while the other is inhibitory? If so, which are the sensory fibres? Another question to be answered is, whether those sympathetic ganglionic bodies, with the nerve-fibre arising from them, lodged in the plexus gangliformis of the pneumagastric-nerve, bear any relation to the inhibitory action of this nerve? Dr. Schmidt's view is that difference in structure conditions a difference in function.

REMARKS ON DR. SCHMIDT'S PAPER.

Dr. Jewell. This finished drawing of Dr. Schmidt's is almost like one by Dr. J. Arnold, of Heidelberg. Dr. Schmidt's labors have been conducted in the South, at a time when he could not have had very much literary aid, and hence he may not have seen Arnold's paper. This peculiar reticulated appearance, Courvoisier and Schwalbe, do not explain, as do Arnold and Dr. Schmidt, that instead of being protoplasmic threads meandering over the capsule of the cell, the reticulated appearance is probably due to the lines of apposition of epithelial bodies which cover the ganglion cell walls.

Dr. Seguin. It seems to me, Mr. President, that there have been a good many illusions of vision in looking at nerve cells. I have looked at ganglion cells a great deal, and have been unable to recognize many of the figures described by certain writers. I think they have been figured wrongly. I refer to the nucleus and nucleolus threads which are described by some. When I had the privilege of studying in Prof. Max Schultze's laboratory, I asked him in regard to these points, and he said that he had never seen anything of them, and considered them an illusion. He considered the various appearances as due to fibrillation, and he explained to me orally that working with poor instruments had made this mistake. And so, too, about the cells with respect to the connection of the axis cylinder with the nucleus: Some have figured cells with an axis cylinder beginning in the nucleus, all of which Dr. Schultze assured me he had never seen.

Dr. Miles. Is there not a breaking up of the axis cylinder into fibrillæ?

Dr. Seguin. I have looked very faithfully for these, but

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have never seen them, even with an immersion 15th of Wales.

Dr. Jewell. Dr. Schmidt has some material on this subject. That Dr. Schmidt is an accurate and able observer, I believe no one will doubt. He says he has found rows of granules which seem to be the early axis cylinder, so disposed as to reach the nucleus of the cell. Arndt figures very nearly the same thing, after careful research, and he represents the axis cylinder as breaking up into a sort of spray, extending quite as far as the nucleus, so as to partly obscure it.

Dr. Mason then gave an informal demonstration of the structure of the spinal cord of the American bull frog, and the medulla oblongata and spinal cord of the alligator, by means of fresh and permanent microscopic specimens. Several microscopes were provided for the purpose.

Dr. Seguin showed a number of transverse sections of the spinal cord of the green sea-turtle, exhibiting remarkably long cell processes proceeding from the anterior horns into the white columns. There were also shown three photographs of the same specimens, made by Dr. J. W. S. Arnold.

At 9:20 p. m. the Association adjourned to attend a reception at the residence of Dr. William A. Hammond, to which he had invited the Association and a number of the profession in New York.

TUESDAY, JUNE 7TH.—AFTERNOON MEETING.

The Association was called to order at 2 o'clock by President Jewell.

Present: Drs. Jewell, Miles, Hammond, Mason, Dupuy, Emerson, Beard, Rockwell, Shaw, Kinnicutt, McBride, Hamilton, Loring, Seguin.

The Secretary read the report of the Council recommending the following gentlemen for election as active members: Dr. V. P. Gibney, and Dr. E. C. Spitzka, both of New York. These gentlemen were unanimously elected.

The first paper was read by Dr. N. B. Emerson, of New York, and entitled

SYPHILITIC SCIATICA.

Dr. Emerson called attention to the fact that syphilis is very

infrequently recognized as a cause of sciatica. Out of the reports of sixty-three medical men, in answer to Lauder Brunton's questions in regard to sciatica, only twelve spoke of the syphilitic form.

There was sufficient clinical and post-mortem evidence for making the statement that syphilitic neoplasmata are capable of producing violent pains and loss of motor function in the sciatic nerve by pressure. Cases were given to substantiate this point. The Doctor considered it established that the syphilitic diathesis is capable of producing neuralgia in the sciatic nerve in some occult manner, without lesion. His reasons are, in brief, as follows:

1. Cachexia is acknowledged to be a frequent cause of neuralgia.

2. Syphilis is a notable cause of cachexia.

3. Syphilitic cachexia and neuralgia are frequently found to coexist, and with relief of cachexia comes relief of neuralgia.

For a diagnosis of syphilitic sciatica, the patient must have been free from sciatica previous to the syphilitic attack; an attack in the late stage of syphilis is more significant than in the early. The prognosis of syphilitic sciatica is the same as that of the syphilis which causes it. The treatment is practically that of syphilis; opiates may be necessary for the relief of pain; ferruginous tonic remedies should be made use of for the relief of the cachexia.

There was no discussion on this paper.

The next paper was read by Dr. Eugene Dupuy, of New York, on

HEREDITARY EPILEPSY.

It is well known that Dr. Brown-Séquard has discovered that certain lesions of the spinal cord, or the brain, or the sciatic nerve, in Guinea-pigs, will give rise to an epileptic malady in these animals. In from three to six weeks after the operation, it is found that an alteration in the nutrition takes place in an area of skin, which is limited by a line starting from the outer canthus of the eye, and running to the median line on the upper lip enclosing the nostril, thence backward, enclosing the lower jaw to the anterior portion of the

shoulder to the median dorsal line, to the base of the ear and inner canthus of the eye. The alteration in nutrition takes place on the side corresponding to the injury. It consists in this, that the faculty of feeling pain, heat and cold, disappear by degrees, while tactile sensation appears to be exalted. In a few days it is found that tickling this zone of skin will give rise to twitching which are limited to the muscles of the eye and the eyelids on the same side. Later, the muscles of the mouth and of the face partake; still later the contractions become more general, until this whole side becomes the seat of convulsions; then the convulsions attack the other side also.

When things have come to this point the convulsions precede, by a very short time, a complete loss of consciousness. If the subject of experiment be a white Guinea-pig, it is found that there is paleness of the face, but in all cases there is a little foam at the mouth, and dilatation of the pupils. In some cases the animal utters a cry, probably corresponding to the epileptic cry in the human species. Not only are the convulsions identical with those in epileptic man, but there is also loss of consciousness, a state of torpor, stupidity, and even in some cases something like insanity.

It happens that such animals recover spontaneously, and in so doing, all of the phenomena described above occur in a reverse order, and the zone of skin regains its lost functions. When the epilepsy is due to the destruction of the sciatic nerve, the foot of that side loses the two outer toes, so that the animal has only one toe, the inner. When young are born to such a parent or parents, (for it matters not whether one or both of the parents have been operated upon) they have this peculiarity of having only one toe on the posterior foot. Sometimes, however, they have additional toes, which in this case are attached by a pedicle to the limb.

Now, all of those peculiarities which have been observed in the parents, all things in all their details, are witnessed also in the Guinea-pigs, hereditarily born toeless, who have developed epileptic phenomena. There is, therefore, an inheritance of a power to develop the disease, but no inheritance of the disease itself. Dr. Dupuy has examined the sciatic nerve of such animals, and found them healthy, before, during, and after the

existence of the disease. He has also followed these experiments through five generations.

Dr. Dupuy made allusion to the doctrine of Balbiani on embryogeny, and thinks that, according to Balbiani's laws, the phenomena of inheritance, in the case of epilepsy, can be explained, epilepsy being a malady of nutrition like all other nervous diseases. Dr. Dupuy stated that only those young which are born with alteration in the normal nutrition of parts become epileptic; in such the disease fatally occurs.

The paper being open for discussion.

Dr. Jewell. Did you say that those anmials upon which you had operated invaribly transmitted their deformity? Dr. Dupuy. No not invariably, but it is so sometimes.

Dr. Hammond. I have been astonished at the small proportion of transmitted cases of epilepsy in the human species. Although its hereditary nature is undisputed, yet I do not think it is by any means so very common as is generally supposed.

Dr. Loring. I would like to ask in regard to the defect in the toes. I would like to ask, whether in following out five generations those two toes were absent, or whether the tendency was towards the normal number? We know that in the case of the Chinese women, no matter how badly the feet are deformed, no deformity is found in the feet of their children. I ask this question as bearing upon my own specialty, in regard to the inheritance of diseases of the eye: it has never been proven that any hereditary tendency has been produced, and I would like to know Dr. Dupuy's experience? Also if by simply cutting off the toes the defect would be transmitted? And in the case of circumcision, whether there is any alteration in the prepuce?

Dr. Dupuy. I thought that I said that such deformities were very rare. It appears that to have a lesion transmitted it must cause an alteration in nutrition. The tendency was of course towards the normal number of toes, since such a small number of the young have the deformity.

Last year I read a paper in the " International Ophthalmological Congress," which met in this city, in which I showed that the phenomena followed a lesion of the sympa-

thetic nerve, or the corpora restiformia, in the Guinea-pig, are invaribly inherited by their young, and it is known that these lesions modify the organs of sight.

In reply to Dr. Loring's question as to whether the simple act of cutting off the toes would be inherited, I would say, I have not tried that, but it is known that in dogs, when their tails have been cut off, sometimes their young are born without tails. In cutting off the toes of the Guinea-pig the lesion is so small as not to cause any disorder of the nervous system. As regards the Chinese and circumcision, we know nothing positive in regard to those, as no body has ever made examinations, it is impossible to say whether there is any change in those parts. I must say that that argument has been constantly put forward. As for fingers I know a lady in England, who lost her ring finger and little finger, and sometime afterward gave birth to a female child, in whom the same fingers were absent.

Dr. Loring. Do you know of any cases where deformities produced by amputation, have been transmitted? Dr. Dupuy. I know of two cases where amputation of the feet have been transmitted.

Dr. Seguin. It strikes me, Mr. President, in regard to the question put by Dr. Loring, that we must take into consideration the fact that the human subject presents much more resistance to these influences than the lower animals; still Dr. Dupuy's facts are of importance, because by their aid we can establish an argument by analogy with proper restrictions.

Dr. Spitzka. I have read in Obersteiner's essay, that those animals which are most likely to become epileptic, are those which resemble their parents in color or hair. We know that in insanity the chances of transmission are greatest when the resemblance is greatest. The experiments of Westphal led to the view that epilepsy depended upon hemorrhage in the medulla oblongata.

Dr. Dupuy. The experiments of Westphal are very rough and brutal; he used to knock the heads of his Guinea-pigs against some hard object and of course extravasation of blood took place, not only in the medulla oblongata but also in other parts of the centres. In the Guinea-pig there is a point at

the base of the brain, the mere pricking of which, induces an immediate attack of epilepsy. Now it may be that Westphal, in his experiments, knocks the pigs in this place and an attack of epilepsy ensues. I have had over one thousand Guinea-pigs under observation, and I have only noticed transmission seven times. I do not lay much stress upon the results of Obersteiner, because his results show transmitted epilepsy too often.

Dr. Spitzka. I simply repeated what I had read in Obersteiner. As regards the *modus operandi* of Westphal's experiments, whether they are brutal is not the question, but whether they do cause epilepsy. About seven per cent. of all human cases of epilepsy are traumatic. I think there is no doubt but what in Westphal's experiments the cases were epileptic. In regard to the third point or Dr. Dupuy's stating that the cell of Balbiani is more important than the cell of Purkinje, I do not admit that.

Dr. Dupuy. As for peripheral lesions causing epilepsy, I can make no ratio, although cases are not rare. In regard to Balbiani's cell, I would say that all biologists have accepted his views, moreover, I beg leave to state that it is taught, that Balbiani's cell and Purkinje's cell are two different things. The first carries ancestral tendencies and the second, maternal; that is based upon the history of the development of those bodies; moreover they fecundate one another and have an independent existence: they afterward retrograde if male fecundation does not supervene.

The germinal vesicle is now determined by embryologists, especially Oellacher and His to have no relation to embryonic development whatever, as soon as fecundation takes place it is driven to the periphery of the yolk, collapses, and becomes an inert body. The collapse is a process of retrogression which only supervenes if no male fecundation occurs.

Dr. Mason. I have seen Prof. Westphal perform his experiments, and I think others place more stress upon his experiments than he does himself. From what he has said to me, I think he looks upon the experiment more as a joke than anything else.

Dr. Jewell. There is one point I think, which is left out

of consideration by Dr. Loring, in his remarks about the feet of Chinese females and similar cases: no deformity is transmitted except where some of the parts have been removed. A distinction should be made between those changes which are merely morphological and those which are structural.

Dr. Dupuy. The Bretons have been in the habit of putting out of shape the heads of their children, for ages, as shown by fossil crania and they practice the same up to this date, yet their heads are not congenitally deformed.

Dr. Jewell. In considering the influence which the nervous system may have upon intra-uterine peripheral changes, especially where there is loss of members, it should be remembered that the peripheral nervous system makes its appearance first, and hence before the spinal cord could produce any influence upon the members. This fact has occurred to me as of considerable importance, especially in the case of those monsters which a perfect peripheral nervous system, and the spinal cord and brain undeveloped. That these peripheral parts have taken their shape, before the central nervous apparatus can exert any influence, is a point which should be taken into question in considering changes which take place *in utero*, that is before the central nervous system has been developed sufficiently to enable it to exert an influence on non-nervous parts.

CASE OF LOCOMOTOR ATAXIA IN A CHILD.

Dr. J. C. Shaw, of Brooklyn, exhibited a very interesting case of locomotor ataxia in a female child four years of age. When eighteen months old she had measles and scarlatina, and in a few months afterward she began to experience difficulty in walking, followed by an inability to hold things in her hands. This difficulty increased until she was unable to walk. She now presents marked ataxic movements in both of the upper and lower extremities. When the child was supported, and she attempted to walk, both of the lower extremities were jerked forward and outward, and the heel brought down first, in a truly ataxic manner. There did not appear to be any anæsthesia. It had been observed that at times the child would scream out without apparent cause. On May 2, 1877, she screamed out suddenly and said that she

had a pain in her left heel. She has no muscular atrophy or paralysis, and the muscles react normally to faradization.

Dr. Shaw stated that the diagnosis was made by exclusion, and gave his reasons for excluding, brain tumor, lesion of the ganglionic cells of the anterior horns, paralysis from peripheral irritation, and pseudo-hypertrophic paralysis; and advanced an argument to show that it was a case of sclerosis of the spinal cord—disseminated, perhaps, and located in the lumbar part of the cord, and in the posterior columns.

REMARKS UPON DR. SHAW'S CASE.

Dr. Seguin. I am not aware of any cases of locomotor ataxia younger than twenty years, except Friedrich's cases, which were probably cases of disseminated sclerosis.

Dr. Hammond. I think it a little doubtful in regard to existing ataxia in the upper extremities of this patient. The defective vision is competent to account for the motorial disturbance observed.

Dr. Shaw. I would like to ask Dr. Loring if it is possible, from the appearance of an atrophied optic nerve, to say whether there had been choked disk at any time?

Dr. Loring. It is often very difficult to determine in a given case of atrophy of the optic nerve, whether this has been preceded by choked disk or not. There are, however, some ophthalmiscopic signs which are, if present, of some assistance in determining this question. With choked disk there is usually a considerable amount of swelling of the nerve, together with a large amount of hypertrophy of the connective tissue, which after the active stage of the inflammation has subsided can be traced, as fine bands along the vessels, not only in the head of the nerve, but also into the adjacent tissue of the retina. Moreover, in atrophy from choked disk the edge of the nerve, instead of being sharply cut, is more or less indistinct, has a ragged appearance, its contour being dotted here and there with the remains of choroidal pigment. From what I can judge, that little child has not much interference with vision. I would ask Dr. Shaw, if there was any evidence of a limited field of vision?

Dr. Shaw. Judging from the child's actions, I should think there was no limitation to the visual field.

Dr. Hammond.—The youngest case of ataxia that I have ever seen, was 25 years of age.

The next was a paper read by Dr. William A. Hammond, of New York, which was, as Dr. Jewell afterwards remarked, interesting from a scientific, as well as from a theological point of view. The title of the paper was,

THE ODOR OF THE HUMAN BODY AS DEVELOPED BY CERTAIN AFFECTIONS OF THE NERVOUS SYSTEM.

Dr. Hammond called the attention to some facts in regard to the natural odor of the body in the human species, and of the faculty which some of the lower animals possessed,—that of differentiating between the odors of different individuals. Besides the inherent odor of the body there was reason for believing that an entirely different one may be given off, not only as a consequence of disease, but as a result of emotional disturbance. During the middle ages, manifestations of the kind in question were not uncommon in the persons of both sexes, and were attributed to miraculous power. That such cases existed was probable, not, however, as a special gift of God, but as a neurosis similar to other instances which had come under Doctor Hammond's own observation. Cases were then cited, of a number of the more important instances among the saints, who were considered highly odoriferous. So far as the author of the paper was aware, there had been no attention given to the subject in the relations now under notice. The cases cited by Dr. Hammond as bearing upon this point were briefly as follows:

A young married lady of strong hysterical tendencies, from whom, during a paroxysm, an agreeable odor, similar to that of violets, was exhaled only from the left lateral half of the anterior wall of the chest. At such times the perspiration was remarkably increased in this region, as compared with the corresponding part opposite. The odor was perceptible at a distance of several feet, but was entirely absent during the intervals of the paroxysms. From an examination of an alcoholic extract of the odoriferous perspiration exhaled by this patient, it was presumed that the odor was due to the presence of butyric ether. The local application of several remedies to the parts, among which were preparations of carbolic acid,

soap and water and other alkaline substances, gave the patient only temporary relief from the odor; but the internal administration of the salicylate of soda, in doses of five grains, entirely cured this lady of her violaceous odor, and the perspiration of the region was reduced to the normal character.

A second case was that of a young lady in whom the first exhibition of the odor (in this case, that of pine-apple) occurred contemporaneously with an attack of chorea.

In a third case, a pine-apple odor was emitted from the skin of the head, neck, and chest of a woman whenever she was angry.

A fourth case was that of a man who, during frequent hypochondriacal periods, emitted a violaceous odor. Occasionally cases were met with from whom a disagreeable odor was exhaled during sexual excitement. No opinion, as to the actual and immediate cause of these odorous emanations, was expressed, further than that they were due to a nervous disturbance.

Dr. Hammond passed around a small vial containing an alcoholic extract of the odoriferous perspiration of his first patient, which had a distinct violet smell; also a second vial of the same extract, with the addition of bicarbonate of soda, smelling strongly of pine-apple.

Dr. Beard.—I was very much interested in Dr. Hammond's paper, and especially with the treatment. I have lately had a case where there was (a profuse) odor from the armpits. A case of profuse perspiration in the hands under care of Dr. Sterling, was cured by injections of atropia. Speaking of those historical cases mentioned by Dr. Hammond, I do not know that you can call them facts. It is said that Alexander the Great had a pleasant odor about his person; but there is much of fallible human testimony in regard to all such claims.

Dr. Jewell.—I think one thing can be said in regard to Dr. Hammond's paper, and that is, it has furnished a new means of disposing of the phrase "odor of sanctity." Believing as I do that the secretions are largely under the control of the nervous system, I do not see why matter from the skin may not acquire certain odors, from a nervous causes. The subject has undoubtedly not secured the attention that it ought to have.

Dr. Hammond.—Mr. President, Dr. Wright informs me that he found in the case of a woman, the odor of asparagus, not only in the urine but in the perspiration also.

Dr. Seguin.—I was told by Dr. Brown-Séquard, that the wife of a French physician, emitted a strong general odor after sexual intercourse, and this led to the detection of her adultery.

Dr. McBride.—Dr. Priestmann, of Nicolaeff, reports in the *Med. Cent. Zeitung,* No. 2, 1877, that, for about six hours after coitus, a peculiar odor is discernible in the breath. In support of this, he cites the case of a man arrested for alleged rape, two hours after, it was stated to have taken place. On account of the absence of the characteristic odor, Dr. Priestman swore positively that the accused had not had connection within six hours, and further investigation confirmed this opinion.

Dr. Weir Mitchell reports in the *Am. Jour. Med. Sciences,* July, 1873, that a curious odor of the breath is perceptible in many cases of meningitis.

Dr. Seguin. My father is a believer in the possibility of diagnosticating certain diseases by smells. Sir William Gull, in the Pathological Society of London, has expressed the opinion that syphilitic subjects have a peculiar odor.

Dr. Hamilton cited the case of one gentleman, an Inspector of the New York Board of Health, who could go into the lower hall of one of the large tenement houses, and tell, by sense of smell, whether there was a case of small-pox in the house or not.

Dr. Jewell. There is one disease, namely, *milk sickness*, in which the odor is very remarkable; any one who has ever smelled it will never forget it. Of course it is impossible to describe it, unless there is some other odor like it, with which we are all familiar.

In regard to these odors, a great deal will depend, I think, whether they be general or localized, as for instance, an odor, not from the armpits, but from some unaccustomed part of the body, then the presumption will be strongly in favor of a nervous disorder, but if general, it may probably have no relation to the nervous system.

Dr. Spitzka. In measles there is an odor like that of freshly picked geese.

Dr. Hammond. In answer to President Jewell's remarks, I would say that the odor was limited in the case I have mentioned, to the left lateral anterior part of the chest.

After the close of the discussion, Dr. George M. Beard, of New York, proceeded to read a paper on

THE ENDEMIC TETANUS OF EASTERN LONG ISLAND.

That tetanus had been more frequent in portions of Suffolk county, Long Island, than in other parts of the country, was well known. Dr. Beard had passed several of his summers there, had conversed with a number of the physicians and residents, and corresponded with nearly all of the physicians of the county, asking for facts rather than opinions.

Dr. Beard's conclusions up to statement are as follows:

1. For the last three-quarters of a century there has been an endemic tetanus in certain portions of Suffolk county. It consists of both the traumatic and spontaneous varieties, and affects animals as well as man.

2. This endemic abounds mostly in the towns of the south side, especially at the Hamptons, is less common in the central towns, and on the north side, and in Montauk does not exist.

3. The endemic has been on the decline for the past ten or fifteen years, and in the central portion (excepting Riverhead) no longer exists.

For the causation there are three conceivable theories,— geology, the use of fish on the land as manure, and dampness in the air.

The theory based upon geology and advocated by Dr. B. D. Carpenter, who has studied the subject, appears to be disproved by the decline in recent years. Geological conditions are constant factors. In favor of the fish-on-the-land theory is the occurrence of the disease in those localities where fish are most used, and the decline of the disease with the decline in the use of fresh fish as manure. Dr. Beard thinks, however, that the facts up to date seem to favor the theory of dampness in the air combined with the local dampness of the soil.

Dr. Beard would treat a case of tetanus by calabar bean

(English preparation), in small doses every hour or half hour, so as to affect the pupil, and at the same time apply ice-bags to the spine. As suggested by Dr. Carpenter, the patient should be kept absolutely quiet if possible. The local application of the oil of turpentine to all wounds, is practiced by a number of the physicians of Suffolk county, and is to be recommended. The apprehension, felt by New York surgeons that tetanus is likely to follow surgical operations in Suffolk county, is not justified by the facts as they now stand.

There were no remarks on this paper.

Second Day—Evening Meeting.

The Association was called to order by the President at 8 p.m.

Present: Drs. Jewell, Miles, Hammond, Rockwell, Emerson, Spitzka, Shaw, Beard, Gibney, Cross, Kinnicutt, Loring, Mason, McBride, Dupuy, and Seguin.

The first paper of the evening was read by Dr. A. D. Rockwell, which consisted of the history of a remarkable case of

INTERMITTENT HEMIPLEGIA.

A brief outline of the case is as follows: A stair-builder, aged forty-nine, in fair health, was seized one afternoon in July with dizziness, loss of speech, and complete paralysis of the left side, which lasted for twenty minutes, and then entirely disappeared. Similar attacks occurred every other day for three weeks thereafter, at about the same time of the day. One day about the middle of August he had a much severer attack at 11 a. m., which was repeated every day at the same hour until September 3d, and between 11 a. m. and 4 p. m. each day, during all this time, paroxysms re-occurred three or four times. In the more severe attacks, he was unable to walk or speak, but during the milder ones he could move with difficulty and speak indistinctly. Dr. Rockwell's treatment consisted of a mild séance of general Faradization, in conjunction with three two-grain doses of quinine daily. There was decided improvement

at once, and on the 25th of September, he was discharged as apparently cured. He remained so until December 4th, at which time the paroxysms re-occurred, and in a more violent form, and he died on the following day.

Post-mortem examination showed congestion of the surface of the brain; pia mater covered with a thin film of organized lymph from old inflammations; texture of the brain softer than normal; choroid plexus enlarged and cystic; basilar artery and part of the circle of Willis enlarged and atheromatous; mitral valves and liver in a condition of fatty degeneration; serous effusion at the base of the brain, but there were no arteries ruptured, no evidences of embolism or thrombosis.

As the above described pathological changes seemed hardly sufficient to account for the unusual symptoms and suddenness of death, Dr. Rockwell discussed at some length, the probable cause of the final result, and, by means of exclusion, thought it probable that death was due to a spasm of the vessels in the brain tissue, which rendered it unfit to discharge its proper function.

Dr. EMERSON. I would like to ask Dr. Rockwell, if he made observations on the temperature of his patient?

Dr. ROCKWELL. No, sir; I made no record of the temperature during treatment, but the temperature in the shop on the occasion of the fatal attack was found to be 105 degrees.

Dr. SHAW. I would like to ask, if there was any syphilitic tendency?

Dr. ROCKWELL. There was none.

Dr. HAMMOND. Were the attacks on the same side?

Dr. ROCKWELL. They were all on the same side.

Dr. CROSS. Were all of the arteries at the base of the brain examined?

Dr. ROCKWELL. I think none were overlooked.

Dr. HAMMOND. I am glad to hear the details of this case, for I have a patient under treatment (who presents similar phenomena): three weeks ago my patient was attacked with paralysis on the right side, did not lose consciousness, but called for assistance and went home; very soon afterward he had a second attack; he had three, four or five in the course of that afternoon, and the next day I was called in consul-

tation.   Upon my arrival, I found paralysis on the right side;
there was hypertrophy with dilatation of the heart.   While I
was at his bedside, he remarked to me that he was going to
have another attack; he had at least three or four while I was
there, which was about an hour; I then did not have an op-
portunity to examine with the ophthalmoscope; there was
arcus senilis on both sides.   He was treated with infusion of
digitalis.   He has kept up and has had no further attack.   He
has now, this afternoon, distinct hemiplegia on the right side.
His speech is pretty good, his mind is a little enfeebled, and
he can only walk by holding on to some one's arm.   The case
is, I think, similar to Dr. Rockwell's.   I mentioned the case
to Dr. Jewell, and he suggested at once the existence of
capillary thromboses.

I think that in this case hemorrhage must be excluded en-
tirely, as also in the case reported this afternoon.   It seems to
me that Dr. Rockwell's case and mine are probably analogous.
The eye-sight of my patient is perfectly good.

DR. SPITZKA.   Of course, in the absence of *post-mortem* evi-
dences, we are limited to conjecture, and, as far as I can gather
evidence, there are three things which can cause this.   One is
epilepsy; a second is partial thrombosis of the cerebral arter-
ies; third, spasm of the cerebral arteries.   The doctor then
cited two cases found in the insane asylum of Vienna.   Dr.
Spitzka expressed the opinion that ideas on thrombosis were a
little biased at present, and that many conditions of capillary
and arterial stasis, which deserved the name thrombosis, were
not so regarded.

DR. BEARD.   I had a case of intermittent hemiplegia in a
patient 35 years of age; he recovered and is well to-day.   I
never saw him in an attack.   The attack would last from a
few minutes to half an hour.   The trouble lasted one or two
years.   I made a diagnosis by exclusion of spasm of the ar-
teries, and thought that he would get well after taking a course
of electrical and other treatment.   The prognosis, if not the
diagnosis, was confirmed.

Two years ago a gentleman consulted me for a feeling of
numbness in the right, upper, and lower extremities, which
had existed in an intermittent form, he declared, for several

days. While present in my office he had several distinct partial hemiplegia attacks, each of a few seconds duration, during which there was a decided loss of power in the right upper and lower extremities. Later in the same day, he had several well marked aphasic attacks of very temporary duration, however. Twelve hours later there was developed a complete right hemiplegia, with loss of consciousness. Specific trouble being suspected, very large doses of iodide of potassium were given, and with the most satisfactory results, the hemiplegic symptoms with the aphasia partially yielding within forty-eight hours after the administration of the first dose. Under continuous treatment, the patient so far recovered as to be able to be about, at which time he passed from my observation. A temporary spasm of a cerebral vessel or vessels suggested itself at the explanation of the symptoms preceding the development of the complete hemiplegia.

Dr. Spitzka. My explanation was only for those cases which were not syphilitic; I suppose your case was syphilitic. Your case I would explain as due to a thickening of the vessels,—syphilitic neoplasms. In your case I would suggest that the inner coats of the arteries were roughened, and thrombosis took place with great rapidity.

Dr. Dupuy. I am exceedingly pleased with the view put forward by President Jewell. It is contrary to the statement of Prof. Charcot, whose theories have been so widely noticed by Dr. Jewell. I do not believe that the cortex has anything to do with the nature and the production of these phenomena in a direct manner. As for the re-establishment of collateral circulation, I must say that I disagree with our President; it never takes place on account of the peculiarity of the cortex circulation.

Dr. Spitzka. Did I understand you to say that the cortex of the brain had anything to do with the movements?

Dr. Jewell. I did not wish to be understood that it has to do *directly* with muscular motion; my opinion is, that while it does not form part of the motor apparatus proper, that the cortex is a part from which excitatory impressions do start, to be projected on the motor apparatus below.

There was no further discussion.

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The next paper was read by Dr. V. P. Gibney, of New York, and entitled

SPINAL IRRITATION IN CHILDREN.

The paper consisted of the clinical histories, together with remarks upon a number of cases which Dr. Gibney was wont to consider as cases of spinal irritation.

In the course of the lengthy and animated discussion which followed it was made evident, that there was some difference of opinion as to what spinal irritation really was, several of the members of the Association having expressed their disagreement with Dr. Gibney on this point. The question of local mal-nutrition was raised and freely discussed.

Remarks upon Dr. Gibney's paper, viz.:

Dr. Jewell. I will make a remark or two and then give way to others. I was much interested in the paper which has just been read, but many of the cases cited were not what I call spinal irritation; they are rather cases of spondylitis, that is inflammation of the joints between the vertebræ, the inflammation extending subsequently to the dura mater and sometimes passing into effect the cord itself. They seem to be more truly cases of spondylitis—leading to muscular and sensory troubles as a consequence of the disease, not of the spinal cord but of the dura mater. Seventy or eighty such cases are reported by Braun, in a recent monograph. The cases cited in the last paper are not, many of them, such as I am accustomed to call spinal irritation. It is important to define just what one means by the terms employed.

Dr. Hammond.—Mr. President, I am very much interested in the paper of Dr. Gibney, but I think the name is unfortunate because the cases are not such as are usually considered cases of spinal irritation. Indeed they have scarcely any analogy with these latter, except in the one respect of local tenderness. This, though perhaps, an essential feature of spinal irritation is, of course met with in various other diseases either of the vertebral column or its contents. I am very reluctant, Mr. President, to open up any discussion on a subject about which there is so much difference of opinion as exists in regard to spinal irritation. Such a discussion would, in my

opinion, be out of place for the name given that Dr. Gibney's cases are not instances of that affection.

Dr. Dupuy.—Perhaps it will be a good plan to give a typical case of spinal irritation? There is not a fact in experimental science to support the view that pain is due to anæmia of the nervous centres. On the contrary, experiments show that pain is found to be present together with hyperæmia of the centres as proven by the experiments of Brown-Séquard on the spinal cord, and those made by the pupils of Ludwig in Leipzig, I refer to the hyperæsthesia following lesions of the spinal cord, and other experiments.

Dr. Hammond.—I am not going to attempt to give the description of a typical case of spinal irritation, for there is no such thing. One might as well attempt to give a typical case of constitutional syphilis. As to the symptoms of spinal irritation being due to organic lesions, I think that is absurd: for I have seen all the symptoms disappear with a single application of galvanism to return the next day perhaps, and again to be dissipated by like means.

Dr. Dupuy.—I do not see where the distinction lies. Duchenne de Boulogne has shown how to stop the most excruciating pains in the heads of patients suffering from tumor, sclerosis, or other disorders, by a mere application of electricity; also later, Dr. Charcot has shown the same; so that fact which Dr. Hammond brings forward does not show that pain is due to anæmia of the cord and not to organic changes in the membranes of the cord, or other organic changes as Dr. Gibney shows; because the pain can be removed by galvanism —for the same obtains with organic diseases.

Dr. Beard.—I did not suppose that there was any difference of opinion among neurologists in regard to the general nature of spinal irritation. It is spinal tenderness with various attendant symptoms in which every symptom of inflammation in organic disease is excluded.

Dr. Hammond.—One other point: I have known, at least, a dozen cases where patients have been cured of every symptom present—neuralgic pains, vomiting, spinal tenderness, muscular contractions—all disappearing on a single application of the actual cautery, and I have known all of these phenomena

to reappear upon the occurrence of emotional excitement, which I think is entirely inconsistent with the theory of any organic lesion.

Dr. Dupuy.—The same will happen when organic lesions are present as I have just said.

Dr. Jewell.—I recognize two classes of cases which may be named spinal irritation; one in which there is distinct spondylitis, if you please, and possibly inflammation of the dura and of the spinal cord. But there is another class of cases which I cannot allow to go into the one just named. I may illustrate what I mean by a case like the following. A gentlemen came to consult me, who was accustomed after certain meals, when unwell, to have his food apparently undergo a species of fermentation. Attempts at regurgitation of food were unsuccessful, on account of spasm of the œsophagus. In this case, coincidently with the gastric disorder, there was severe pain, and marked spinal tenderness about the fifth, sixth and seventh dorsal vertebra. Pain also was experienced in the shoulders, and down the arms. But as soon as his stomach was in a better condition, tenderness would be entirely gone. Now, in this case there was what I call spinal irritation,—there was intense hyperalgesia, not hyperæsthesia, for I think they ought to be distinguished from each other. I do not think, it more than just possible, that the case I have described was due to spondylitis. With regard to the explanation of this class of cases, the point I wish to make is this; that they do not depend on so serious a lesion as a local inflammation would be, but that they depend rather on a combined nutritive and circulatory lesion of the cord which had not only been produced by peripheral irritation of related nerves, but in turn may induce it elsewhere.

Dr. Dupuy.—I do not wish to be understood as saying that spinal irritation is spondylitis: All I want to say is, that it is not due to anæmia of the cord, but is the result of irritation inducing alteration in the nutrition of that centre by an augmentation of the blood supply. The fact that the pain can be relieved by elecricity and that it reappears, does not show that pain is not due to an organic disorder.

Dr. Hammond.—But there is much more in spinal irritation

than pain. And as to pain not being a consequence of anæmia, I would like to ask Dr. Dupuy, if he recognizes the existence of such an affection as anæmic headache.

Dr. Dupuy.—Yes sir, I do; but the cases we have considered to-night have no connection with that. Anæmic persons are prone to congestion of certain organs, and that induces the headache, for instance, costiveness, dysmenorrhœa, anorexia, *et cætera.*

Dr. Hammond.—I think they do have some analogy with anæmic headache, making allowance, of course, for the different anatomical situation of the disturbance. But as I said, I am not going to discuss the subject of spinal irritation. At most, our views of its pathology are but guesses, and my guess is that it is essentially anæmic. My reasons for this opinion have been given in full in other places.

Dr. Seguin.—Before speaking of what I think spinal irritation is, I must coincide with Dr. Jewell, that the cases related in the paper are not all such as I have been in the habit of calling spinal irritation. In regard to the pathology of spinal irritation, I have no theory to which I cling with any force. If there is one theory that I feel like accepting more than another, it is the one which I have for several years taught in this amphitheatre, namely, that spinal irritation is due to a malnutrition of the nervous centres, not directly to anæmia or hyperæmia. I am not prepared to accept the anæmic theory. I agree with Dr. Jewell, that many cases of spinal irritation are symptomatic. I know of many cases of spinal irritation, which were cured by proper treatment of the local trouble. The treatment of urethral irritation, I have seen relieve spinal irritation. Still I do not deny that malnutrition of the spinal cord, may give rise to symptoms located in the genitals, or in the uterus, or in any other organs.

Dr. Dupuy.—Malnutrition of the spinal cord is nothing but hyperæmia brought on by reflex action.

Dr. Gibney.—There is one case I did not report; it was a case of spinal irritation, typical, as far as I know. It occurred in a girl ten years of age. There was parotitis and much irritation over the region of the ovaries. I was called up several times to see her, they thinking that she was dying. She

had the typical eruption of typhoid fever on one or two occasions, but there was no elevation of temperature. She was in this condition for two or three months. One day in examining her, I passed my hand over towards the spinal lumbar region, and found that there was excessive tenderness. This was treated, and in two or three weeks she recovered, and has had no irritative symptoms since. At one time I thought she had typhoid fever, but the temperature saved me from making that diagnosis.

I had three or four cases of unmistakable spondylitis, but the other cases I am sure were not spondylitis.

Dr. Cross.—I would like to ask Dr. Jewell how he accounts for the malnutrition of the spinal cord, if there be neither anæmia or congestion. For the cord to be in that abnormal condition there must be, according to my idea, either anæmia or congestion, if we, perhaps, except cases of shock. Consequently, malnutrition must depend upon one of these two conditions. The symptoms are not those of congestion, and the influence of the strychnia treatment goes to prove that fact. If then there be no congestion and no anæmia, I would like to have him explain to me how there can be defective nutrition.

Dr. Jewell.—The malnutrition arises, either from a defective supply of nutritive materials, as in case of impoverished blood, or from too prolonged action or excitation— too little rest of the cord, or in both these ways combined. There is thus a more or less marked tissue waste produced, and irritation sets in, and without any circulatory disorder necessarily being present. Just as surely as you put these patients at rest they get better; for, if they are not kept quiet, tissue waste so exceeds tissue repair as to prevent recovery.

Dr. Hammond. I would like to ask Dr. Jewell, how he can explain the fact of the existence of irritation in the lumbar region of the cord, produced by depraved blood, at the same time that good blood is furnished to other parts? I cannot conceive of good blood going to one place and bad blood to another.

Dr. Jewell. That is not the point, doctor. It is, that the part in which a change has been excited, has more work to do

than it should do, or than other parts do, or relatively induces more excitation. The blood may be as good for that part as other parts of the body, but that part sustains relatively excessive tissue waste as compared with others.

DR. HAMMOND. I cannot conceive of local malnutrition, independent of local lesion.

DR. CROSS. That was the point I put. I merely asked the question for information. I do not see how your argument stands on any better basis, for may not anæmia produce malnutrition just as well?

DR. JEWELL. Most certainly; that is one way in which it is produced. I am not arguing against anæmia, but that I think there are cases in which it does not occur. The blood may not have anything to do with them. They may be solely due to too much activity, and too little rest. It is not malnutrition, therefore, so much as denutrition (excessive waste) which occurs in these cases.

DR. DUPUY. It has been proven by Brown-Séquard, twenty years ago, that there is no pain produced in the spinal cord if there is no increase in the vascularity of the gray matter.

DR. SEGUIN. I would like to ask Dr. Dupuy whether Dr. Brown-Séquard reached this conclusion by inferences from experiments, or whether he proved its truth directly by experiments?

DR. DUPUY. He has directly proven them, and so have the pupils of Ludwig. The experiments he has published. He has shown that when the spinal cord is laid bare, if it is protected from the atmosphere, there is no hyperæmia, and consequently, no hyperæsthæsia. The experiments of the pupils of Ludwig, made a few years ago, show conclusively that hyperæsthæsia is due to vaso-motor paralysis.

DR. SEGUIN. The experiments are very complicated, and there are many sources of error in them.

At the close of the discussion, Dr. E. C. SEGUIN read a paper entitled

A CLINICAL CONTRIBUTION TO THE STUDY OF POST-HEMIPLEGIC CHOREA.

He related the history of two cases. In the first, observed in 1873, a young man of eighteen suddenly became paralyzed

in the left arm, without disorder of sensation. Later, some numbness appeared in the left hand, and in about two months convulsive movements manifested themselves in the left upper extremity. About this time the left leg became affected with weakness and tremor; later still, the lower part of the left side of face twitched. The arm and leg were the seat of convulsive movements of choreiform type, made worse by emotion or attempt to use parts; no ataxia; sensibility slightly lessened in hand. Later, there occurred palsy of third cerebral nerve on the right side; palsy and spasm in left face; he died, but autopsy could not be obtained.

Case second was presented to the Association. Its history was briefly this: Male, aged twenty-six years; in April, 1876, sudden right hemiplegia, with partial loss of conciousness and temporary aphasia. During second and third months, convulsive movements appeared in the right arm and hand, while great recovery of muscular power was obtained. Partial right temporal hemiopia. Numbness and slight anæsthesia in face, arm, and leg on right side. A few epileptiform attacks since. Seven years ago had chancres, followed by doubtful secondary symptoms. Movements on right side are now nearly normal in force; while at perfect rest slight oscillations (of paralysis agitans type) occur in fingers and hand. During attempt to use hand violent inco-ordinate movements occur, irregular extension and flexion, of quasi choreic, quasi ataxiform type. Partial right hemiopia and hemianæsthesia.

In both these cases the movements, first described by Dr. S. Weir Mitchell are present; and from the other symptoms present it would appear as if the lesion in these cases had been in the basal part of the hemisphere, just below and posterior to the thalamus opticus, involving, to a certain extent, the internal capsule. In the first place, the lesion extended downward so as to involve the crus or the origin of the third nerve.

WEDNESDAY, JUNE 8TH.—AFTERNOON MEETING.

The Association was called to order at 2 P. M.

Present: Drs. Jewell, Miles, Shaw, Hammond, Dupuy,

Loring, McBride, Cross, Beard, Kinnicutt, Emerson and Seguin.

The Committee on Nominations presented the following report, through Dr. MILES, its chairman:

For President—Dr. J. S. Jewell, of Chicago.

For Vice-Presidents—Dr. F. T. Miles, of Baltimore, and Dr. S. G. Webber, of Boston.

For Corresponding Secretary—Dr. J. J. Mason, of New York.

For Recording Secretary and Treasurer—Dr. E. C. Seguin, of New York.

For Curator—Dr. T. A. McBride, of New York.

On motion, the report of the committee was accepted, and the gentlemen named duly elected officers of the Association for the ensuing year.

Under the head of Miscellaneous Business, the President read a letter from Dr. William A. Hammond, offering to the American Neurological Association a prize of two hundred and fifty dollars to be awarded at its next annual session on the favorable report of a committee of three of its members, to the author of the best essay on the anatomical and physiological effects of strychnia on the brain, spinal cord, and the nerves.

COMMITTEE ON PRIZE ESSAY.

Dr. S. Weir Mitchell, of Philadelphia; Dr. J. S. Jewell, of Chicago; and Dr. E. C. Seguin, of New York.

Dr. MILES exhibited a cast of the brain, showing an exceedingly rare anomaly in the arrangement of the cerebral convolutions. On the left side of the brain, the fissure of Rolando was bridged over by a complete secondary gyrus. This, Dr. Miles thought, had been seen but once or twice.

The reading of papers being next in order, DR. J. C. SHAW read a short paper entitled,

A CONTRIBUTION TO THE SYMPTOMATOLOGY OF BRAIN TUMOR.

Given in brief the two cases cited were as follows:

A lady, aged fifty; "choked disk," both eyes; dizziness, headache, attack of falling, without convulsions general or local; no paralysis. Post-mortem examination revealed a sarcomatous tumor in the interior of the orbit on the right side, pressing upon the inferior and middle frontal convolutions.

W. P., aged twenty-nine; male: " choked disk," attack of falling without loss of consciousness; no paralysis, choked disk lasting three and a half years without passing to atrophy; vision $\frac{20}{40}$. Post-mortem—Pedunculated cystic tumor pressing on the anterior surface of the left temporo-sphenoidal lobe. This man was one of the victims of the fire in the Brooklyn Theatre.

Dr. Seguin.   I never accept the statement of a patient in regard to loss of consciousness, when the patient falls down. That there was no paralysis in the second case is in accordance with my own experience.   I have observed a case of tumor larger than a hen's egg springing from the dura mater and from the squamous portion of the temporal bone, violently compressing the temporo-sphenoidal lobe.   There were only three epileptiform attacks, never any paralysis, no choked disk, and the patient died in mixed tonic and clonic convulsions.   I was lead to localize the lesion correctly in the right hemisphere by finding vaso-motor paralysis on the left side of the body as shown by a red patch on the buttock, and higher temperature.

Dr. Miles.   In a case which I had, after death the tumor was found to be of considerable size situated in the portion of the brain, regarded as not motor.   There was no loss of power, no paralysis, and, if it does not appear flippant, I will say how I found it out.   I had not seen the patient, but I asked the question, and it was answered in the affirmative, if he had used the chamber vessel?   So I thought there was no paralysis of the legs.

Dr. Dupuy.   It is well known that persons suffering from paralysis, due to softening of the brain, will sometimes get out of bed and pass water, on the bed of their neighbors—or do other mischiefs.

Dr. Miles.—I think, that does not have anything to do with my case.

Dr. Dupuy.   It shows that paralysis can follow or not follow, no matter what territory of the brain is diseased.

VASO-MOTOR CENTRES.

Dr. Eugene Dupuy made a communication to the Association on the above subject, which was to the effect that vaso-motor centres are not to be looked for in the spinal cord or

brain, but outside of them. The vaso-motor centres are made up of the ganglionic system and of exceedingly numerous ganglionic cells, that are to be found scattered in the pia mater in the neighborhood of the medulla oblongata and the pons; also in the pia mater of the anterior parietal portion of the brain; also from ganglia that are found on the track of the ganglia of the trigemini, and the ganglia of the spinal and cerebral nerves; also from the ganglia that are scattered through the cranial and other viscera.

That vaso-motor fibres only become connected with the cerebro-spinal system at their apparent origin, that is to say, at that point where the nerves get their sheath of pia mater after leaving their centres.

Dr. Jewell. This is a subject in which I have felt much interest. I have formed certain notions, partly as the result of my thinking, and partly as the result of experiments, and I am glad to see that the progress of research has fully confirmed them. There is one point in regard to which I am not able to share the views of Dr. Dupuy; that is, his denial of the existence of vaso-motor centres in the spinal cord. Unless it is by some such central mechanism, I do not see how the peripheral portion of the vaso-motor nervous system can be brought into connection with the central nervous system. I would like to inquire as to what becomes of the fibers which pass to and from the spinal cord, connecting it with the sympathetic? It has been said that they are for the control of the action of the arteries of the cord. But it seems to be forgotten, that the blood vessels which supply the spinal cord receive their vaso-motor supply, as do other blood vessels in different parts of the body. Vaso-motor nerves follow the blood vessels themselves, and do not take an independent course, as these *rami commnuiicantes* do, and there ore it has seemed to me that the fibers that come out from the spinal cord and pass to the sympathetic ganglia, arise out of a central vaso-motor mechanism in the spinal cord. If I have understood Dr. Dupuy correctly, he affirms that the vaso-motor system is disconnected from the spinal cord.

The spinal vaso-motor mechanism to which I have referred, may be summed up in the medulla oblongata, or in the neigh-

borhood of the so-called "convulsive centre," from which the whole vaso-motor nervous system may be excited. It seems to me that we have reason for thinking there is a connected line of vaso-motor centres the whole length of the cord, which receive impressions that pass into the spinal cord by the way of excitor nerves, the impressions being reflected again from the vaso-motor centres of the cord, outwards along vaso-motor nerves toward the periphery. Again, I am not prepared to admit that there are two classes of vaso-motor nerves, vaso-constrictor and vaso-dilator. I believe moreover that particular vascular areas of the body are related to definite regions of the spinal cord, where the corresponding spinal vaso-motor centres lie. I believe that the sympathetic in the neck, which when divided leads to changes in caliber of the extra-cranial vessels, does not contain the nerves which control the circulation in the brain, these latter nerves have their origin much higher up. They rise from within the skull itself. You may have the circulation of extra-cranial parts of the head change very greatly without the brain being affected, and *vice versa*. I do not see how Dr. Dupuy can avoid admitting vaso-motor centres in the cord and medulla.

Dr. Dupuy. The fact which you have stated that the vaso-motor centres culminate in the medulla oblongata, I think can be explained as well upon the theory of classical authors as the one I have tried to establish; moreover, as vaso-motor nerves will carry impressions both ways, and as it is also impossible to make a lesion of the medulla oblongata, without at the same time making a lesion of the membranes, which contain so many of the elements, it is easy to understand how you can have the results of Bernard without having made any disorder of the centres. The mere lesion of the membranes in that neighborhood will give rise to the production of sugar in the urine. These facts which I have brought forward to-day, I have seen many times.

Dr. Jewell. I would not be understood as making any objection to facts, for when facts are brought forward, that is an end to argument. If facts lead us in a certain direction then that is the way to go: if facts show that the phenomena can be better explained without vaso-motor spinal centres,

than with them, all right. It is difficult for me to see how a connection between these two nervous systems is to be made, if Dr. Dupuy's views are correct. I do not see how we can explain a case in which an irritative impression has been directed into the cord by the way of a sensory nerve, and following this is a change in some related vascular domain, apparently only through the spinal cord. I do not see how this can be explained unless there is some central mechanism through which it is accomplished.

Dr. Dupuy. There is a series of facts tending to show that lesion of the elements of which I have spoken, brings on vaso-motor paralysis, while there is not a single case on record of alteration of the cerebro-spinal nervous system, not implicating the membranes some way or other, with vaso-motor disturbances.

Dr. Seguin. I would like to ask Dr. Dupuy how he reconciles that statement with the high temperature in cerebral hemorrhage ?

Dr. Dupuy. The temperature only rises after a series of reflex actions have been set up by the hemorrhage.

Dr. Seguin. I would like an explanation as to whether pressure upon the vaso-motor centres produces the fall and afterwards the rise in temperature? At the first period we have a fall and then a rise. In the first stage it seems to me that the lesion is never complete.

Dr. Dupuy. I say that the blood generally finds its way to where those ganglionic cells are located; the blood gets into the ventricles and the œdema which follows and also the congestion of the choroidal membrane, which contains a large number of the vaso-motor nerves, all go to explain the facts of elevation in temperature. In all cases of hemorrhage no matter where located, there is always within 20 minutes to 24 hours after its occurrence, a production of albumen in the urine,—polyuria, and I have frequently seen sugar also. On the other hand the fourth ventricle is so protected anatomically, that there is less pressure on it than in other organs, when there is hemorrhage in the brain.

Dr. Loring. Is there any proof that with hemorrhage in the brain we have an increase of tension in the vessels of the

retina? It is the exception to find choked disk in these cases.

Dr. Seguin. I do not think there is any means to determine that. I think you will acknowledge that choked disk does not invariably express pressure.

Dr. Dupuy. I think there is an experiment which shows that tension in the arteries is augmented in hemorrhage of the brain.

Dr. Seguin. I always believed there was pressure during hemorrhage.

Dr. Loring. Some say that in apoplexy and other lesions of the brain substances, choked disk does not take place, and hence it has been argued that there was no pressure with hemorrhage.

Dr. Jewell. I do not see how the experiments referred to by Dr. Dupuy, show anything about intra-cranial pressure. I do not see how the experiment in which you take the carotid artery, which is outside of the cranial cavity, gives any just idea of intra-cranial, as distinguished from vascular pressure elsewhere. It may be shown in a vessel of the leg by this method as well, perhaps, as in the carotid artery. I do not see how increase of pressure in that vessel can teach us anything specially as to intra-cranial pressure.

Dr. Dupuy. When the pressure is increased in the carotid artery, it is in the brain also.

Dr. Jewell. I do not wish to prolong the discussion, but I am not able to see, thus far, any answer to Dr. Seguin's question, in regard to the greater elevation of temperature on one side of the body as a result of cerebral hemorrhage. In at least many such cases, there is no lesion at all, as far as is known, of the dura mater itself, or the cortex of the brain. If these parts are not involved, I do not see how, or through what mechanism the change in vascularity and temperature in the opposite leg and arm, can be explained. I want to know, if possible, what the mechanism is. If it is extra-spinal, I want to know how it can be limited to one side? I want to know what vaso-motor apparatus connects the cerebrospinal axis with the peripheral vessels of the opposite side?

Dr. Dupuy. I will try and make more clear the answer which I gave to Dr. Seguin's question. When there is a very

small hemorrhage in the brain tissues far from the membranes, there is no elevation of temperature in any part of the body. In order to give rise to an elevation of temperature, it must be a hemorrhage of larger extent, and the elevation is more marked as the hemorrhage is nearer to those membranes which I have said contain those centres. When it is in the cortical region, it is still more easy to explain, considering that this part is really vaso-motor, on account of the proximity of the pia. That is the way in which it takes place in a direct manner. When it takes place in a reflex manner, it is by acting upon those cells which are in connection with the spinal cord. I must state that all I have said is a mere outline of my studies, which are in course of prosecution.

Dr. Seguin. I fail to see, if the vaso-motor centres are broken up and distributed to the several parts of the body, how we can account for the rise in temperature which I have seen in many diseases; and how we can have vaso-motor paralysis following injury to the spinal cord. It is an eminent fact, that with hemisection of the spinal cord, we get on the same side vaso-motor paralysis, and on the opposite, anæsthæsia.

Dr. Dupuy. The vaso-motor paralysis in hemisection of the spinal cord, does not always involve the whole side, and also, it does not last so long as paralysis of motion. It is at present one of the most vexed questions, as the researches of Brown-Séquard, Schiff, Sanderson, Hutchinson, and others, bear witness. You must remember that as I say, the vaso-motor nerves must be injured when the spinal cord is cut across, since you cannot divide it without injury to the membranes which contain the vaso-motor nerves. You must observe that motor paralysis can be obtained without vaso-motor paralysis in the process of disease.

Dr. Seguin. If these are distributed, as you say, why do they not continue to act after hemisection of the cord?

Dr. Dupuy. Why do not the ganglia of the brain, after a mere pricking of one of them, act?

Dr. Jewell. I did not know that by simply scratching the pia mater you would have the same phenomena as when you make a hemisection of the cord.

Dr. Dupuy. Yes, and moreover, you will have anæsthæsia following.

Dr. Jewell. Yes, I am prepared to believe that; but not that the same phenomena follow as in hemisection.

Dr. E. C. Seguin read a report of seven cases in which more or less limited lesions of the cerebral cortex had been connected in life with definite symptoms.

The cases were grouped in three categories.

1st. Cases in which a limited lesion produced aphasia with or without hemiplegia. In Case I., *ramollssement* of the convolutions, in front of and just behind the fissure of Rolando on the left side, produced right hemiplegia and complete aphasia. Much recovery of motion occurred; aphasia remained complete. Careful examination of brain showed that the posterior part of the third frontal convolution and the folds of the island of Reil were involved in the lesion. In Case II., embolism of the first part of the left middle cerebral artery produced right hemiplegia with complete aphasia. There was *ramollssement* of the superficial part the left third frontal convolutions and of the first two folds of the island of Reil and their adjacent white matter. In Case III., chronic aphasia of varying degree, epileptic attacks, and partial temporary right hemiplegia were found to have been caused by a chronic pachymeningitis adherent to the altered left third frontal convolution near the fissure of Sylvius. Death in status epilepticus, with complete right hemiplegia, caused by extensive central softening of left hemisphere; very recent compared with meningitis. In Case IV., a secondary (by infection) tubercular meningitis, with its focus about the left middle cerebral artery, and over the third frontal convolution and the folds of the island of Reil, produced intermittent aphasia (occurring almost every forty-eight hours), and at the close of life, in twelve days, there were complete aphasia and right hemiplegia.

2d. Cases in which a limited lesion produced paralysis. Case V.: Small patch of softening in the middle of the left ascending frontal convolution, reaching inward to roof of ventricles, produced a right hemiplegia without aphasia, and probably without facial palsy. When examined, had palsy of right arm, none of face or tongue, and was paraplegic (from spinal lesion found in cervical region).

3d. Cases in which a limited lesion gave rise to limited convulsions. Case VI.: A young man, injured on top of head, developed common epilepsy, replaced by local epileptiform spasms in left face, neck, hand, and arm; paresis of these parts. Later, left hemiplegia and return of general spasms. Autopsy showed injured skull, thickened bone, adherent thickened bony dura over upper (inner) end of left ascending and first frontal convolutions, a sarcomatous tumor, starting from adherent dura and penetrating into nearly whole of upper half of right hemisphere. Case VII.: Septicæmia, suppurative meningitis, and two abscesses in cortex of brain. One as large as an almond in lower part of left second frontal convolution, not involving more than anterior margin of the third, gave rise to no symptoms. The second, about the size of a pea, situated in the white matter just under the cortex of the middle part of the right second parietal convolution, was in all probability the cause of very singular epileptiform (no loss of consciousness) spasm, which occurred early in the disease, in the left hand, arm, and face.

These cases seem to favor recent views on the localization of function in the cerebral cortex. The first group emphatically supports Broca's hypothesis of a speech centre in the left third frontal convolution, and the anterior folds of the island of Reil. The cases of the third category also give support to Hitzig's and Ferrier's views. The author is not prepared to accept so fine a localization as proposed by these gentlemen, but it seems to him that we are in a position to speak of excitable and non-excitable regions in the cortex (pathologically speaking.)

Dr. Cross. I think the thanks of the Association are due to Dr. Seguin for his valuable paper. I know of no way in which we can advance our science to such an extent as by an accumulation of clinical cases, accompanied with post-mortem examinations as carefully recorded and as accurately localized as have been made in this paper, together with such thorough microscopic investigations. I do not propose to discuss the subject of localization of the functions of the brain. We are not yet prepared with sufficient clinical facts to either affirm or deny that these different motor centres exist in the human subject,

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as have been demonstrated by Fritsch and Hitzig to exist in the lower animals. I do not believe we can say that they are right until we have further confirmed their experiments.

Dr. Dupuy. I am greatly interested in the paper of Dr. Seguin. There is something which strikes me as interesting in this paper; it is that he has a number of cases which show a relation between lesions and symptoms, and others which do not. That is to say, some which support Fritsch and Hitzig's theories, and others which do not. There is no greater error in science, since the doctrines of Longet concerning the spinal cord, than the teachings of Fritsch, Hitzig, and Ferrier. The error in both cases was of the same nature, and arrived at by the same method. Longet pretended that as the application of electricity to the posterior columns of the cord caused pain, therefore those columns were tracts for sensory impressions, but nobody believes that now. In like manner the localizers having used electricity, and seeing movements following irritation of the cortex, with that agent, concluded that the parts irritated were motor centres. Prof. Charcot has himself published a case, which is to be found in Trousseau's clinical lectures, which goes entirely against the aphasia theory. Again, Prof. Charcot has himself stated that persons suffering from lesions of the internal capsule were permanently disabled because that centre was destroyed. Now he has since published a case of lesion of the internal capsule, of 15 years standing, I believe, where not only all of the phenomena were observed, but also hemichorea; and yet all those symptoms disappeared entirely for several days after the application of metallic bodies, in the manner of Dr. Burq. Now how could that be, if a centre had been destroyed, for the internal capsule was destroyed before, during and after the application of the metals? And yet we have the changes in the phenomena observed.

Dr. Seguin. With reference to Charcot's first case, was the question discussed in regard to the left and right handedness?

Dr. Dupuy. Yes, sir, Trousseau has discussed those subjects.

Dr. Seguin. I think there is great doubt in regard to the old cases. With reference to Dr. Spitzka's own case just announced, I think it is one of possible great value, and should

be examined with great care. I had a case last year in a boy with supposed syphilis of the brain and right hemiplegia. We found a tumor of the right hemisphere near the apex of the sphenoidal lobe. We made some cuts through the healthy hemisphere, and we were satisfied that there was no lesion in it. I put the brain in bi-chromate of potash, and it was my intention to present it to the Pathological Society and have it examined by a committee, without my being present, and thus have an unbiased opinion, but it decomposed. I should be unwilling to publish a negative case of such importance without the support of a committee. I attach an inferior value to negative evidence.

Dr. Jewell. I do not intend to prolong the discussion. Notwithstanding you have a few cases in which the lesion is on the same side as the paralysis, yet there can be no question but that in the majority of cases it is the other way. I can never give up the general idea of localization of functions in the brain, until I have better reasons for so doing than I have yet known. But as to what part of the brain exists for particular functions, I am sure we cannot with certainty say.

On motion, the Association adjourned, to resume the discussion in the evening.

The Association was called to order at 8 P. M. by the President.

Present: Drs. Jewell, Miles, Emerson, Dupuy, Spitzka, Hammond, Cross, Beard, Seguin.

On motion, the reading of the minutss of the preceding meeting was dispensed with.

On motion of Dr. G. M. Beard, it was resolved to hold the next annual session of the Association in the city of New York.

On motion of Dr. T. M. B. Cross, of New York, the Recording Secretary was directed to preserve fifty copies of the first volume of the Association's Transactions.

On motion of Dr. E. C. Spitzka, a vote of thanks was tendered to Mr. Josiah Roberts, of the New York Medical Record, for his attendance and note-taking during the entire session, thus securing a full report of the proceedings of the Association in a widely circulating medical journal.

Before the discussion of Dr. E. C. Seguin's paper was resumed, DR. EUGENE DUPUY made a few remarks upon

HEREDITARY TRANSMISSION OF PECULIARITIES

It was a report of a curious case of heredity. Dr. Dupuy stated that he owed to his friend, Dr. Gibney, the opportunity of observing a family consisting of father and mother, five children, and one grandchild. The father and mother are semi-ambidextrous. All of the children and the grandchild are ambidextrous to an annoying degree; all of the movements which they perform with one hand are simultaneously performed by the other hand. The girls are obliged to use only one hand when dressing themselves, or when cutting patterns, and hold the other hand down by their side, because the two hands perform the same movements at the same time, and would interfere with each other.

Attention was called to the fact that the father of the grandchild is not semi-ambidextrous. Dr. Dupuy has made experiments upon these persons, and has found that, if the skin of the forearm on one side be kept well dry, and a rapidly interrupted electrical current be used, so as only to call forth reflex actions, it is possible to induce synchronous movements in the fingers of both hands, and also muscular contraction in the lumbricales muscles of the fingers, which are too rapid to be carried on by the will. Dr. Dupuy considered these facts of great interest when coupled with the facts which he reported yesterday about hereditary epilepsy.

DR. JEWELL. In this connection, I will refer to a case belonging to a similar class to the one referred to. A medical student of mine relates the case of a boy who fell from a height upon the back. Paralysis of the legs followed, but in one or two years movement again became possible. The resumption of voluntary movement was like this, when he willed to move his right leg his left leg would move, and *vice versa*. Whether it may have been a delusion of the patient as to his willing to move one leg rather than the other, I cannot say.

DR. DUPUY. There are many such cases upon record. I know of four cases which are identical with Dr. Jewell's, and are authentic. The two last that I remember, were published by Dr. Hayem and Dr. Onimus, of Paris. In these cases, the

phenomena depended upon lesions localized in the spinal cord, whilst the cases I have reported to-day seem to have a cerebral origin, as voluntary movements as well as involuntary movements are bi-lateral.

Dr. Spitzka called for Dr. Seguin's views in regard to the subject of his last paper—Localization of Brain Lesions—which was given, and an animated discussion followed.

Dr. Seguin. Perhaps I should have stated my opinion upon localization in my paper, but as I did not, I am now very glad of the opportunity of placing myself on record this evening, and reproduce what I have taught here in this amphitheatre for several years. I think there can be no doubt that irritations of different parts of the brain do excite motor manifestations. The application which I make of this proposition to my practice and thought, is to use the facts as a guide in study. I do not accept the hypothesis of Ferrier, although I consider it a very important hypothesis. Before the students at my clinic, I have attempted to localize lesions in the brain, and have felt no hesitancy in localizing the lesion near or in the island of Reil in cases of clear aphasia. With reference to my communication this afternoon, I am free to say that some of the cases there given show that certain parts of the brain are very excitable, and that certain other parts are not very excitable. If I were asked to give my opinion in regard to localization of excitable and unexcitable regions, I would say that the anterior part of the frontal lobes and the posterior lobes are not excitable, but that the median parts of the cortex are.

Dr. Spitzka. It is one question whether there is such a thing as localization, and quite another thing whether the experiments of Hitzig are of any value. In regard to the second statement, I do not think that Dr. Seguin was right in saying, that all writers agree as to the experiments of Ferrier. Fuerstner, for instance, differs in his results. In regard to the paper this afternoon, I had an autopsy this day of a patient in whom the convulsive movements were very marked in the right facial muscles and arm, but there were none in the leg. This would have been a case for special localization. I found meningitis of the right cerebral hemisphere and the superior sur-

face of the cerebellum, which extended symmetrically for a distance of three centimetres. The lesion was one purely of the right side, as regards the hemispheres, and the lesion was on the same side as the convulsive movements.

DR. DUPUY. The case of Dr. Spitzka is right against the localization theory: the lesions were on the right side in the cortex, and in the so-called motor regions; and the symptoms on the right side in the limbs. If there were unseen lesions in the left hemisphere to explain the symptoms on the right side of the body, why were there no symptoms on the left side of the body to explain the lesions in the right hemisphere, since the cortical lesions on the right side occupy those areas of the gray matter which are called psycho-motor? If there are lesions in the left hemisphere to which the symptoms on the right side are to be referred, then it must be admitted that the cortical region on the right side does not contain centers, as there are no symptoms in the left side to be ascribed to them; which is contrary to the doctrine of localization. I do not agree with Dr. Seguin when he says that all physiologists have accepted that there are excitable and non-excitable regions of the cortex of the brain. I have shown long ago that it is not so, and so have Goltz, Schiff, Brown-Séquard, and many others. Moreover, Ferrier himself has shown that irritation of the *gyrus angularis*, which is, according to him, a sensory center, gives rise to motion, but he pretends it is true that that motion is the awakening of organized passed experience; the motor manifestation of a sensory impression, of course this is untenable. Schiff has found what I have myself discovered, that during complete anæsthesia the irritation of the cortex gives rise no more to contraction of muscles which already reflex action cannot be obtained by excitation of the sciatic nerve, although direct action on the gastrocnemii muscles is obtained; therefore, when the irritation of the cortex is not responded to by any movements, it is not because it is anæsthetized, for if the irritation be applied to the tissues underlying the cortex, which is removed, we have movements at once. It should be remembered that the cortex of the brain is said to be excitable, and yet unlike other excitable nervous matter, it can only be excited with electricity; I have shown

abundantly that the action of electricity cannot be localized
to any particular spot. If, as Ferrier puts it so forcibly, the
irritation by electricity of one point of nervous matter will
always give rise to one and the same kind of phenomena,
whilst irritation of another point very near will not have a
similar effect, I do not see that it proves anything further than
this, that electricity being a physical agent, acts in accordance
to certain laws in a constant manner every time that identi-
cal circumstances are present. Now the convolutions are sep-
arated by sulci, which are channels of conduction to those
parts of the brain which are known to be excitable, according,
therefore, as electricity is carried or not to them, we shall
have or shall not have movements.

But Ferrier says that there is a case taken from human pa-
thology which confirms the doctrine; it is that experiment of our
associate Dr. Roberts Bartholow. Now, any one who will take
the care to read the account of this experiment, will see for him-
self that it does not prove anything since the author inserted the
needles, insulated up to their point, two inches deep into both
hemispheres in their posterior region, so that the cortex which
was destroyed in part by the cancerous growth, of which the
patient was suffering, could have in no wise been irritated in
that experiment. All the experiments that I have made up
to this time show that the gray matter in animals is not
excitable.

DR. JEWELL. I cannot say that I have performed experi-
ments, but notwithstanding this, I feel that I am in a position
to interpret the work of others. I have not supposed, for a long
time past, that the gray matter of the cortex, or of the spinal
cord, is excitable by any known artificial means. The gray
matter, the cells, if ever acted upon, as they must be, are never
excitable except by the channel of their fibres, therefore the
conclusions drawn from one whole class of experiments which
have been made, must be rejected. The fibres which lead
downward from the gray matter are the excitable parts, and
not the gray matter itself

The cortex forms no part of the motor apparatus proper.
It is only an excitation mechanism, from which impres-
sions pass downward to the true motor apparatus which

culminates in the basal system.   I must yet hold that different parts of the cortex of the brain have different functions, at least in the present unsettled state of experimental physiology.

Dr. Dupuy.   All I wish to say is, not that the cortex has no functions, but that what the localizers state is unsound.   I believe we ought to stand by Flouren's doctrine.

Dr. Spitzka.   I have endeavored to show that no number of experiments will satisfy us that lesions on one side will produce effects on the other side of the body.   If there is anything that is exact, it is anatomical research, and this shows that there are undoubtedly variations which may explain why we have sometimes lesions on the opposite side, and sometimes on the same side with the convulsions or paralysis, and these variations are to be sought for in the greater or lesser completeness of the pyramidal decussation.   But I cannot admit that each of the complex functions of the mind, or even all of the simpler ones, are situated in one, and only one cortical area, as the former are but the compounds of simpler functions, their anatomical seat must correspond to these simpler functions taken in the aggregate, or rather, it has its seat in several perhaps distant cortical territories, as well as in the associatory tract of fibres which connects these.   In regard to Dr. Jewell's statement, I wish to be distinctly understood as saying, that each group of nerve cells in the cortex must have its function.

Dr. Hamilton.   It has struck me that more attention should be paid to the study of localized meningitis.   In meningo-encephalitis, the motorial results of cortical irritation are quite distinctive.   That the physiological experiments of Hitzig and Ferrier have had ample confirmation by cases collected by Charcot and others, there can be no doubt, and I have recently seen a case of epilepsy in which local spasms of one hand preceded the general convulsions.   After death, an exostosis was found pressing upon the centre, which is supposed to preside over the movements of the hand and arm.   Landouzy has collected over one hundred suggestive cases, to which we must admit, those of Dr. Seguin form a valuable addition.

Dr. Dupuy.   I have spoken so much lately about the part played by the membrane of the brain in the production of

movements, that I will say no more. Now, as for the spicules
of bone pressing upon the motor centres and producing
paralysis, there are a great number of such cases. That fact
does not show much. Dr. Proust relates the case of a man
who received a blow with a bayonet, and there were spicules of
bone which, according to measurement, were pressing in the
region of the angular gyrus. The patient had paralysis of the
face and arm on the same side, and aphasia. No sooner were
the spicules of bone removed, than at once he recovered; after
the dressing of the wound had been completed, it was found
that the dura mater had not even been torn by the bone. It
is useless to state that the lesion was far from the speech cen-
tre. The ideas of Landouzy are the same as those of Rendu,
Wilks, and before those, Bouchut, and Cazauvielh, and many
others who have written a great deal on the subject of arach-
nitis.

MENTAL THERAPEUTICS.

This was the title of a paper read by Dr. Geo. M. Beard,
and was a continuation of a paper which he read last year be-
fore the Association, on "The Influence of Mind in the Cau-
sation and Cure of Disease."

Dr. Beard cited two cases of organic disease of the spine,
which had received decided temporary relief while sitting un-
der blue glass.

The opinion was expressed, that in time all works on thera-
peutics must include a chapter on mental therapeutics.

From experiments and study, Dr. Beard was able to make
the following psychological suggestions:

1. The ill-success of patients treating themselves, and of
physicians treating their own families, was partly due to the
want of awe and the emotion of wonder to co-operate with
them.

2. The old custom of keeping patients ignorant of the con-
tents of prescriptions by writing them in Latin, had psychol-
ogy on its side.

3. It is entirely possible that hydrophobia and tetanus may
be brought on by mental causes, with all their distinctive
symptoms, and that death may result through the emotions of
fear and expectation alone.

4. Patients whose will and intellect are feeble have a bad prognosis, for with them the objective symptoms are trifling; and *vice versa.*

5. Physicians of great scientific attainment and real worth, may fail when an ignorant and obscure charlatan succeeds, because in the latter wonder and awe are excited, and these are more powerful therapeutically than simple respect.

6. In experimenting in hospitals with new medicines, patients must be deceived, or else the results are complicated by mental influence.

Dr. Beard expressed a belief that those who would repeat his experiments would confirm his results and conclusions.

DR. SEGUIN. I did not have an opportunity of expressing my views upon the subject of Dr. Beard's paper last year, had I been present, I would have sided with him in saying that the emotions of patients may be made the means of curing them of a disease. I deny, with Dr. Beard, that there is any trickery in the use of the emotions to this end. We do many things in the practice of our profession (such as exposure of the person, mutilations, etc.,) which, considered *in themselves*, apart from the *end in view*, might be termed indecent and barbarous; yet we think it is right to use these means. Again, in our prescriptions we give certain assurances, the fulfillment of which we are not certain of. In prescribing hypnotics, a very uncertain class of remedies, we tell the patient, " That will make you sleep," and the hypnotic effect is increased; the drug has a better chance to act. In many cases, the physician's conscience must be the higher law.

This paper was further discussed by Drs. Dupuy, Miles, Seguin, Beard, Jewell, Hammond, and Cross, pending which, the society adjourned.

E. C. SEGUIN, M. D., *Recording Secretary.*

Reviews and Bibliographical Notices.

I.—KUSSMAUL: THE DISORDERS OF SPEECH.

HANDBUCH DER SPECIELLEN PATHOLOGIE UND THERAPIE. Herausgegeben von Dr. H. Von Ziemssen. ZWOELFTER BAND. ANHANG. DIE STOERUNGEN DER SPRACHE. Versuch einer Pathologie der Sprache von Dr. Adolf Kussmaul, Professor in Strassburg. Leipzig, 1877. (*The Disorders of Speech, etc.*) 300 pages.

This work appears to us to be one that it is somewhat difficult to adequately notice in a JOURNAL like ours. Its subject is the disorders of a function which from its importance has intruded an irregularity into the otherwise usually pathological classification of nervous diseases. This special study, moreover, from the intimate relations it has demonstrated to exist between the most special and important mental manifestations and processes, and our physical system, is one of surpassing interest, not only to the physician but also to the philosophical psychologist. It is not surprising, therefore, that in such an extended monograph, such as the one before us, the author should deviate somewhat from the ordinary course in medical text-books, and attempt to enter upon and discuss the higher and more abstract questions suggested by the theme. Thus in the present volume we find a very considerable space devoted to the physiological psychology of speech, its origin and earlier development, all preceding the real subject of the work as a medical treatise—the disorders of this function. This materially adds to the completeness of the monograph, but it appears to us to have some disadvantages. Thus it brings in some speculative questions in relation to thought and speech, and their mutual interdependence. Probably as much can safely be said of some of the philological bearings of the ideas here advanced, but we cannot discuss either of these points adequately in the present notice.

The first fifteen or sixteen chapters are mainly given to the discussion of the nature of speech, its origin, psychology, etc.,

and, therefore, are included under the above-mentioned head as less medical than linguistic in the character of their contents. There is much of interest, however, in the author's method of treating the subject, that it will not do to pass over unnoticed. While he studies the function of speech in these chapters mainly in its psychological relations, its origin, connection with ideas, etc., he still treats the subject more or less from a physiological point of view, and moreover, touches to some extent upon its pathological conditions. Thus in the eighth chapter is given a general statement of the varieties of the general disorders of the means of the expression of our thoughts, followed in the ninth by one of those of diction and articulation in the more restricted sense. While we observe that he uses the term aphasia in its general sense throughout the volume, probably because of its customary usage, he endorses the objection of Finkelnburg, that it is not a sufficiently comprehensive term to properly include all the disorders of expression to which it is applied, but instead of the word *asymbolia*, proposed by that author, Prof. Kussmaul suggests that *asemia* of Steinthal be used instead, as of still less restricted signification. Thus we may have, he says, asemia verbalis, graphica and mimica, and as varieties, asemia paraphrasica, paragraphica and paramimica, and may speak of an asemia expressiva or perceptiva according as the ability to make or understand the signs is meant. Then he classifies the disorders of speech proper, or lalopathies, into those of articulation and diction as follows, those of the former as dysarthrias, subdivided into those of the external organs of speech, (dyslalia) and the essential or central dysarthrias, according as the disturbance is merely functional or is connected with organic lesions of the nerve centres. Diction is a mixed sensory-intellectual act, in which the words are not only connected with ideas, but are grammatically combined, and to its disturbances he gives the name dysphasias. This is not a complete classification, nor is it the one followed throughout in the book, but it is here reproduced as one that is rather comprehensive and philosophical.

After noticing the relation of speech to memory and the forms of amnesic aphasia in a brief and general way, and giving some space to the development of language in infancy, the author comes in his fifteenth chapter to the discussion of the necessary relations of the various special senses to speech. In this, the most striking example is, of course, the well-known case of Laura Bridgeman, narrated by her instructor, Dr. Howe, one of the most suggestive cases ever published. The final conclusion which this case teaches, and which the author admits, is that the tact and muscular senses are the only ones essential for the formation of a comprehensible language, and the development of the intelligence, and therefore the only absolutely necessary reflex sources of expression, a fact that has been frequently ignored or overlooked by writers on mental philosophy. In

normal individuals, however, there is no question but that the
senses of hearing or sight are essential to perfect speech. The
imitative articulation of infants first learning to use their vocal
organs, unlike the echo-speech in certain forms of disease, and
the verbal articulation of parrots, is not classed by Prof. Kuss-
maul as a simple auditory reflex, the sound exciting the proper
centre at the base of the brain, and reflexion taking form of the
reproduction of the sound. He discusses this question physio-
logically, reviewing the evidence pro and con as to the possi-
bility of the act being performed without the intermediation of
the higher organs of the brain, and concludes as follows: "Only
one clinical proof can be given for the, to us altogether im-
probable hypothesis, that the imitative reflex speech is performed
directly and solely between the auditory nerves and the motor
nerves of articulation in the basal centre. After a complete de-
struction of the voluntary motor routes in the hemispheres, in-
cluding the internal capsules, words must still be spoken. Until
this proof is established, we shall still believe that imitation is
always a function of the cerebrum." That the author is correct
in this decision, we presume few will deny; it is, indeed, to us
somewhat remarkable that speech should be classed in any usual
sense among the reflex acts. This cannot be the case by any
means in infants, unless we place all their early indications of
intelligence among the reflexes. In the case of the interjections,
oaths, etc., which are sometimes readily uttered by aphasics, who
have completely lost the faculty of speech, it may be said that
they belong in a certain sense to the reflexes of education or
habit, but as such it is hard to prove that their reflex centre is
situated anywhere below the cerebrum. The theory of Hughlings
Jackson, that this automatic articulation is a function of the
right hemisphere is very properly rejected by our author, not so
much on the grounds, however, of its needlessness, as we would
prefer, but because of the evidence we have that emotional
manifestations proceed from either hemisphere. That of Jac-
coud, that a basal centre was alone enough to account for them,
when we consider the complex phrases and even whole sentences
that are sometimes thus ejaculated appears also improbable. It
seems to us, indeed, to be only different in degree from ordinary
speech, and in the fact that the special and stronger stimulus in
this case is an emotional and not so much an intellectual one.
In ordinary talking, the ideas are all we are conscious of, the
words usually come of themselves, and their formation, which
was once a task of conscious intellection, takes place unconsci-
ously and, as we might even say, reflexly. But one would
scarcely be tempted to place the centre for this reflex excited by
ideas in the medulla, or base of the brain, nor does it seem right,
unless we are, like Carpenter, Vulpian, and some others, inclined
to make the medulla the seat of the emotions, to refer these
ejaculatory articulations to that portion of the nerve centres.
So, also, in respect to the musical expression of which some

aphasics are capable, in some cases the rhythm even carrying
with it the words of a piece of poetry. In the same way some
patients have been able to repeat words in connection with
others and in short sentences, that they were unable to repeat
alone, this being only a pathological example of what occurs
normally in almost every one's experience, but which is inter-
esting in this connection as indicating, as it appears to us, a
higher situation of this, as it were reflex, centre for speech
than the medulla, where its purely mechanical centre must be
located. This hypothesis of a superior automatic phono-motor
centre above the mechanical centres of speech, has been some
what developed by Onimus, in an interesting paper published
in the *Journal de l' Anatomie et de Physiologie*, 1873, and a
translation of which appeared in this JOURNAL for April, 1874.
This paper seems not to have been known to Prof. Kussmaul,
as he makes no mention of it, nor does he follow out to any extent
its line of thought.

That the centre for articulation, or the purely mechanical
part of speech is situated in the basal portions of the brain is
proven sufficiently, not only by physiological, but also by path-
ological evidence. That it is in the medulla is supported by the
facts that in that region are the nuclei of the various nerves
supplying the parts employed in articulation. But the view
that the cerebellum has any part in these disorders, as held by
Jaccoud and Luys, our author considers rests upon no secure
foundation, and he briefly reviews the facts bearing on this
question in support of his position. As to the part played by
the higher ganglia, there is too little positively known of the
course of the nerve tracts to exactly state the mechanism. Some
points, however, may be regarded as established. Thus, it is a
common observation, that the dysarthric complications are more
pronounced and enduring in cases of right hemiplegia from
lesions in those parts than in those where the paralysis is on
the left side of the body. The corpora quadrigemina may be
said to have no special connection with articulate speech,
though their importance from their optic connections, to other
methods of expression is, Prof. Kussmaul thinks, sufficiently
obvious. As to the thalamus, the evidence is not so strong in
any direction, but from some observations it appears probable
that disturbances of articulation may be caused, perhaps, indi-
rectly by lesions of this ganglion. The relations of the corpus
striatum to articulate speech are more evident, but still as our
author admits, clinical proofs are not yet sufficient to enable us
to form perfectly satisfactory conclusions, as to the relations of
the corpus striatum to the basal centre for articulation. "Only
this much is certain, that lesions of the corpus striatum render
the articulation stammering to the point of unintelligibility, or
may altogether suppress it, and this disturbance of function is
the more sure and considerable, the more extensive the lesion,
especially on the left side. But the distinction of the effects

from lesion of the gray or white substance, and the respective
parts they take in the function, remains a difficult question for
future decision." The theory of Broadbent, that the formation
of words and word groups takes place in the corpus striatum, is
hardly much favored by Prof. Kussmaul, and, indeed, it appears
not probable when we consider the complexity of this operation
in most cases. Still, as he states, it may have a part in the
formation of syllables, and the vocalization of consonants, and,
in short, in whatever may be the basal mechanism of speech.
It is, he holds, the uppermost brain tract injuries of which
can lead to simple dysarthric alterations of speech, the dys-
phasic symptoms appear when injuries are received in the still
higher regions of the brain.

In the twentieth chapter, there is a rather lengthy psycholo-
gical digression, as it appears to us, which we will notice, since
the author's views are perhaps not those most generally accepted
either by metaphysicians or physiologists. They are not new,
however, but have been advanced, perhaps, in a somewhat differ-
ent form, previously. The question, where are the forms of
speech elaborated in the brain, follows naturally that as to what
are the sensory routes by which the external excitations reach
our mental laboratory, and react in the form of spoken or writ-
ten language. That is, where are the spoken or written signs
for ideas elaborated, and this is hard to consider separate from the
the other question, where are these forms understood as signs
of ideas? The first question is easily disposed of, we really
know nothing of the higher course of the fibres from the optic
and acoustic nerves. But we know from pathological evidence
that these routes may be intact, and yet the power of appreci-
ating words as signs or symbols may be lost, thus indicating
that the cerebral regions for the perception of sounds or signs
are not the same as those for the ideas they represent. This
naturally brings in the question as to the seat of the psychic
functions and of consciousness which our author discusses at
some length. His psychology is much the same as that of
Wundt, and he adopts what is possibly a necessary outcome of
those views, the opinion that the psychic (seelische) functions
are not limited to the cerebrum, but that other portions of the
nervous system are the organs of the mind. This view he sup-
ports not only on the physiological grounds adduced by Pflueger
and Hammond, but by the theory of the unconscious conscious-
ness of which Wundt makes so much in his physiological
psychology, though he does not directly refer to that author in
this connection. We have not the space here to give the rea-
sons why we disagree with our author in this regard, the sub-
ject is too extensive, and, as we have said, his remarks here ap-
pear to us like something of a digression from the proper
theme of his memoir. We must leave its further discussion till
we take up in some future number of this JOURNAL, the physi-
ologico-psychological theories of a modern German school.

We will pass over the chapters upon cerebral localizations and the remarks on the history of the localization of the function of speech in a special region of the brain, the literature of both of which subjects must be familiar to our readers. Prof. Kussmaul concludes that while the third frontal convolution is not the only cortical region concerned in speech, still it has a rather more important connection with this function than other parts. That the left hemisphere is most generally the one that has to do with this faculty he admits, and accounts for the fact by the general right-handedness of individuals indicating the greater functional ability for most purposes of the left hemisphere. The only suggestion he offers to account for this is taken from Ogle who thought that the greater blood supply of the left hemisphere might be the cause of this difference, and who it is known, made some investigations on this point. He does not quote, nor seem to be aware of the very elaborate memoir of De Fleury upon this subject. The final conclusion he appears to reach from the study of the reported cases, is that the third frontal convolution appears to be concerned in the combination of speech, that in ataxic aphasia the anterior portion of the hemisphere, either alone, or together with the posterior region, is almost invariably affected, while in purely amnesic aphasia, sometimes one sometimes the other of these tracts appears to be the seat of the lesion. Beyond this we are not in any position to form even approximately correct judgments of the localization of speech in the cortex.

We have already given the general classification proposed by our author in his ninth chapter. In the twenty-fourth he lays down his clinical classification of aphasic disorders, in the usual and more restricted sense of the word, comprising the dysphasias of the former plan. These he divides into two great classes, the dysphasias and the dysphrasias or dyslogias, the latter including the disorders of speech due secondarily to disease, affecting the intellectual functions. The first of these he divides in a general way into the following:

1. Ataxic aphasia or inability in the motor co-ordination of words. This really is only a cortical mechanical trouble, a verbal anarthria, and as Steinthal correctly says, a true dysphrasia, though here included.

2. Amnesic aphasia or inability to remember words as acoustic sound-complexes.

3. Verbal deafness (Worttaubheit) or inability to understand words as before, notwithstanding the retention of normal hearing and a fair degree of intelligence.

4. Paraphasia or the inability to rightly connect words with ideas, so that instead of the proper term one of the opposite signification may sometimes be employed.

5. Agrammatismus and akataphasia, or inability to grammatically form words and arrange them in sentences.

These, taken in a sense so as to include the corresponding

disorders in writing, signs, etc., as well as in speaking, comprise all the clinical forms of aphasia, in the commonly employed signification of the word, but not, of course, all cortical lesions of speech. Prof. Kussmaul mentions a number of these which he treats more fully in later chapters of the book. Such are retardation of speech, and its acceleration, sometimes to a very remarkable extent rendering it even unintelligible, peculiar modulations in speech, aphasic stuttering, and the inability to manage long and many syllabled words as a whole and at once.

Another point to be borne in mind, as a matter of course, is that these varieties of aphasic disorder are not always well marked and distinct. Thus amnesic and ataxic aphasia include each other to a certain extent, and paraphasia is, as Bastian says, an ataxic disorder of diction.

We can pass without lengthy notice the parts in which simple ataxic and amnesic aphasia are discussed, since these must be more or less familiar to the readers already, and we do not find much to call for special mention in the author's very excellent treatment of his subject. The curious conditions, however, which he distinguishes under the names of "Worttaubheit" and "Wortblindheit" are worthy of particular notice. In these the centripetal routes of communication between the ideational centres and the external world, are, or appear to be, interrupted, while the centrifugal routes of expression may seem to be comparatively only slightly embarrassed. The patient may retain a fair degree of intelligence, and be able to express himself more or less perfectly in language, have perfect sense of hearing, and be able, as in cases here reported, to distinguish the quality and tones of sounds, and yet the words of others in the same language as he himself uses, be utterly unintelligible to him. There are various curious modifications of this condition illustrated by cases given, collected from various sources by the author; they may be able to converse, but though they distinguish the forms of letters, they lose entirely the power to read, or they may also lose the ability to distinguish numerals, etc. It would seem that the special apparatuses, in the brain for these particular faculties, had alone suffered. It might seem that when the centrifugal routes of expression were still nearly intact, and the lesion affected only the perceptive channels, that aphasia was hardly the correct term, notwithstanding the disturbance was still of the same general class. These interesting phenomena need still further study, and their treatment here is by no means exhaustive. On page 182, Prof Kussmaul presents a schematic drawing, representing his ideas of the routes and centres employed in speech, with his explanations accordingly of the different varieties of disorder of the functions. But it would seem from the clinical records of disorders of speech, that almost every possible interruption of communication may take place, pathologically, along these routes.

The numerous cases of functional and hysterical aphasia that

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are met with in almost every extensive practice, are briefly no-
ticed in the thirty-first chapter. He does not, however, attempt
to explain at any length their pathology. He merely says that
almost every possible morbid condition of the brain may cause
aphasia, if the routes and centres for speech are either directly
injured or if their functions are indirectly and temporarily dis-
ordered from pressure, ischæmia or collateral hyperæmia, or
perhaps from reflex irritation and irradiation. One remark of
the author's in this connection' appears to us to require explana-
tion. He says that in cases of functional or hysterical aphasia,
the words are intact, as is likewise the connection between the
words and ideas, but that the interruption is somewhere be-
tween the word centre and the muscular apparatus of speech,
that the general cortical and infra-cortical excitation, by means
of which the conception is translated into words, is too weak,
and that the so-called "moral treatment" is efficacious in
some of these cases by supplying or reinforcing this psychic
excitation. This is, it appears to us, rather too general a state-
ment, without more qualifications than it here receives, we have
met with cases of what might well be called transient hysterical
aphasia, occurring from uterine irritation in neurotic females, in
which the disorder seemed to affect higher cerebral centres than
are here admitted.

We are compelled to pass the remaining portions of this work,
those in which are described the various dysphrasic or dyslogic
disorders of speech, and its mechanical derangements from
lesions or deficiencies of the more peripheral organs concerned
in its production with only bare mention. We can honestly
refer the reader to this volume, as the most thorough and com-
plete monograph of the subject that has as yet appeared.
While the tendency of the author to amplify on certain points,
may possibly be criticised, it must be admitted that this could
hardly be avoided, and that there is a certain advantage in hav-
ing these subjects viewed from their physiological side, by so
eminent an authority as Prof. Kussmaul. And, as we have said
before, these parts materially add to the completeness of the
work.

II.—THE FUNCTIONS OF THE BRAIN.

THE FUNCTIONS OF THE BRAIN. By DAVID FERRIER, M. D.
With Numerous Illustrations; P. 323. G. P. Putnam's
Sons, New York.

(CONTINUED FROM THE JANUARY NO.)

After a consideration of the functions of the semi-circular
canals, and the maintenance of equilibrium, Dr. Ferrier passes
to a study of the functions of the optic lobes.

He says that the facts of anatomical, and of physio-
logical resaerch, mutually support the view that the *corpora
quadrigemina*, though not the centres of vision proper, are
centres of co-ordination of retinal impressions with special
motor reactions.

Before proceeding to relate the results of his own experiments,
he gives a short review of the anatomical and physiological re-
searches of Gratiolet, Meynert, Broadbent, and others. Dr. Fer-
rier describes the results of a long series of experiments con-
ducted upon various animals, and more especially upon monkeys,
in relation to the functions of these bodies. He says, in respect
to the latter class of animals, the results of a lesion of both optic
lobes were that the animal was rendered completely blind; a
fact which, among other tests, was ascertained some twelve hours
after the operation, by reason of its inability to discern the po-
sition of some milk which it was eager to reach, and which it
drank eagerly when the dish was held to its mouth. The pupils
were dilated. "For some hours the animal kept its eyes closed,
or only opened them partially when aroused, but there was no
real ptosis. Before death the eyes could be opened freely. With
the exception of sight, the other senses were retained." Move-
ments of the limbs were still made, and the animal could grasp
firmly, as before, both with hands and feet.

"To sum up the results of destructive lesions of the optic lobes,
in the various animals experimented on, we have blindness,
paralysis of irido-motor, and some oculo-motor reactions; disor-
ders of equilibrium and locomotion in frogs, and apparently in
other animals, annihilation of certain forms of emotional ex-
pression.

" Simple irritation of one optic lobe caused the opposite pupil
to become widely dilated, followed almost immediately by dilata-
tion of the pupil of the same side. The eyes are widely opened,
and the eye-brows elevated; the eye-balls are directed upward,
and the opposite to that of the optic lobe which is irritated.
The head is moved in the direction of the eyes; the ears are
strongly retracted; the arms are approximated to the sides, and
drawn back flexed at the elbows; and ultimately, when the stim-
ulation is kept up, a state of complete opisthotonus is produced.
Irritation of the testes or posterior tubercles produces the same
effects, and in addition, cries are excited varying in character
from a short bark, caused by the slightest contact of the elec-
trodes, to all varieties of vocalization, when the stimulation is
continued,

"The motor effects are shown, first on the opposite side of the
body, but ultimately both sides become affected by unilateral ir-
ritation."

Dogs and cats, the jackal, rabbits, pigs, and other animals are
affected in a similar manner, as far as could be expected in view
of their differences of organization.

The functions of these bodies appear to us to be so important

that we will give considerable space to the statements of the
views of Dr. Ferrier, and all the more so, because of the relation
which they sustain to those diseases of which tetanus may be
taken as the type.

"The destruction of the corpora quadrigemina annihilates the
manifestation of those functions which still remain practically
unimpaired by removal of the cerebral hemispheres, that is,
equilibrium, locomotion, and in some measure, emotional ex-
pression.   We have, therefore, reason to believe that the *corpora
quadrigemina* form an essential part of the central mechanism
by which these are rendered possible, and there is an evident re-
lation between the development of the ganglia, and the degree
of independence with which some of these functions are mani-
fested after removal of the hemispheres.

"The corpora quadrigemina as compared with the cerebral
hemispheres are relatively large, and in these animals particu-
larly, we find that removal of the hemispheres has less affect
on equilibrium and co-ordinate locomotion than in animals with
more highly developed hemispheres.

"The apparent retention of the power of forward progression
in frogs, after the removal of the optic lobe, seems to me, more
of the character of mere change in position, caused by sudden
reflex movements, consequent on powerful reflex stimulation;
for, in several experiments which I have made, I have found
that when the optic lobes have been thoroughly removed, the
animals cease to make true co-ordinate attempts at progression,
and are unable to maintain their ordinary attitude."

Dr. Ferrier declines any attempt to differentiate between the
optic ganglia, as such, and the underlying tracts of the nervous
system, for he says: "I do not think it possible to determine ex-
perimentally, what are the functions of the mesencephalic gan-
glia and cerebellum, apart from their connections and relations
to the crura and pons."

Says Dr. Ferrier: "Momentary irritation of the optic lobes,
excites a reaction closely resembling the sudden backward start,
which we see reflexly excited by the sudden approximation of
an external object to the eyes.   In this we have optical impres-
sions co-ordinated with muscular actions, a result of which is
to withdraw the head and eyes from dangerous approach." "The
contraction of the facial muscles, together with the general
opisthotonus which results from powerful stimulation of the
corpora quadrigemina in mammals, may be regarded as a physi-
cal manifestation of painful stimulation in general.   The phe-
nomena are of the same character as those which are induced
by the sensory irritation through the spinal centres, such as are
observed in idiopathic or traumatic tetanus.

"The clenching of the teeth, with retraction of the angles of
the mouth, which is such a common manifestation of painful
stimulation of sensory nerves, may be accounted for by the
structural connection of one of the sensory roots of the fifth

nerve with the *corpora quadrigemina.* The dilatation of the pupils, which is so readily induced by irritation of the *corpora quadrigemina*, is probably of the same nature, that is, an indication or expression of sensory irritation.

"It is well known that sudden, or painful stimulation of sensory nerves is associated with dilatation of the pupils; this reaction is produced through the medium of the sympathetic nerves, which act on the dilated fibres of the iris, for it has been found by Knoll, [Eckhardt's Beitraege,] that irritation of the *corpora quadrigemina* causes no dilatation when the cervical sympathetics have been divided.

"These results are in accordance with the theory of the coordination in the *corpora quadrigemina*, of sensory impressions with the mechanism of emotional expression." (P. 82.)

In short, the various considerations had by Dr. Ferrier, have led him to declare "that the relation between the phenomena of irritation, and destruction of the *corpora quadrigemina*, though in many respects one of a hypothetical nature,—tends to support the view that these ganglia are the centres especially concerned in the reflex expression of feeling or emotion." "This is rendered still more probable by the recently demonstrated influence which the *corpora quadrigemina*, or more properly, the deeper parts of the *corpora quadrigemina* exert on the functions of circulation, modifications of which are one of the most frequent concomitants of states of feeling or emotion. (P. 83.)

"In addition to these facts, it has been stated by *Valentin* and *Budge*, that irritation of the *corpora quad igemina* has a direct influence on the viscera, causing contractions of the stomach, intestines and bladder. These, effects, if well established, would be another proof of the relation of the *corpora quadrigemina* to the reflex manifestations of emotion, for it is well known that under certain forms of emotion, contractions of the stomach and bladder may occur, as shown by the sudden expulsion of their contents. Monkeys in particular, express terror in this manner." From all of which it would appear, as a result of Dr. Ferrier's experiments, that the *corpora quadrigemina*, whatever other functions they may perform, are concerned in the reflex expression of emotion and especially of the emotions which appear to result form visual sensations, whether as regards acts of expression or those having relation to the maintenance of equilibrium.

We would be glad to give more space to the consideration of this subject than we can in the present notice. It is one worthy of serious study, on account of its widespread and important relations, both physiological and pathological. We dismiss it, at the present, with the hope of returning to it at some time in the future.

Dr. Ferrier next begins the study of the functions of the cerebellum, a subject which, as is well known, to those who attend to the progress of knowledge in regard to the nervous

system, embraces certain of the most unsettled and closely disputed questions within the domain of physiology. The most competent among physiologists have adopted widely different views.

The cerebellum forms an apparatus, as it were, lying off to one side of the great central axis of the nervous system. It reaches out and establishes connections with the summit of the cerebro- spinal axis, by means of bands of fibres, as is well known. These bands are three in number, the superior, the middle, and the inferior.

By means of the first, the connection is made between the cerebellum and the cerebral hemisphere of the side opposite; by the middle peduncle the connection is had with the pons, and the groups of nerve cells that lie imbedded between its fibres; while by means of the inferior peduncle a connection is had with the gray matter of the spinal cord, probably with its anterior and posterior columns, or the spinal apparatus for the co-diornation of muscular movements.

But, there is comparatively a great unanimity of opinion among physiologists in respect to the view, that the cerebellum is one of the chief organs concerned in the maintenance of equilibrium in muscular action. This position, however, has been strongly contested, both on the grounds of experiment, and pathological observation.

Cases have been cited from both these sources, which have seemed to require this view to be either modified or abandoned. These are discussed at some little length by Dr. Ferrier, and especially the remarkable case cited by Combette, that of a girl in which there was found to be complete absence of the cerebellum, and yet she had been able to walk, and did not present those symptoms which we might naturally expect, if the cerebellum is charged solely with this function.

The remarks of M. Vulpian are quoted, touching still another case, in which the cerebellum was destroyed by disease, but in which the power of co-ordination of movements was retained, only the patient was observed to totter when he walked.

M. Vulpian says: " There we have a case in which the cerebellum was destroyed, and yet the patient could walk, though in an unsteady manner, but if this (Flouren's) hypothesis, were well founded he ought not to have been able to stand or walk, and especially if the combination of muscular contraction necessary, for locomotion, or maintaining the erect attitude, ought to have been altogether impossible."

Dr. Ferrier continues at some length the discussion of cases like those just cited, without reaching any very satisfactory conclusions, and then passes on to relate the results of his excitation and mutilation experiments. He reports a large number of highly suggestive observations, but which we do not have space to consider in detail. But in general, electrical excitation of different parts of the cerebellum of monkeys was found to

cause movements of the eyes in various directions, the excitation of the same parts, generally being followed by the same movements. Besides the ocular motions, "certain movements of the head and limbs were likewise produced." As regards the movement of the limbs they occurred on the same side, as that to which the half of cerebellum belonged, to which the exciting agent was applied. It will be noticed that excitation of the cerebellum led to different results from that of excitation of the cerebral hemispheres, for in the latter case, at least, according to Dr. Ferrier, the movements are generally in the opposite half of the body. This agrees perfectly with the results of a study of the mutual anatomical relations of the cerebellar and cerebral hemispheres, whether healthy or morbid. Irritation of the cerebellum is followed by contraction of the pupils, especially that of the eye of the same side; viz., the side corresponding to the half of the cerebellum operated on, and the contraction was found to be more or less permanent. No vomiting or sign of excitation of the genital organs, was observed in any of the animals operated on, though special attention was directed to these points.

Dr. Ferrier makes certain remarks *apropos* to excitation experiments on the cerebellum, which have been suggested by his exceptionally large experience with the higher animals, more especially monkeys, and which it appears to us should be remembered, in endeavoring to estimate the value of single experiments, or even short series of the same. They are as follows: " It is to be noted in reference to electrical imitation of the cerebellum, that occasionally stimulation is absolutely without effect at first, and that after a lapse of sometime, the phenomena follow with great precision. I have not been able to satisfy myself with respect to these variations, as to whether they depend on degrees of anesthesia, or states of shock, or not. From another cause, the excitability of the cerebellum is subject to variations, which render the investigation troublesome, and, unless sufficient care is taken, may easily lead to apparently contradictory results.

" Frequently, after the application of the electrodes, a condition of nystagmus comes on, and lasts for sometime, so that unless due time is allowed for the subsidence of this irritation, the results of applying the electrodes to another part become so confused with the effects of the preceding, that analysis is impossible. The results above described are those which I have obtained after careful repetition on different animals, and with due regard to those precautions which I have indicated as being necessary." (P. 101.)

Essentially the same results were obtained by operating on the cat, dog and rabbit.

The relations which Dr. Ferrier would seem to have shown to exist between the state of the cerebellum and motion of the eyes, and disturbances of equilibrium, throw some light on, and in return receive some confirmation from, "certain phenomena

frequently observable in connection with disease or injury of the
cerebellum." Dr. Ferrier also called up, in confirmation of his
experimental results, " the phenomena which are observed in a
man when a galvanic current is passed transversely through the
skull in the cerebellar region, and which were first described,
according to Dr. Ferrier, by Purkinje. (1827.)

These phenomena have been studied by Hitzig in his work on
the brain, and which was reviewed at some length in this JOUR-
NAL. (Vol. 1, p. 513, 520.)

They are described as follows by Dr. Ferrier: " When a gal-
vanic current of moderate intensity is passed through the head
by placing the poles of a battery in the mastoid fossæ just behind
the ears, the individual experiences a feeling of vertigo, in which
the relation of his body to surrounding objects seems to be, or
is, actually disturbed, or external objects seem to alter their rela-
tion to him. The direction in which the equilibrium is disturbed,
or in which objects seem to move, depends on the direction of
the current through the head. When the positive pole, or
anode, is placed on the right mastoid fossa, and the negative
pole, or cathode, on the left, so that the current passes from
right to left, at the moment when the circuit is closed, the head
and body suddenly sink toward the anode, while external objects
seem to wheel to the left. . . . When the eyes are closed,
the appearance of motion is transfered to the individual himself,
who feels as if he were whirled from right to left, or as if the
basis of support on the left side had been suddenly withdrawn.
The direction is exactly reversed when the positive pole is placed
on the left, and the negative on the right side, or, while the
electrodes maintain their former position when the circuit is
broken." (P. 106.)

The eyeballs are moved, according to Hitzig, in the same
direction as the head, and frequently are thrown into oscillations
or nystagmus, that is, the movement is towards the anode.
This motion is the same as that which results from irritation
of the corresponding side of the cerebellum. It is the anode,
(positive pole) therefore which excites the cerebellum. This
was found to be a general fact by Hitzig, as regards the action
of the galvanic current, on the brain. Besides the objective
phenomena referred to, there are certain subjective or mental
states connected with them worthy of consideration. But these
subjective phenomena (feeling of vertigo, etc.) are only ac-
companiements of the objective, and depend, not on the cere-
bellum, but on the cerebral hemispheres, just as reflex action
may be accompanied by consciousness, though consciousness
plays no part in the mechanism itself." This position we
believe to be a correct one, and worthy of more extended con-
sideration than we can give it here.

It will not be possible for us to quote and comment on all the
passages of interest in Dr. Ferrier's highly suggestive chapter
on the functions of the cerebellum, but we cannot pass them
all by.

He says, that "the cerebellum would therefore seem to be a complex arrangement of individually differentiated centres, which in associated action regulate the various muscular adjustments necessary to maintain equilibrium of the body, each tendency to the displacement of the equilibrium round a horizontal, vertical or intermediate axis, acting as a stimulus to the special centre which calls into play the antagonistic or compensatory action."

This view would seem to be borne out by the facts of comparative anatomy, as well as by experiment. As regards the physiological relations of the cerebellar hemispheres to the cerebral, Dr. Ferrier has the following amongst other important remarks: He says " but while we may theoretically in all animals, and practically in many, abolish consciousness and volition by removal of the cerebral hemispheres, and still leave the mechanism of equilibration intact, yet in the normal state, cerebellar activity is associated with that of the hemispheres, and it is this association which serves to explain many of the facts which would seem to oppose the view which we have taken of the function of the cerebellum as a whole. * * * * * *The same muscular adjustments which are capable of being effected by the cerebellum, are also under the control of the will, and may be carried out by the cerebral hemispheres independently of the cerebellum.* Hence it is that lesions of the cerebellum, while interfering with the mechanical adjustments against disturbances of bodily equilibrium, do not cause paralysis of voluntary motion of the muscles which are concerned in these actions. This is an exceedingly important fact which, though disputed by some, seems to be established experimentally beyond all question."

In speaking of the effects of lesions of the cerebellum, Dr. Ferrier makes a distinction as between reflex adjustment, and reflex action, as the latter phrase is ordinarily understood. He says, " in terming the effect of cerebellar lesion a *paralysis of reflex adjustment,* I do not thereby imply a *paralysis of reflex action.* This which would result from spinal lesion must necessarily coincide with paralysis of voluntary motion, as the path from the hemispheres would thereby be interrupted. What is implied is, that the same *combinations* of muscular action, which are co-ordinated in the cerebellum for the maintenance of the equilibrium, are capable of being called into *voluntary* action by the hemispheres. Hence, though lesions of the cerebellum destroy the *self-adjusting* co-ordination of muscular combinations necessary to maintain the equilibrium, they do not cause loss or paralysis of voluntary motion in the same muscles. So, conversely, by removal of the cerebral hemispheres, we cause paralysis of voluntary motion, but do not affect the independent mechanism of cerebellar co-ordination. When we make this distinction we are enabled to understand how limited lesions of the cerebellum may produce only

transient effects, and how even complete destruction of the cerebellum may ultimately be recovered from." In this way would Dr. Ferrier explain cases like that of Combette, or those of Weir Mitchell, who found pigeons to recover ultimately their power to maintain an equilibrium, after a time, notwithstanding the complete and permanent removal of the organ in question.

With most of the views which we have quoted, we can heartily agree. But inasmuch as they do not fully represent the case as it seems to us it should be, we will proceed to give an outline of our own opinions, as to the nature of the functions of the cerebellum.

We have been led to regard the cerebellum whatever its other functions may be,—as an important part of the nervous apparatus for the co-ordination of muscular motion, the highest part in fact, though in a peculiar sense. It forms no portion of the immediate apparatus proper, for this purpose, for this lies wholly in the cord, medulla, pons, and basal system of ganglia. But the cerebellum lies off to one side, and has as *its chief office the co-ordination of muscular motions, in subserviency to the requirements of thought and emotion for the purposes of our higher or intellectual lives.* It uses the *immediate* nervous apparatuses for muscular co-ordination, so as *through them* to make muscular action subservient to the purposes of mind, more especially the expression in many ways of thought and feeling. Hence it is connected on the one hand with the cerebral hemispheres, and on the other by means of its middle and inferior peduncles, with the immediate co-ordinating apparatuses in the pons, medulla, and cord. Hence injury or even removal of the cerebellum does not finally destroy the power to co-ordinate, though lesions of the same, at least in the higher animals, always appear to disturb the maintenance of equilibrium. And hence, it comes to pass, as Dr. Ferrier says, that removal of the cerebral hemispheres in the lower animals does not always, or at times not at all, destroy the power to co-ordinate muscular action, for by such an operation the real apparatus for the co-ordination of muscular motion may remain intact.

It is the function of the cerebellum, we repeat, to secure those greatly complicated and delicate muscular co-ordinations and adjustments, required for the adequate expression by voice, hand, face, posture, etc., etc., of the innumerable thoughts, emotions, etc., which we make known by expressive signs to the outer world. This opinion requires that the cerebrum and cerebellum should have close anatomical relations. These, they have in point of fact, through the superior peduncles of the cerebellum. As will be seen in a later part of this notice, when speaking of the cerebrum and its functions, we hold to the localization of function in the same, in a certain sense. We may indeed anticipate so far as to say that it is our opinion,

that different parts of the cerebral cortex, are the seats, and in a sense, the organs of particular mental functions or faculties. These particular areas of the cortex—convolutions for example—are connected with particular and definite regions,—or convolution, if it pleases one to say so, of the cerebellum.

From the cortical seat of a particular thought or emotion, which gives rise to expressive acts, an impulse passes by way of the superior peduncle of the cerebellum to a particular part of the cortex of the opposite cerebellar hemispere. This is excited in turn, and sends an impulse to one or more definite cell groups of the corpus dentatum, in the heart of each cerebellar hemisphere, and so, by way of the middle and inferior peduncles of the cerebellum, to the real immediate co-ordinating apparatus which is co-extensive with, and in close relations to, the central motor tract proper. In this view, the cerebellum, we repeat, does not form any part of the immediate co-ordinating apparatus, though it uses the latter for the purposes mentioned above.

There is a point in each cerebellar cortex, which corresponds to each particular locality of the cerebral cortex, so that they may be connected with each other for the purpose, that every thought or state of feeling, every volition which seeks expression, may find in the cerebellum a mechanism for the *involuntary* co-ordination of the muscular actions necessary to secure expression. But it may be possible to perform the same acts, or at least many of them, under conscious direction of the will, even in the absence of the cerebellum. just as in the case of breathing, which is ordinarily automatic, and unconsciously carried on, but which is often done by an effort of the will.

Such views as we have briefly described, in great measure, appear to coincide with those of Dr. Ferrier, as regards the cerebellum. They certainly seem to harmonize all known facts, as far as we know, better than any other hypothesis with which we are acquainted.

Dr. Ferrier rejects the opinion of Lussana (*Fisiologia dei centri Nervosi Encefalici, etc., etc.*—Vol. II., pp. 322–342—*Sistema peduncolare e cerrelleto. Padova, 1871*), that the cerebellum is the seat of the muscular sense, and correctly, as we think. It is, therefore, in all probability, that part of the nervous apparatus which is chiefly concerned in *indirect, unconscious,* co-ordination, or delicate combination and adjustment, of muscular movements, which are especially subservient to the outward expression,—in the widest sense of the words—of thought and emotion.

Hence as already said it does not form a part of the *immediate* nervous coordinating apparatus, but in a somewhat circuitous manner, establishes a net-work of relations between the cerebrum and the co-ordinating apparatus proper, which lies below it. But we may find opportunity for further remarks on this subject,

when speaking of the functions and relations of the cerebrum, an account of which is crowded out of the present, but will be given in our October number.

<div align="center">(TO BE CONTINUED.)</div>

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VI.—SHORTER NOTICES.

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I. MYELITIS OF THE ANTERIOR HORNS, OR SPINAL PARALYSIS OF THE ADULT AND CHILD. By E. C. Seguin, M. D. New York: G. P. Putnam's Sons, 1877. 120 pp. Chicago: Jansen, McClurg & Co.

II. A COURSE OF PRACTICAL HISTOLOGY; BEING AN INTRODUCTION TO THE USE OF THE MICROSCOPE. By Edward Albert Schaefer, Assistant-Professor of Physiology in University College, London. With Illustrations on Wood. Philadelphia: Henry C. Lea, 1877. Chicago: Jansen, McClurg & Co.

III. TRANSACTIONS OF THE AMERICAN GYNECOLOGICAL SOCIETY. Vol. I. For the year 1876. Boston: H. O. Houghton & Co. 396 pages.

IV. CONTRIBUTIONS TO REPARATIVE SURGERY. By Gurdon Buck, M. D. Illustrated by numerous engravings. New York: D. Appleton & Co., 1876. 237 pages.

V. THE USE AND VALUE OF ARSENIC IN THE TREATMENT OF DISEASES OF THE SKIN. By L. Duncan Bulkley, A. M., M. D. New York: D. Appleton & Co., 1876. 45 pages.

VI. TRANSACTIONS OF THE 79TH ANNUAL SESSION OF THE MEDICAL AND CHIRURGICAL FACULTY OF MARYLAND. Held at Baltimore, April, 1877. 190 pages.

VII. THERAPEUTIC USE OF FARADAIC AND GALVANIC CURRENTS IN THE ELECTRO-THERMAL BATH, with History of Cases. By Justin Hayes, M. D. Chicago: Jansen, McClurg & Co., 1877. 112 pages.

VIII. GENERAL INDEX TO THE NEW YORK MEDICAL JOURNAL. From April, 1865, to June, 1876 (twenty-three volumes). By James B. Hunter, M. D. New York: D. Appleton & Co., 1877. 144 pages. Chicago: Jansen, McClurg & Co.

IX. ATLAS OF SKIN DISEASES. By Louis A. Duhring, M. D., Philadelphia, 1877. J. B. Lippincott & Co. Chicago: Jansen, McClurg & Co.

I. In the issue of this JOURNAL for April, 1875, we noticed a *brochure* by Dr. Seguin on spinal paralysis of the adult. The approval of and demand for that essay, on the part of the medical profession, has led to the production of the present able and well-written monograph on anterior polio-myelitis.

After a brief historical sketch, the author proceeds to the narration of clinical histories, of which he has here brought together over forty, some of his own observation and others collected from various sources in medical literature. Then, comes in order, a careful analytic and synthetic study of the symptoms, and of the pathological anatomy of the disease in adults and in children, remarks on the diagnosis, etiology, treatment and prognosis, and, finally, the author's general conclusions, which we cannot do better than to extract bodily in this notice. He sums up as follows:

1. "Careful study of a large number of cases of disease shows that there is an affection common to all ages of life, characterized by atrophic paralysis of one or more limbs, rarely of other parts, loss of faradic reaction, and diminution or loss of galvanic reaction in the paralyzed muscles; by remarkable freedom from anæsthesia and retention of urine, and absence of bed sores; and by temporary numbness and referred pains.

2. The course of the disease in children is nearly always acute; in about fifty per cent. acute and febrile. In adults, it runs an acute febrile, an acute non-febrile, a sub-acute febrile, a sub-acute non-febrile, or a chronic course. The last-named type of the disease is rare; excessively so in children.

3. In adults and in children the lesion of the spinal cord consists in myelitis of the anterior horns, with atrophy (through granular degeneration) of the ganglion cells of this part. The motor cells constituting the trophic centre (Waller's law) of nerve trunks are destroyed, and the resulting atrophic paralysis greatly resembles that of peripheral paralysis.

4. The causes of myelitis of the anterior horns are: Predisposing, age and sex. In childhood the two sexes are about equally liable to the disease, and are more frequently attacked when from one to four years of age. Adult males are much more often affected than women; and the best years of life, twenty to thirty-five years, are those during which the disease occurs. Exciting causes: The only one that we have good reason to believe effectual is the impression of cold or dampness; the affection is often, consequently, a paralysis *a frigore*.

5. The treatment consists in the heroic use of antiphlogistic means, such as counter-irritation to the spinal region, the internal administration of ergot, belladonna, iodide of potassium, during the acute and sub-acute stages of the disease; and of the

judicious use of galvanism, faradism, massage, douches, tonic and supporting treatment, and orthopœdic apparatus during the stage of residual atrophy.

6. The prognosis is very good as regards life, but unfavorable with respect to functions of paralyzed parts; very few patients recover perfectly from myelitis of the anterior horns.

II. This little work of nearly three hundred pages is devoted to the description of the methods of histological preparation, and examination, in regard to which its author, one of the ablest microscopists in Great Britain, is thoroughly well informed. Its title, an introduction to the use of the microscope, is modest enough, the reader is pretty thoroughly introduced to all or nearly all the best methods of histological manipulation. It appears to us to be the best work on its subject and of its compass, in our language, and we doubt not will take its place at once as the most approved text-book on practical histology.

III. The Transactions of the American Gynæcological Society forms a very handsome volume of nearly four hundred pages, containing both the papers and the discussions read at the first meeting in New York, in June, 1876. Many of these are of decided interest, though none of them are so directly neurological in their character as to call for notice here.

These special societies, it seems to us, are the ones that will serve more than the general ones to directly advance medical science, and the volume before us speaks well for the usefulness of the association it represents.

IV. This little work, the last contribution of the late Dr. Buck, should have received an earlier notice from us in this JOURNAL. It is in every respect a monument of the advance in surgical science, showing the power of adaptive skill to remedy conditions, at first sight apparently hopeless, and which, many of them at least, would even, a short time ago, have been considered as beyond the reach of surgical aid. Success such as is shown in many of the cases recorded here, goes far to justify even the vulgar exaggerated opinion of operative surgery, of which, indeed, this is one of the very highest branches. The work is one well worth adding to the library of every surgeon and general practitioner.

V. This is a reprint from the *New York Medical Journal* of August, 1876, of an essay read before the American Medical Association in June last. We cannot do better than reproduce the conclusions of the author, which in the memoir are well supported by argument and authorities. They are as follows:

1. Arsenic, when administered in medicinal doses, has quite another action from that manifested by poisonous doses; the average dose of the former is one twenty-fourth of a grain of arsenious acid, while the smallest toxic dose is stated at two grains.

2. Arsenic in medicinal doses does not produce any slow poisoning, but has been administered for months or years in quantities, a small portion of whose aggregate amount would destroy life at once. Hebra has administered a total of more than half an ounce to a single patient. The accounts of the toxiphagi of styria are true, and arsenic is eaten by some for many years without apparent ill effect.

3. Arsenic given by a careful practitioner, in doses to be effective, need never produce any symptoms which should cause regret.

4. Arsenic is eliminated very rapidly, chiefly by the bowels and kidneys, so that the urine shows evidences of it in a few hours; no trace of it can be found on careful analysis of the body after death, two weeks after the last dose of arsenic.

5. Arsenic, therefore, does not accumulate in the system, and no fear of this need be entertained, but, when it is administered in increasing doses, absorption may be hindered, and when the doses become very large, active absorption of the large dose may give rise to the suspicion of cumulative action.

6. The first symptom of a full dose of arsenic, in a very large share of cases, is a fullness about the face and eyes, and conjunctival irritation and tenderness. This need not be exceeded, but may often be kept up with advantage to a slight degree till the disease yields. Before any harm is done by the arsenic, either this or a slight nausea or diarrhœa manifests itself.

7. Arsenic should always be given with or just after meals; it is often best to give it alone, or with a small amount of bitter infusion.

8. The bowels should be first well purged, and an occasional laxative will both assist the action of the drug, and prevent or modify some of its unpleasant effects.

9. If the urine becomes loaded and the tongue coated, it is best to stop the medicine for a short time, and give diuretics; some of these disturbances can be prevented by combining an alkali, as acetate of potassa, carbonate of soda, or aromatic spirits of ammonia, with the arsenic.

10. The most serviceable forms in which to use arsenic, named in the order of their value, are: solution of the chloride of arsenic, solution of the arseniate of potassa, that of the arseniate of soda, and the arseniate of ammonia, arsenious acid, iodide of arsenic, and the arseniates of iron and quinine; of as yet untried efficacy, solution of the chloro-phosphide of arsenic and arseniate of antimony.

11. The dose of arsenic, small at first, is to be increased slowly until some of its physiological effects are manifested or the disease yields; it may then be somewhat diminished.

12. It is very important that arsenic be taken very regularly and persistently, and always under the supervision and frequent inspection of the physician.

13. Arsenic is valuable in chronic rheumatism, hence is use-

ful in arthritic eruptions; it is serviceable in certain neuroses, as chorea and neuralgia, therefore in skin diseases with neurotic elements; and it possesses anti-malarial properties, and is consequently serviceable in diseases of the skin showing periodic symptoms, as intermittent urticaria, etc., likewise in patients with other skin-diseases who have been exposed to miasmatic influences.

14. Arsenic is certainly valuable in psoriasis, eczema, pemphigus, acne, and lichen, in proper cases and when due regard is paid to the secretory organs, and to diet and other elements of general health; of less certain value in lupus, ichthyosis, sycosis, verruca and epitheliomatous and cancerous diseases; it is absolutely useless or harmful in the syphilodermata, the animal or vegetable parasitic diseases, (except in rare cases,) in elephantiasis græcorum and arabum, in purpura, true prurigo, herpes zoster, scleroderma, molluscum contagiosum and fibrosum, keloid, vitiligo, nævus, etc.

15. The only local application of arsenic which is justifiable is either one where the strength is so weak, and the extent of its use so small, that there is no danger from absorption, which may occur when not expected, or, one of such a strength as to kill the adjoining tissue at once, and so prevent absorption, as is the case with Marsden's mucilage.

The allusion above to chorea and neuralgias in which arsenic is serviceable, indicates one of its uses in nervous disease, and the above therapeutic conclusions are therefore of service to the neurologist as well as to the dermatologist.

VI. The Transactions of the Maryland Faculty, contain as is common in the proceedings of state medical societies, the usual reports of sections embodying more or less fully the more recent advances in their respective departments of medicine. But besides these, this volume contains the annual address, by Dr. Weir Mitchell, from which we have made extracts in another part of this JOURNAL. The essay of Dr. Arnold on the "Medico-legal Relations of Melancholia," forming the report of the section on Psychology and Physiology, though short, is good and suggestive. As a whole, the volume compares very favorably with the Transactions of other similar bodies in this country.

VII. The publication of a work like this little volume we must consider as most unfortunate for its author. It hardly seems destined for the consideration of the medical profession, but rather to have been intended as a kind of popular advertisement of its author in which he succeeds, however, in exposing most thoroughly only his own vanity and bad taste. His book is simply a series of imperfect histories of cases given for the purpose of showing the advantages of the electric bath over other methods of treatment of various diseases, some of them indicating very dubious diagnoses and a very considerable igno-

rance of pathology, as well as of the art of writing correct English. Much is made of what is called the "vitalized treatment," a rather quackish name—given to a sort of general electrization. Much of the matter is altogether irrelevant and introduced only for the purpose, of gratifying the writer's *penchant* for what he considers elegant composition. Such passages as the following will serve to exhibit his capabilities in this direction: "With one stroke of his razor the work was done with the skill of a surgeon, and without leaving a note or good-bye, his spirit left to join his peers in eternity."

The book is really not worthy of as much notice as we have given it, and we should have passed it by with the briefest mention, but for the fact of the unprofessional and apparently intentional discourtesy shown in it towards respectable and eminent members of the profession. Retailing to the public patients stories of their former physicians, such as is indulged in here, seems to us one of the venial offenses against professional good morals. Inasmuch, therefore, as its author has acquired a standing as a regular member of the medical profession of Chicago, we notice the work, more than anything else, to call attention to his violation of general and professional ethics.

VIII. Dr. Hunter has prepared and Messrs. Appleton & Co. have published in book-form a very neat and convenient general index to the *New York Medical Journal*, for the past ten or eleven years. In style and appearance, this is decidedly beyond any similar publication that we have recently seen, and it is in its way a very useful as well as handsome addition to a medical library.

IX. We have given our opinion already of this work in our notice of the first part, which appeared some time since. The second fully bears out the promise of the first, it is every way admirable. We know of no similar publication that equals it in this country or in Great Britain. It will be a profitable investment for every one who can afford the very reasonable price, considering the nature and execution of the work, at which it is offered.

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Editorial Department.

MORAL INSANITY.

" Do facts justify the recognition of moral insanity as a distinct type of mental disease?' This was the somewhat objectionable title of a paper read before the section on Medical Jurisprudence and Psychology at the recent meeting of the American Medical Association in this city, by a prominent asylum superintendent, and discussed by a number of the leading alienists of the country. The author used a rather ingenious arithmetical argument, showing from the statistics of American asylums that the recognition of this form of disease had evidently become much less general in practice, and that in theory also its advocates were weakening and becoming fewer. Even in the very full and elaborate classification of his patients adopted in the reports of one superintendent who had been prominent as an upholder of the existence of this form of psychosis—and the author of the paper made rather a special point of this instance—there were no cases of moral insanity.

Whatever may be thought of this argument, the principal one of the paper, there seemed no doubt that the majority of those present were in sympathy with the conclusions of its author. Two, at least, of the speakers in the discussion that followed, explicitly repudiated any belief in moral insanity, and others were apparently equally decided in the same opinion. If a vote of the section had been taken upon the question, there is every probability that its decision would have been unfavorable to a recognition of the existence of moral insanity. The subject is one of great importance in a medico-legal point of view, and if the above is, as it appears to be, the prevalent opinion among alienists, the question of its cor-

rectness is worthy of the most serious attention. The Association, as a whole, took pains to put itself on record in regard to a matter infinitely less important than this, inasmuch as at the best it could only affect pecuniary interests and never human life. If it be understood in our courts of justice that the weight of scientific authority is against the existence of moral irresponsibility, independent of intellectual disorder as commonly distinguished, and yet if such a condition really exists, the possibility of grave injustice will be evident to every one. It was stated in the paper read, and the discussion that followed, that few of the highest authorities, medical or legal, recognized the condition as ever occurring —a statement, however, with which, in its strictest sense, we cannot agree. For, leaving aside all discussion as to names and terms, in regard to which there is generally more or less confusion, and considering only the facts, we find it admitted by very many of the ablest writers, that the moral faculties may be affected without necessarily implicating the mental faculties properly so-called. Among them we need only mention the names of Krafft-Ebing, Dagonet, Legrand du Saulle, and Flemming, to show that the leaders of continental medicine in this special department are not at all on one side.

Clinically, we do not see how we can explain morbid impulses which even generally sane persons sometimes experience, and which require to be only a little stronger in those cases to impel their victim to the most absurd and even criminal acts against the dictates of their reason, without admitting the existence of a certain degree of independent moral irresponsibility. In such cases as these the highest moral sense may not be able to restrain the morbid tendency from overcoming a weak will power.

But it is to cases where this moral sense is defective, that the term moral insanity most properly applies. This defect may be, and often is, hereditary, and this is the form most generally recognized. But it seems not improbable that it may also be acquired through disease. The brain is the organ of all our psychic faculties, moral as well as intellectual, and its derangement, it is according to reason to believe, may affect one set of faculties as well as another. That we rarely meet with

a case of moral insanity uncomplicated with intellectual derangement, is no proof of its non-occurrence; the numerical argument is no argument at all, and had there never been a case observed, it would still be impossible to demonstrate from any pathological or clinical reasons that it could not occur.

The argument sometimes offered that it would be hurtful to society to recognize such a form of disease as moral insanity, appears to us slightly jesuitical, if that is a proper word. We are not such absolute utilitarians in morals or in social ethics, as to perceive no higher function of justice than that of ministering to the greatest good of the greatest number on such merely politic grounds, and without regard for the welfare of the unfortunate exceptional minority. And if our laws fail to afford protection for and against the morally insane, it is time for them to be amended. Such an argument is moreover unscientific and reactionary.

We intend, at a future time. to give further attention to this subject in the pages of this JOURNAL; our present object in noting it, is to simply state our own position upon one of the most important medico-legal questions of insanity.

The following note explains itself :

43, West Fifty-fourth St.. NEW YORK.
June 8, 1877.

Dear Sir: I have the honor to offer to the American Neurological Association a prize of two hundred and fifty dollars to be accorded at its next annual meeting, on the favorable report of a committee of three of its members, to the author of the best essay on the anatomical and physiological effects of strychnia on the brain, the spinal cord and the nerves.

The only conditions I have to make are:

1. That the essay shall be based upon original observation and experiment.

2. That the prize shall be open to competition irrespective of nationality, under such regulations as the committee may deem proper.

3. That if the best essay be not up to the highest standard

demanded by the present advanced state of science, the prize shall not be awarded.

4. That the successful competitor shall give to the Association his or her entire right, title, or interest in the essay, and that this latter shall be published by the Association in its transactions.

Although I do not make it a condition, I would respectfully suggest that the committee consist of Dr. S. Weir Mitchell, of Philadelphia, Dr. J. S. Jewell, of Chicago, and Dr. E. C. Seguin, of New York.   I am, Sir, yours sincerely,

WILLIAM A. HAMMOND.

DR. J. S. JEWELL, President.

The above-named gentlemen were duly appointed a committee on the Hammond prize.

Competing essays, accompanied by sealed envelopes, are to be sent to Dr. E. C. Seguin, 41, West Twentieth Street, New York, on or before April 1, 1878.

The committee reserve the right of awarding no prize.

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Several important Reviews have been crowded out on account of the excessive amount of matter admitted into the first department of this number of the JOURNAL. For the same reason, some editorial matter which should have appeared has been omitted.

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The lateness in the appearence of the present number is due to unavoidable delays in the receipt of the proceedings of the American Neurological Association.

ℌeriscope.

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a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

CEREBRAL LOCALIZATIONS.—In a short note reprinted from the *St. Bartholomew Hospital Reports*, vol. XIII., Dr. Ainslie Hollis maintains that in the present state of our knowledge, we have only the assurance that there exist in the brain a posterior, a retentive system, and an anterior or expressive system. The expressive system may be said, generally, to consist of the fronto-parietal convolutions. Of these the parietal convolutions, immediately bounding the great fissure of Rolando, are concerned in the movements of limbs, neck, back, etc.,—that is to say, the acquired movements of these parts; for Soltmann and others have found that in the very young, before experience has been acquired, the movements described by Hitzig, etc., as depending on electrization of these convolutions and no others are not in the same way present. The adjoining frontal convolutions are concerned in the complex symbolic actions of speaking, numbering, writing, etc., as has partly been made out by direct pathological evidence, and partly may be inferred from the greater frontal development in cultured races as compared with savages, whose sense-acuteness is not accompanied by the intelligence which involves a highly developed system of symbolic expressions. What Dr. Ainslie Hollis calls the *retentive* (better, perhaps, the receptive) system consists, he believes, of the posterior or occipito-temporal lobes. He adduces two cases in support of this position. One of these was noted by Dr. Bateman in his essay on Aphasia— that of a gentleman who put vinegar on his food instead of pepper, and said "How bright the poker looks," but adding, when told he meant the fire, "Yes, I mean the fire." Dr. Bateman called this (with some hesitation) a case of amnesic aphasia, supposing that "the idea was conceived, but the means of communication with the external world did not exist." But as the autopsy showed that the frontal lobes were perfectly healthy, and only the posterior third of the left hemisphere was diseased, it is rather, Dr. Ainslie Hollis urges, to be supposed that, while the power of expression was intact, there was a loss of power of appreciating or recognizing the attributes of objects. His other case is that of a letter sorter,

who became unable to do his work; first losing, as he declared, the clear mental picture of the position and relations of the openings in his nest of pigeon-holes. Here the disease proved to be tumor in the left temporal lobe. In conclusion, the author utters a warning against the attempt to localize in the cortex too closely the several faculties of the mind. It is preposterous to expect that similar cells are reserved for similar functions in all human brains, knowing what we do of the great diversity in man's mental nature, his various occupations, proclivities and talents. *Mind,* Apr., 1877.

The following are the general conclusions at the close of a very elaborate article by MM. Charcot and Pitres, running through several numbers of the *Revue Mensuelle de Médecine et de Chirurgie* and concluded in the June issue:

1. The cortex of the human brain is not functionally homogeneous; only one part of the convolutions is devoted to the regular exercise of the voluntary movements. This part, which we may designate as the cortical motor zone, comprehends the paracentral lobule, the ascending frontal convolution, the ascending parietal convolution, and perhaps also the foot of the frontal convolutions.

2. All cortical lesions, of whatever extent, lying outside of this motor cortical zone, are latent as regards troubles of motility; that is, they cause neither paralysis nor convulsions. We may add that they are never accompanied by secondary degenerations of the spinal cord.

3. On the other hand, destructive lesions, even when very limited, attacking, either directly or indirectly, the motor zone, necessarily provoke troubles of voluntary motility.

4. If the lesion is *brusque,* if it destroys at once a large part of the cortical motor zone, it gives rise to a sudden hemiplegia, accompanied later by a secondary degeneration of the spinal cord, and later, developing contracture of the paralyzed muscles, resembling altogether the common form of central hemiplegia.

5. If the lesion is limited to a restricted tract of the cortical motor zone, it gives rise to monoplegias (suppression of functions) and to convulsions, generally of the form we have studied under the name of partial epilepsy (phenomena of irritation). After a certain period these very limited destructive lesions of the motor zone cause a secondary degeneration, which descends through the cerebral peduncle and the medulla oblongata into the lateral column of the opposite side of the cord.

6. The study of the paralyses and the convulsions of cortical origin demonstrates that the cortical motor centres for the two members of the opposite side are situated in the paracentral lobule, and in the two upper thirds of the ascending convolutions, and that the centres for the movements of the lower part of the face are located in the lower third of these convolutions, near the fissure of Sylvius.

7. It is very probable that the centre for the isolated movements of the upper member are situated in the middle third of the ascending frontal convolution of the opposite side.

8. Finally, we do not know yet exactly the situation of the cortical

motor centres for the movements of the nape, the neck, the eyes, or the eyelids.

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The Vaso-Motor Nerve-Roots in the Sciatic.—The following is the abstract of a memoir by Stricker on the vaso-motor roots in the sciatic. (*Sitzungber, der Akad. d. Wiss.* Wien, July, 1876) and some criticisms on the same, and reinvestigations of the same subject by A. Cossy, (*Arch. de Phys.*, 832, 1876) as given in the *Revue des Sciences Médicales*, Apr., 1877.

The author divides his memoir into three chapters.

In the first, he studies the origin of the vaso-dilator fibres of the sciatic nerve. These fibres are direct and indirect; the direct are contained in the posterior roots of the fourth and fifth lumbar pairs; the mechanical or electrical excitation of the peripheral ends of these roots causes an elevation of temperature in the corresponding members. The indirect fibres follow the track of the superior lumbar and the thoracic nerves. Their origin may be demonstrated as far as to the fourth thoracic pair.

In the second chapter, he explains the method followed in his experimentation.

Finally the third chapter contains the account of two experiments, of which we give the conclusions.

The first was on a young dog, whose spinal cord was divided about the fourth dorsal vertebra. Repeated sections of the sensory root of the right sciatic nerve (right sensory root of the last lumbar pair) caused in the corresponding limb a rise in temperature of $10°$ (from $22°$ 2 to $32°$ $5C$); its excitation only caused a rise of $4°$ 4. Excitation of the motor root caused first a reduction, then an elevation of temperature.

In the same animal, excitation of the peripheral end of the left sciatic, recently divided, caused an elevation of temperature of between $2°$ and $10°$.

Finally separation of the lumbar from the dorsal cord, caused an elevation of temperature of the posterior members lasting from two to many days.

The second experiment was also on a dog, whose cord was cut between the fifth and sixth dorsal vertebra. The section or ligature of the sensory root of one lumbar pair caused a rise of temperature of from $1°$ to $3°$.

Section or excitation of the corresponding motor root gave no results.

Altogether, there was a constant elevation of temperature after section or excitation of the motor roots in eight animals experimented upon. On the contrary, the same operations performed upon the motor roots produced only a very slight elevation of temperature or none at all.

In this memoir we have therefore this new evidence of the presence of vaso-dilator fibres in the posterior roots of the fourth and fifth lumbar nerves, in opposition to the usual opinion, according to which the vaso-motor fibres leave the cord by the anterior roots.

This conclusion seemed premature to M. Cossy, who made, under M. Vulpian's direction, two experiments to test its truth. The results he obtained are analogous rather than similar to those of Stricker. In fact, the

rise of temperature consecutive to the section, and especially to the excita tion of the posterior roots of these last lumbar pairs, is not produced in any constant fashion. Further, the section of the anterior roots left their temperature stationary, and their excitation constantly increased it. But experiments carried out so minutely are free from many causes of error that Stricker could not avoid. Such are, first, the muscular contractions which in non-curarized animals, may contribute to increase the tempera- ture; in the next place, it is difficult to take this temperature, and then we have still the procedure adopted for the division of the cord, difficult in its application and often uncertain of success.

Finally, the modifications of temperature observed by Stricker differ much from those produced by electrization of the vaso-dilator nerves that have been well studied up to the present date, the chorda tympani, for ex- ample. We are not yet in a position, therefore, to consider the existence of vaso-motor nerves in the posterior roots of the lumbar nerves, as estab- lished.

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NEW RESEARCHES ON THE LIVER.—Dr. B. F. Lautenbach, in an inter- esting paper in the *Philadelphia Med. Times*, of May 26, publishes the re- sults of some very important experiments on the functions of the liver, part of which were made in connection with Prof. Schiff at Florence, and part independently.

He found that ligation of the portal vein in mammals produced the fol- lowing symptoms, which we state in the author's own words:

"A great tendency to sleep, owing to the abolition of both tactile and general sensibility.

"A diminished frequency in the beat of the heart, and an increased, fol- lowed by a decreased arterial pressure.

"Paresis of the heart—arresting fibres of the pneumogastrics, so that irri- tation of the sympathico-vagi trunks will no longer cause cessation of the heart's movements. In this connection it must be stated that, following the ligature, contractions of the diaphragm occur isochronous with the beat of the heart, and disappear when the left phrenicus is lifted off the pericardium, to occur again when this is again brought in contact with the heart. I have frequently observed this symptom in animals that have been poisoned with drugs which produce paralysis of the heart arrestors; and I have shown (*Phil. Med. Times*, Mar. 31, 1877.) that this phenomena is due to an electrical current which is developed in the muscles of the heart and conducted by the left phrenic nerve (the right not being in contact with the pericardium, its presence or removal has no effect on the con- tractions) to the diaphragm.

"A great diminution in the number of the respiratory movements, which frequently become stertorous.

"Shortly before death a most curious symptom can frequently be ob- served. The animal has ceased to respire for several minutes, there is no heart beat to be felt, and to all purposes the animal is dead; if, however, you now draw on the trunk of the carotid slightly downward, the animal

frequently commences again to breathe, and the heart-beat again becomes manifest, to cease again after several minutes. Should artificial respiration be resorted to, the beat of the heart would continue for an indefinite period of time."

In cold-blooded animals these phenomena were lacking after extirpation of the liver, but appeared in some cases in which he experimented by tying the hepatic vena cava in the frog.

In seeking the cause of these symptoms, the author excludes portal congestion because ligature of the hepatic veins produced no such symptoms, anæmia, since the convulsions that would accompany death from that condition were wanting, accumulation of biliary matters, since the ligation of the bile ducts produced no such phenomena. Nor can the symptoms be due to the formation of septic poison in the liver through decomposition, as the time is hardly sufficient, the poison could not be detected by inoculation of other animals, and in experiments in which this factor was carefully eliminated by supplying the liver with blood amply enough to prevent decomposition, the results were the same.

These hypotheses being disposed of, Dr. Lautenbach next offers what seems to him the true explanation. Noticing that certain animals secrete normally, and others pathologically, virulent poisons, he asks is it not possible that all animals produce in their systems poisons, which, if not excreted or destroyed internally, would rapidly prove fatal to their generators. Then following he seeks to prove that the symptoms observed after the tying of the portal vein are due to the accumulation in the system of a poison, that under normal conditions is destroyed by the liver. Reasoning from this he concluded that in the blood of an animal whose portal vein was tied, there would be found poison which could be detected by its action on lower animals, such as frogs, especially on such as had had their liver extirpated. The experiments performed to test this question proved the fact, the frogs dying with same symptoms as the higher animals after the operation of tying the portal vein. Control experiments with the blood of the same animal before the ligation and with that from animals whose vena cava had been tied, or in which a sling only had been placed around the vena porta, produced absolutely no effect on frogs. The author therefore concludes that the blood of an animal whose portal vein has been tied contains a virulent poison which does not exist normally, but accumulates directly upon the operation. The final proof of this poison, its separation, was impracticable, all attempts to isolate it failed, owing to its great volatility or destructibility.

Dr. Lautenbach next attempted to find what other poisons had their action destroyed or modified by the liver. Having noticed that it was almost impossible to kill an animal with nicotine administered by the mouth, he first tested this agent, by so injecting it into the veins that it would first have to pass through the liver before entering the general circulation, after having first determined the lethal amount when directly introduced into the blood without this precaution, and also the amount sufficient to produce death rapidly in animals after ligation of the portal vein. The results of these experiments seemed to show very plainly that the

poison was very materially modified and even destroyed by the liver. To see whether the contact with the liver substance destroyed the poison, he macerated a quantity of nicotine with the livers of freshly-killed animals, and then injected the expressed juices into dogs and liverless frogs, with the effect of producing some of the symptoms of nicotine poisoning, which, however, soon passed off and the animals recovered. Then, after ascertaining that the expressed juice of the kidney is not poisonous, he macerated fresh kidney tissue with quite a small quantity of nicotine, and succeeded in killing the animals into which it was injected.

From the results of all his experiments with this agent, Dr. Lautenbach felt justified in believing that nicotine contains two poisons, one of which, producing the non-fatal ataxic symptoms, is not affected by the liver, while the other, causing the fatal tetanic symptoms, is completely destroyed by it.

Similar experiments were made upon other poisons to find which of them were also neutralized through the action of the liver. Such were found to be hyoscyamia and conia, and the venom of the cobra snake, which was obtained for experimentation from Dr. S. Weir Mitchell. On the other hand, atropia, curare, and hydrocyanic acid were found not to have their action modified by this organ. This point of difference between atropia and hyoscyamia is of interest, as they are much alike in some other respects.

From the whole of his experiments the author is led to the following conclusions:

1. The liver has for one of its functions the office of destroying certain of the organic poisons.

2. A poison is being constantly formed in the system of every animal which it is the office of the liver to destroy.

The suggestiveness and importance of these investigations will be evident to every one, and it is to be hoped that they may be still further followed up. They suggest new explanations of many hitherto obscure phenomena, and if the results are verified by future observers, will entitle the author to a rather high rank among physiological discoverers.

EFFECTS ON THE CIRCULATION OF EXCITATION OF THE SPECIAL SENSES. —At the session of the Soc. de Biologie, Apr. 28, (rep. in *Gaz. des Hopitaux*) M. Couty offered in his own name and that of M. Charpentier, the following communication:

Operating on curarized dogs, and using the kymograph, and more rarely the cardiograph also, they excited the ear by various noises, cries of some other animal, metallic sounds, whistling, etc.; the eye by diffuse light, the light of a lamp, the sight of another animal, mechanical irritation of the retina, etc.; the taste by aloes, colocynth, salt, etc.; the nose by sulphohydric acid, various essences, etc. All these methods produced vasculo-cardiac disturbance varying with the irritation, and for the same irritation varying with the animal, and for the same animal varying with the

moment of the experiment. The heart was sometimes slowed, sometimes quickened; the vascular tension sometimes augmented, sometimes slightly reduced; and in the case of the purely emotional excitation caused by the cries of another animal these troubles were comparable as regards intensity, if not as regards their nature, to those produced by energetic excitation of the sciatic.

The author stated that they would endeavor in a future communication to determine the mechanism of these disorders and their relations with those caused by excitations of other sensory organs.

THE PHYSIOLOGY OF SLEEP.—The following are the conclusions of a communication by M. Villemin, read before the Acad. de Medicine, Paris, April 3d, and as reported in the *Bull. Gén. de Thérapeutique,* Apr. 30.

Physiological sleep is a law of cerebral nervous activity, the law of functional periodicity.

This law has its *raison d'être* in the necessity for repair of the organic elements, which are incessantly used by the functional activity, and the sufficient repair of which cannot be accomplished during the continuity of action of the organ.

The physical conditions in which the encephalic cells are after a period of activity, modify the vaso-motor innervation; the cerebral cells contract; the afflux of blood is diminished, the functional activity of the brain is suspended, sleep ensues and the repair of the nervous elements begins.

For the awakening, the incitation carried to the brain any one of the sensory nerves, or the mere fact that repair has been accomplished, causes the nerve cells to retake their functions; this involves a vascular dilatation, and the functional activity of the brain recommences.

In the sleep produced by anæsthetics, the defect of excitability of the brain is the cause of the phenomenon; it is not due to fatigue of the cells, but results from a special physico-chemical action of the toxic agent on the elements; by a reflex action the vessels contract themselves and enough blood does not reach the brain to call it into action.

Magnetic sleep does not depend upon any physico-chemical action of the encephalic cell, but is due to a reflex action exercising itself from the eye on the nervous vaso-motor centres. The vaso-motor spasm may be general, and then complete hypnotism appears, either with or without cataleptic accidents, (complete abolition of the sensibility when the sensitive centres are deprived of blood, circumscribed paralysis when the ischemia is partial).

ELECTRIC CURRENTS OF THE BRAIN.—Dr. Richard Caton, of Liverpool, England, publishes in the *British Medical Journal,* May 5, an "*Interim* report on the investigation of the electric currents of the brain." As this is a summary of the results of his investigations, we reproduce it here, as follows:

This research has consisted in the examination of the electric currents of the brains of upwards of forty rabbits, cats and monkeys; the rabbit having been principally employed. The instruments used have been Sir William Thomson's reflecting galvanometer, etc., with Du Bois Reymond's non-polarizable electrodes. Small, light electrodes, supported by small screw clamps, fixed firmly to the skull, in such a manner that no movement of the animal's body could affect the position of the electrodes on the brain.

The results hitherto obtained may be summed up as follows:

I. *Facts observed relating to the electric currents themselves.*

a. All the brains examined have shown evidence of the existence of electric currents.

b. If one electrode be applied to the external surface of the brain, and the other to the vertical section, a strong current passes through the multiplier, the external surface being usually positive to the vertical section.

c. If both electrodes be applied to the external surface, or one to the external surface and one to the surface of the skull, a feebler current passes through the multiplier.

d. The strength or the current varies at different points.

e. The current is usually in constant fluctuation; the oscillation of the index being generally small, about twenty to fifty degrees of the scale. At other times great fluctuations are observed, which in some instances coincide with some muscular movements or change in the animal's mental condition.

II. *Observations on the relations of the electric currents to the function of the brain.*

a. I was led to suppose it probable that some such relation existed, from the fact that fluctuations of the electric current often occurred coincidentally with some movement of the animal's body, or change in its mental condition; *e. g.,* a variation of the current frequently occurred when the rabbit awoke from sleep, or when anæsthesia was produced, or when death was occurring. The current usually fell to near zero after death.

b. An examination was made of the currents of special areas. For example, the area pointed out by Professor Ferrier as related to rotation of the head, was studied in six rabbits, with a view to discover if any change in the current occurred when the animal turned the head. In two rabbits out of the six, variation of the current was observed whenever the head was turned toward the opposite side.

c. The masticatory area was next experimented upon in eleven rabbits and two monkeys; and in four of the former, and one of the latter, marked variation was seen whenever the animal masticated. The remaining experiments were without result, either because the animals refused to eat, or from other causes to be spoken of below.

d. In two rabbits, a point was discovered close to the masticatory area, the current of which always showed variation when food was presented to the animal, but before mastication commenced. This area was thought to be probably related to the perception of the odor of food.

e. A number of experiments were made, to ascertain if the electric cur-

rents of any part of the hemisphere were related to common sensation in the skin. The skin was stimulated in different parts of the body by means of a gentle interrupted current. Nothing resulted, excepting that some evidence was obtained indicating that the currents in and about the masticatory area were influenced by stimulation of the lips and cheeks. Gentle pinching of the lips and cheeks was also seen to be invariably followed by fall in the current of the same part of the brain. This was observed in six rabbits.

f. Search was made to discover an area related to perceptions of sound. The electrodes were placed on various parts of the brain, and loud sounds were made close to the rabbit's ears by means of a bell, etc. No results were obtained.

g. A similar search was made to discover an area related to impressions on the retina. A point was found on the posterior and lateral part of the hemisphere in which, in three rabbits out of seven experimented on, variation of the current was seen to occur whenever a bright light was thrown upon the retina.

The fact of so large a proportion of the experiments (more than half) being failures may be accounted for by the great difficulty encountered. Swelling and congestion of the exposed brain occurs sooner or later, and is accompanied by great disturbance of the electrical currents. If it occur early, no observations of any value can be made. Hemorrhage, the development of currents in the electrodes, and other causes, render a large number of the experiments unproductive. The investigation thus far tends to indicate that the electrical currents of the gray matter have a relation to its function similar to that known to exist in peripheral nerves, and that the study of these currents may prove a means of throwing further light on the functions of the hemispheres.

Considering the comparatively small number of experiments yet made, and also the obscurity which involves the whole subject of the electrical currents of nerves, great caution is needful in drawing inferences from the facts above stated, and any such inferences must be considered merely provisional until many more observations have been made.

THE DEPRESSOR AND ACCELERATOR CENTRES IN THE CORD.—W. Weliki and W. Istranin. Preliminary communication in the *St. Petersburger Med. Wochenschr.*

Vulpian has shown that irritation of the cord by the electrical current at the level of the second cervical vertebra, has dilatation of the vessels of the retina and the intestines for a result, and produces contraction of the spleen. The experiment was performed on a curarized dog. This phenomena is similar to that we observe after irritation of the central end of the depressor nerve. We have repeated this experiment with the modification that the pressure in the carotid artery was registered by means of the kymographion. After curarization of the animal experimented upon, and opening the spinal cavity at the second cervical, the cord was irri

tated by a weak induction current (scarcely perceptible to the tongue) at about this level, the result was increased pressure in the carotid and slowing of the pulse-beat. With irritation at the level of the epistropheus, we observed sometimes an increase and sometimes a diminution of the pressure, but the variations were extremely slight. By carrying the irritation more peripherally, as far as to the point from which the nerve fibres forming the third pair are gathered together, we had a decided lowering of the pressure and a marked acceleration of the heart-beats. These facts should find their explanation in the supposition that with the irritation of the depressor centres diffusion currents also excite the accelerator centres, which probably lie in their immediate neighborhood.

The following are the most striking curves of variation of the blood pressure. Using a, to denote the spot over the second cervical, b', that at the level of the first fibres of the third pair of nerves, $b,''$ the one two millimetres lower down, and b''', that still one millimetre lower, we have

Duration of Experi'nt.	Locality Irritated.	Pressure before Irritation.	Pressure during Irritation.	Pulse for 10" before Irritation.	Pulse for 10" during Irritation.	Absolute Variation of Pressure.
10'	b'	118	112	8	9	− 6
—	b"	124	100	8	11	− 24
—	b"	122	76	9	15	− 46
—	a'	108	150	9	7	+ 42

We have often noticed acceleration of the heart-stroke before the occurrence of any noticeable decrease of pressure—these facts suggest that the accelerator centre may be situated somewhat higher than the depressor; that is a conjecture. The electrodes must be inserted deeply into the substance of the cord, as far as to the anterior columns. The acceleration after the irritation lasts fifteen seconds, while the pressure variation immediately disappears.

After this communication had already gone to press, we received the lastnumber of Brown-Sequard's Archives, containing a memoir by Claude Bernard on the accelerator centre in the cord.

Claude Bernard has demonstrated this centre in an entirely different way from ours—still, this circumstance renders our results more worthy of credence—as regards our results in relation to the depressor centre, they belong to us alone.

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The following are the titles of some recent articles on the Anatomy and Physiology of the Nervous System:

Lussana and Lemoigne, On the cerebral motor centres, *Lo Sperimentale,* April and May, 1877; Vance, The Encephalic circulation, *N. Y. Med. Record,* May, 12; Biffi, Organism and Mentality, *Archivio Italiano,* Jan. 1877; Langlet, Cerebral Localizations, *Rev. Med. du Nord-Est,* March, 31.

b.—PATHOLOGY OF THE NERVOUS SYSTEM AND MIND AND PATHOLOGICAL ANATOMY.

The Diagnostic Value of Conjugate Deviation of the Eyes and of Abnormal Positions of the Body in Cerebral Disease.

Dr. Martin Bernhardt, *Virchow's Archives* LXIX. I., discusses at length the diagnostic importance of symptoms to which attention was called in 1868 by, Prevost of Geneva, namely, the conjugate rotation of the eyes and head, and the decubitus in lesions of the brain. That author had laid down the proposition that, in all cases this deviation of the eyes and head is toward the sound side of the brain, and the paralyzed side of the body, both when the lesion is cortical or meningeal, when it involves the centrum semi-ovale (without implication of cerebral ganglia,) in hemorrhages into the lateral ventricle, and those in which only the thalamus and corpus striatum with the peduncle of the same side. Dr. Bernhardt reviews the literature for the cases contradictory to this general rule, and finds several observations by Eichhorst and Curschmann, and himself that are not in accordance; and he also discusses at length the experimental results of Hertwig, Magendie, Schiff, Longet, Adamuk, Ferrier and others who had observed similar phenomena from artificial cerebral lesions in the lower animals. The conclusion he reaches may, perhaps, best be stated in the following from the closing paragraphs of his paper. M. Bernhardt says: "As far as the results of my consideration reach, I have found, out of the whole symptom-complex of the position of the head, the lateral posture, and the ocular deviation, only the last, and this to a very limited degree, of any value for the local diagnosis. It is not the conjugate deviation to the right or left in the horizontal plane, but the occasionally observed vertical deviation that is diagnostic. If one eye is turned downward and the other upward (Magendie's position), then, according to clinical observations and the experimental results of the majority of investigators, there is undoubtedly a lesion of those parts of the cerebellum next adjoining the crus. This position of the eyes has never been observed with lesions of the cerebrum, and it is also undescribed as occurring with lesions of the pons."

Thus this phenomenon, the ocular position early described by Magendie, is the only one of positive signification. The author's results do not, however, invalidate the fact that, as a rule, the interpretation of the horizontal ocular deviation is according to the statement of Prevost.

Tumors of the Brain.—We take from the close of a lengthy continued article by Dr. Petrina, of Prague, on the localization of cerebral tumors, published in the *Vierteljahrschr. fuer die prakt. Heilkunde.* CXXXIII., I.

and II., Bd., following a lengthy analysis of a large number of cases, the following *resume* of the principal diagnostic points to be looked for, as indicating tumors indifferent portions of the brain.

I. Tumors of the Convexity.

Contra-lateral, direct, clonic cramps, limited to only single groups of muscles or single extremities: consciousness usually retained, since only when the tumor is deeply situated and is of uncommon size, do we observe loss of consciousness; hemiplegia never complete, lasting headache, notable vertigo, nervous irritability, and *circumscribed* disturbances of sensibility, amblyopia and alterations of hearing in consequence of intracranial pressure complete the picture. Galvanic reaction of convulsibility.

The central localization of the circumscribed motor disorders of innervation is at present to be only ventured in a general way, and is to be limited to the region of the anterior and posterior central convolutions, and for the symptom of aphasia to the left insular convolution.

II. Tumors of the Anterior Lobes.

Generally frontal headache, the *intellectual sphere implicated*, often *psychic disorder*, with or without combination with partial chorea. Paresis or hemiplegia (the latter less frequently), absence of all disturbance of sensibility; general convulsions with loss of consciousness (rare) only from decided pressure from the large tumor. Frequently decided disturbances of visual power, olfaction, and hearing, are produced by intra-cranial pressure.

III. Tumors of the Parietal Lobes.

Contra-lateral hemiplegia, frequently occurring suddenly with apoplectiform attacks. Aphasia very frequent, accompanied with serious compression or destruction of the left insular convolutions. General convulsions when the tumor is large and deep seated compressing the ganglia. Disorders of special sense—except those of sight—rare. Disorders of sensibility, especially of the skin, frequent. Headache (frontal region).

IV. Tumors of the Occipital Lobes.

Of the valid characteristic signs of tumor in this region, only one case XV. afforded the contra-lateral paralysis and the partial oculo-motor paralysis of the same side: on the other hand the remaining pathognomonic symptoms are altogether lacking, the disorders of the intelligence and the psychic activity; the convulsive attacks and all disturbances of sensibility, likewise the implications of the organs of special sense which is mentioned by Ladame, and also Rosenthal and others.

V. Tumors of the Motor Ganglia.

Nucleus lenticularis.—Contra-lateral hemiplegia with loss of consciousness and frequently convulsions. High degree of cutaneous anaesthesia when there is simultaneous destruction or implication of the internal capsula; not infrequently aphasia.

Corpus Striatum. Complete hemiplegia; loss of consciousness and convulsions, disturbances of psychic functions and intelligence, irritative motor phenomena, such as tremor and choreiform symptoms. Disorders of the organs of special sense apart from amblyopia, rare.

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VI. *Tumors of the Optic Thalamus.*

With tumors affecting alone the optic thalamus, extensive motor lesions are altogether lacking, and general convulsions and disorders of sensibility are not constant. According as the tumor affects more the bundles of fibres going to the optic tracts, or those branching out from the cerebral peduncle, we have sometimes predominating paralytic phenomena in the optic nerve, alterations of the pupil, and disturbances of the innervation of the ocular muscles (nystagmus, exophthalmus), sometimes, again, the most remarkable vaso-motor circulation anomalies (striking alterations of temperature, cyanosis, circumscribed redness) as the chief morbid symptoms. Pronounced disorders of speech (retarded speech) and of the intelligence are symptomatic only of quite extensive tumors in the thalamus; decided paralytic phenomena are likewise characteristic of simultaneous destruction of the peduncular fibres, or of one of the motor ganglia.

VII. *Tumors of the Hypophysis.*

Marked somnolence, decided mental weakness and apathy. Remarkable slowness of speech. Amblyopia and amaurosis, and frequently disorders of other organs of sense. Oculo-motor paralysis and cephalalgia. According to Rosenthal's observation, diabetes mellitus likewise occurs as an important complication in cases of tumor in this region.

VIII. *Tumors of the Cerebral Peduncles.*

Serious vaso-motor disorders and temperature anomalies, early oculo-motor paralysis on the same side as the tumor, occasional paralysis of the bladder, contra-lateral paresis and disorder of sensibility; no decided disorder of intelligence. Frequently, disease of the organs of special sense, especially of the optic nerve. Intimations of involuntary movements and position anomalies on the side opposite the tumor.

IX. *Tumors of the Crus Cerebelli.*

Unilateral position of the body, involuntary lateral decubitus, rotation on the axis of the body, unilateral deviation of the orbits, reeling gait, with tendency to fall to one side, frequency of disturbances of special senses; vertigo and headache.

X. *Tumors of the Cerebellum.*

Severe headache, usually in the occipital region, motor irritative phenomena, reeling gait, very severe vertigo, more or less pronounced disorders of co-ordination contra-lateral paresis of the body. Frequently disorder of special sense, convergent strabismus. The same electric reaction as in cases of basal tumor. (Diminished reaction on the sound side of the body.)

XI. *Tumors of the Pons Varolii.*

Alternate hemiplegia, paralysis in the sphere of the ocular muscles (convergent strabismus): paresis of the muscles of the tongue, anæsthesia, dysphagia, disorders of special sense, especially of the visual power; implication of the trigeminus, frequently crossed sensory disorder of the trunk and one-half of the face. Very commonly, vertiginous sensations and marked vaso-motor disturbances. Generally partial convulsions are lacking. Interesting and characteristic electrical reaction in the region

of the paralyzed facialis, extinguishment of the electro-muscular contractility to the induction current, with increased electro-muscular contractility to the galvanic current, and at the same time diminished galvanic irritability of the facial branches.

THE CONDITION OF THE NERVE CENTRES IN MIGRAINE, EPILEPSY, ETC.—Dr. Sidney Ringer, *Lancet*, May, publishes his views as to the condition of the nervous centres "in migraine, epilepsy and other explosive neuroses." It is, in brief, that in these there is a lack of resistant power, thus permitting impressions conveyed to certain parts to extend beyond their normal area, thus producing the abnormal symptoms of these diseases. For example, he thus explains an attack of migraine, the common form in which the symptoms are supra-orbital headache, with nausea and vomiting. He says: "In a paroxysm with these symptoms, there occurs an evolution of nervous force, first in that part of the nucleus of the fifth nerve in connection with the supra-orbital nerve; the discharge then travels backwards and involves the centre for vomiting. Ascending to the view now advanced, there is diminished resistance in the nervous structures between these parts, so that a stimulus causing a discharge of force by the fifth nerve can pass backwards and involve the centre for vomiting. It may be urged that, according to this theory, any discharge of force in the nucleus of the fifth nerve should travel to the centre for vomiting, and produce nausea or vomiting; but this is not the case, for if the loss of resistance is slight, only a strong discharge in the fifth nucleus can overstep its usual limits and reach the centre for vomiting. Now, the strength of the discharge depends on the amount of nourishment (potential force) of the nucleus of the fifth, or on the strength of the stimulus converting the potential into kinetic force, chiefly on the amount of potential force. In a paroxysm of migraine, the potential force accumulated by nutrition being discharged, the parts become weakened or depressed, and time is required for nutrition to supply the last discharged force; and till by nutrition the potential force has accumulated to its original amount, each stimulus conducted to the fifth is limited to the part of the nucleus naturally associated with it; but when the potential force has accumulated to a sufficient extent, then the discharge overcomes the weakened resistance and reaches the centre of vomiting."

Dr. Ringer sums up his case as follows: "In the explosive neurosis there is diminution of resistive power, the nature of the symptoms depending on the part and extent of the nervous system affected; the degree of loss of resistance, varying much in individual cases. When the potential force in the part with diminished resistance is weak, the remaining resistance is sufficient to restrain the evolution of force within its normal limits; hence, we get co-ordinated normal action. But when sufficient time has elapsed, the accumulation of potential force through nutrition becomes so great that when set free by a stimulus it cannot be restrained by a weakened resistance, but overstepping its normal boundaries, it involves neighboring parts, arousing in them irregular unco-ordinated action; in fact, the symptoms of the neurosal attack."

SPINAL ATROPHY AFTER RESECTION OF NERVE ROOTS.—Bufalini and Rossi, *Archiv. Italieno*, November, 1876, publish a series of experiments to ascertain the effect in producing atrophy of the cord of section of the spinal roots, the results of which are summed up in the following conclusions:

1. That there is no alteration histologically of the gray matter of the cord from section of the spinal roots, and that there is instead a partial atrophy of the white substance, consisting solely in diminution of the number of the elements of which it is constituted.

2. That the atrophy of the cord due to section of the sciatic roots does not extend beyond the lumbar enlargements.

3. That the atrophy of the cord from section of the spinal nerve roots is most manifest in its posterior and lateral portions; this depending on the interruption of the relations between the spinal ganglia and the sensory fibres contained in the posterior part of the spinal cord.

THEORY OF DIABETIC GLYCEMIA.—At the sitting of the Acad. de Médecine, April 10 (report in the *Bull. Gén. de Thérapeutique*), M. Fleury read a paper entitled "The Dynamo-chemical Theory of Diabetic Glycemia." "*En résume*," said he, in closing, "the theory, or rather the doctrine that I have the honor of submitting, reduces itself to the following":

"The pathogeny of diabetes mellitus includes two kind of causes, the physiological and the chemical.

"The physiological cause, whatever may be the variety of the lesion, resides in a functional trouble of the general innervation. This shows itself in a vaso-motor paresis, causing a too free flow of the blood in the capillaries. In consequence of this vascular relaxation, the conflict between the oxygen and the carbon does not take place, or occurs only imperfectly; the oxidation of the globules is hindered. The not utilized oxygen, by default of polarization of its ozone, forms, together with the water of the blood, hydrogen peroxide (HO^2.). This oxygenated water, instead of being constantly destroyed, as would be the case normally when the walls of the capillaries still retained their contractile energy and tonicity, becomes fixed in the blood, and communicates to it the catalytic property attributed to ferments.

"Every organic or functional lesion of the pneumogastric, causing a hypersthenia of that nervous apparatus, and secondarily a hyposthenia of the sympathetic and its vaso-motor filaments, every vascular hyposthenia due to a direct lesion of the sympathetic nerve fibres, is likely to obstruct the intra-organic combustions, the oxidations of the globules in the capillaries, to prevent the formation of carbonic acid, and to substitute in the veins for blood purely venous and brown, a mixture of arterial and venous blood containing peroxide of hydrogen

"The chemical cause of the formation of glucose in the diabetic patient, in so far as it has to do with blood formed at the expense of the starchy and amylaceus matters of the first digestion, is the setting free by decomposition of a sulphur principal. This is formed in the saliva by the

sulphocyanide of potassium, and in the bile by the taurocholate of soda.

"In the normal condition the separation of sulphocyanide of potassium and of taurocholate of soda gives rise to only a moderate production of sugar. But the saliva, like the bile of a diabetic, secreted at the expense of a blood containing oxygenated water, gives to this peroxide of hydrogen a power of catalytic conversion, which is readily explained if we consider that at a temperature, not above that of the stomach, and in contact with starchy substances, every sulphurous principle, only that it is actually acid, converts starch into sugar and dextrine.

"As regards the sugar generated by diabetics, at the expense of nitrogenized substances, albuminoids, the mere fact of the diminution of the combustions and oxidations within the organism in consequence of a non-utilization of a part of the oxygen and of the carbon may suffice to furnish an explanation.

"The proof that the bile supplies elements of the first importance for the manufacture of sugar in the intestine, is the fact that we cannot tie the ductus choledochus without immediately suppressing the storing up of glycogen in the liver.

"The evidence that this element is the principle sulphur, and that this sulphur, set free in the presence of starch, acidified by the oxygenated water in the blood of diabetics, acts without cessation, is that we see everywhere the sulphur required by the organism of the diabetic for this catalytic work.

A New Paralytic Affection.—Dr. W. Macgregor, *Glasgow Medical Journal*, June, 1877, describes a peculiar form of paralysis of the extensors occurring in his practice in the Mauritius. The subjects were all Chinamen, and in all the fatal cases the direct cause of death seemed to be œdema of the lungs, nothing abnormal could be detected in the muscular or nervous systems, beyond incipient fatty degeneration of the muscular fibre. In each case, however, the bile ducts contained large numbers of a parasite, the *Distoma sinense* of Cobbold (*Lancet*, August 21, 1875).

As to the connection between the parasite and the disease, it is by no means clear, but it is difficult to recognize any other cause. The author suggests a poisonous influence; if this be not the fact he thinks the paralysis must be regarded as reflex.

Epilepsy.—O. Moschutkowsky, *Papers of the Physicians of the Odessa City Hospital*, 1876, (Russian), (abst. in *St. Petersburgh Med. Wochenschr*, No. 40, 1876). In order to test the value of the present method of treatment in epilepsy, the author attempted to make full observations of the epileptics received in the Odessa city hospital during the year 1872. The task was one of unusual difficulty, as these cases seldom remained long in the institution, and as soon as they had recovered from the effects of an attack, in which they were picked up in the streets, they generally wanted to leave. Among the forty-six patients received, (32 males, 14 females)

almost one-half remained only about fourteen days. The majority of the patients were young (14 between 20 and 25 years), and the first onset of the disease was predominantly during the summer months. Prodromata were noticed in fourteen cases, but were usually of very short duration, and a pronounced aura was present in but a single case. As primary cause, wounds were most frequently stated, syphilis (9 times), degeneration of the brain, and typhus diseases. Almost all were well built and nourished. The presence of albumen in the urine immediately after the attack was undetected in sixteen cases. The treatment was carried on with a whole series of agents, chief among which were bromide of potassium and atropia. Bromide of potassium was employed by the author in twenty-six cases in very varying doses. Small doses (5 to 15 gr.) had after one or two months an effect on the heart, quickening the pulse; a tranquilizing of the nervous system was nevertheless rather rapidly brought about; exanthemata were very rare. Large doses (in males 20 gr. several times a day to four drachms daily, and in females 15 grs. to amount to three drachms a day), produced slowing of the pulse within a few days, and noticeable sedation of the nervous system at once, later they caused a nervous excitation and diminution of the body weight; exanthemata were almost constantly present. No effect on the secretions and temperature were noted. Altogether, the author observed three stages in the action of the bromide,, in the beginning a sedative action, then excitation and paralytic symptoms, and the last is to be considered as a bromide cachexia, consisting in a feeling of weakness, unsteady progression, anæsthesia, tremor of the members, and arhythmia of the pulse. In the sedative stage the bromide causes a decided decrease in the attacks. Regarding the parts taken by the several constituents, the author ascribes the aplastic action to the potash, aad the diminution of the reflex excitability to the bromine.

Atropia formerly was employed on Brown-Sequard's recommendation with good results, and has of late fallen into undeserved neglect. Trousseau employed it where he saw during the attack a decided narrowing of the pupil. The author has seen a notable improvement as regards the frequency and intensity of the attacks from its use in nine cases, and gives the histories of seven of these. Iodide of potash (8 cases) acted similarly to the bromide. Of the other agents employed, the author saw no effect from nitrate of silver, valerianate of zinc, bromide of camphor, galvanization of the cervical sympathetic, and chloral; amyl nitrite, given in two cases, each time called forth the attack.

The author recommends the use of atropia in recent cases of epilepsy with frequent attacks, but in those of longer standing he advises the employment of bromide of potassium, (differing here from Nothnagel, who attributes no special value to it in one series of cases rather than the other). Neither is an absolute specific, but still both are of decided value. The albuminous state of the urine in epileptics has not the importance in a forensic relation that has been given it.

DISORDERS OF TASTE AND TACT, AND OF THE SALIVANY SECRETION FROM AFFECTIONS OF THE TYMPANIC CAVITY.—The following is an abstract of a recent memoir by V. Urbantschitsch, (Stuttgart, 1876), by Lewin, in the *Deutsche Med. Wochenschift*, Feb. 24:

The author investigated the gustatory surfaces by means of concentrated solutions of salt, sugar, tartaric acid, and quinine. He found, like others, that the intensity of the gustatory sensation was greatest at the margin of the tongue, in the palato-glossal arch, and the soft palate, but that this sense also existed over the middle of the tongue, in the posterior wall of the pharynx, on the under surface of the tongue to both sides of the frænum of the uvula, in the hard palate, and on the mucous membrane lining the cheeks.

Nevertheless, the gustatory surface is not always in condition to feel its special sensation. An alienation of the sense of taste may occur so as to cause a substance to be appreciated first as another, and then later as the correct one, and indeed there are cases in which, in normal individuals, instead of the gustatory sensation, an analogous or altogether foreign smell is experienced; for example, after pencilling the posterior surface of the pharynx or the point of the tongue with quinine, there is experienced an odor of oil of bitter almonds.

The author then gives a review of the results of investigations of the gustatory sense in fifty individuals affected with purulent inflammation of the tympanic cavity, forty-six of whom gave evidence of an abnormal perception of taste impressions. In thirty-eight of these cases there was a heightened sense of taste, and in five there was a partial diminution and a partial increase of this special sense on the side of the mouth corresponding to the diseased ear.

Finally, the author has observed cases in which the intensity of the taste sensation sometimes varied according to the kind. Thus in one patient, with a purulent catarrh of the right middle ear, there was diminution of the gustatory sense for salt and bitter, and, on the other hand, an increased perception of sweet and sour tastes. This is well enough explained, if one supposes, as Young has done for the perception of the colors, red, green, and violet, that different nerve fibres serve for the perception of sweet, salt, sour, and bitter substances.

A complete loss of taste was not met with in any case.

Besides the gustatory fibres, in some cases of purulent ear affection, there are also tact nerves affected, so that this sense is also modified in the same parts. This condition may be due to pressure on the chorda tympani, and on the tympanic plexus, by irritation and injury of these nerves.

An irritation also produces anomalies of the salivary secretion, since through these nerves run the greater part of the secretory fibres of the salivary glands, *i. e.*, the vaso-dilator nerves of Cl. Bernard.

The following are the titles of other recently published articles on the Pathology of the Nervous System, and Mind and Pathological Anatomy:

BURRESI, Tuberculosis of the Excitable Zone of the right cerebral Hemisphere. *Lo Sperimentale*, March, 1877; MORSELLI, Pathogeny of Epilepsy, Epileptiform attacks consecutive to Traumatic Lesions of the anterior cerebral Convolutions, *Ibid*; MORSELLI, Physio- Pathological Illustration of four cases of Disease of the Medulla oblongata and upper portion of the Spinal Cord, *Ibid*, May and Jan., 1877; MORSELLI, Contribution to the Psychology of Crime, Statistical and Anthropological Data of Criminal Suicides, *Archivio Italieno*, March and May, 1877; LIVI, Etiology of Progressive Paresis, *Revista Sperimentale*, Jan. and March, 1877; TAMBURINI, New Observations of Osteoma of the Spinal Arachnoid in Progressive Paresis, *Ibid*; HUGHLINGS JACKSON, Nervous Symptoms with Ear Disease, *Lancet* (*Am. Repr.*), June; GERHARD, Tremor as a Symptom of Nervous Disease, *Med. and Surg. Rep.*, June 23; GALLI, Diphtheritic Paralysis, *Rivista Clinica*, March, 1877.

c.—THERAPEUTICS OF THE NERVOUS SYSTEM AND MIND.

NITRITE OF AMYL.—The following are the classes of disorders in which nitrite of amyl is indicated, according to a recent memoir by Dr. Van Ermingan, analyzed by Dr. Carpenter-Mericourt *fils.* in the *Bull. Gen. de Therapeutique.*

I. Syncopal accidents, comatose, and characterized by feebleness or cardiac weakness with anæmia or venous congestion of the cerebrospinal centres.

II. Diseases characterized by spasm of the vessels.

III. Diseases characterized by spasm of the voluntary or involuntary muscles.

IV. Diseases characterized by extreme elevation of temperature.

Three drops of amyl nitrite inhaled from a handkerchief overcome the threatening syncope of chloroform, and two drops have sufficed for a cure, but it is especially in angina pectoris and asthma that the best results have been obtained. Its employment is contra-indicated in aged persons or those who present indications of vascular alterations, or of heart disease, and it is also contra-indicated in plethoric females recently delivered.

It should be used in all cases with great care, as some serious inconveniences, to say the least, have been experienced by some persons from very small doses. Others again can inhale thirty or forty drops without suffering any serious discomfort.

The following are conclusions drawn from experiments with this agent, by W. D. Lane, and published in the *British Medical Journal*, Jan. 27:

1. Amyl nitrite when inhaled in small quantities, produces reddening of the face in man, and of the nose and mouth in kittens; this action is due, according to Brunton, to the dilatations and over-filling of the arterioles.

2. When inhaled by kittens in large quantities, it produces cyanosis of the nose and mouth along with insensibility. The cyanosis arises from over-distention of the venous system, this being due to the engorged arterioles propelling the blood into the veins, while the insensibility is probably caused by over-distention of the venous system and the heart.

3. When inhaled in small quantities, it produces recovery from chloroformic insensibility, by dilating the arterioles of the brain and thus removing the cerebral anæmia due to chloroform.

4. When inhaled in large quantities, instead of producing recovery from chloroformic insensibility, it not only retards it but it may cause death by paralysis and over-distention of the heart, and engorgement of the venous system.

5. It causes a rise of temperature when inhaled in large quantities by the increased amount of blood in the arterioles, causing an increased tissue-change in the body.

6. In large doses (inhaled) it produces a fall of temperature.

7. It also helps to produce recovery from the chloroformic insensibility by raising the temperature which is always lowered by chloroform, and by removing the paralysis of the heart due to chloroform; this action is well seen by the nitrite of amyl making the hearts beats fewer and its sounds louder.

8. Death is caused chiefly by paralysis of the heart, which is shown by all its cavities being distended, and by engorgement of the venous system.

BROMIDE OF LITHIUM.—Bromide of lithium, introduced into therapeutics some years since is doubly valuable since it acts with equal energy as a sedative and a lithotriptic. In fact, its richness in bromine, 91.95 per cent. is very much superior to that of the other bromides, and its 8.05 per cent. of lithium is sufficient to neutralize a considerable quantity of uric acid, as one part of lithium neutralizes four parts of uric acid.

Thanks to this double action, demonstrated both in the Paris hospitals and in civil practice, the bromide of lithium is vaguely prescribed for the neuroses on the one hand, and on the other for the various manifestations of the uric diathesis. Experience, indeed, has demonstrated its powerful

effects in epilepsy, chorea, insomnia, hypochondria, and in the diverse forms of the uric diathesis, such as gravel, nephritic colic, gout and diabetis.

In these last affections which are accompanied by the symptom pain, as in nephritic colic and gout, bromide of lithium besides its lithotriptic action, acts also as a sedative and calms in a few hours the sufferings of the patients.

In this special relation the bromide of lithium is a most valuable remedy in therapeutics and cannot be too much recommended to physicians.—*Presse Méd. Belge* (*La France Médicale*).

The Action of Hypnotics.—C. Binz, *Arch. f. exper. Path. u. Pharm.* VI., 5 and 6, Hft., 1877. (Abstr. in *Deutsche Med. Wochenschr.*) The author instituted a series of michrochemical researches on the action of certain hypnotic substances on the gray matter of the brain, with special relation to Preyer's theory that " the brain becomes inactive because the accumulated products of decomposition seize upon the oxygen that otherwise is metamorphosed in the brain." It followed from these experiments that morphia, for example, possesses a very characteristic and constant chemical acton on the brain, in so far that it renders the protoplasm of the sharp contoured cells muddy, and darkens the intermediate substance. Similarly constituted bsdies, such as atropia, do not share this property. He lays down the following hypothesis : " Morphia, chloral, ether, and chloroform possess a strong affinity for the cortical substance of the brain in man. It hoids for a time the hypnotics brought to it by the blood, and from the resulting alteration in its processes of tissue change is rendered incapable of calling into action the functions of the waking state.

The author further ascertained by narcotizing dogs and rabbits with trephined craniums, that anæmia of the brain occurred late in complete narcosis, so that this condition is a result of the narcotization and not the reverse. Hence it follows that anæmia is not to be considered as essential to sleep.

The Statistics of Therapeutics.—MM. Lasegue and Regnauld, in the *Archives Génerales de Médecine*, Jan., 1877, have collected from all the available documents of the Paris hospitals, the following interesting statistics as to the quantities of various medicines there employed during the last twenty years. We extract only the figures relative to the quantities of certain remedies acting largely on the nervous system and its functions, and these figures include the more notable results of the research :

Of chloroform, the quantity annually used was 141 kilogrammes in 1855, which in 1875 had increased to 308 kilogrammes. Chloral, which only made its appearance in the hospitals in 1869, its consumption in that year was only five kilogrammes, but in 1875 it had risen to 360 kilogrammes.

Of bromide of potassium, the usage rose from three kilogrammes in 1855, and 22 kilogrammes in 1864 to 760 kilogrammes in 1875.

Opium held its own during the period comprised in the investigation; its annual consumption ranging from 150 to 200 kilogrammes. But the quantity of morphine used increased during the same period from 272 grammes to 10 kilogrammes, indicating the greater prevalence of the hypodermic method of administering opiates.

The use of alcoholics in all forms likewise made great advances during this period, of alcohol from 1,270 litres in 1855 to 37,587 in 1875, of brandy from four litres in 1862 to 4,108 in 1875, and of rum from 199 litres in 1855 to 5,682 at the more recent date.

Of other remedies the increase was as great as in those we have mentioned in but few, chlorate of potash, carbolic acid and cod liver oil being among the more remarkable. Some interesting general inferences may be drawn in reference to the tendencies of modern therapeutics from these figures, thus the increasing favor of anodynes and sedatives is certainly noteworthy. The results are very suggestive, and it is very desirable that similar researches be instituted over still more extensive fields and periods of time.

COUNTER IRRITATION IN NERVOUS DEBILITY AND SPINAL IRRITATION. —Dr. Arthur Gamgee, *Practitioner*, Feb., 1877, recommends the use of counter irritation of the back by means of the Linamentum Sinapis Co., (B. P.), in cases of nervous debility due to overwork, excitement of cranial and spinal centres, or disturbance of the nutritive balance, etc., including the symptoms of the condition often called neurasthenia. He claims that this is a very important and effective adjunct to the other means usually employed in these cases, such as iron, cod liver oil, phosphorus, etc. The explanation of the favorable action of this method of treatment in these cases, he thinks, is to be found in that it increases the vascular tonus of the nerve centres, a lowered condition of which, he believes, to be the underlying condition of this state of nervous debility, and its attendant phenomena of sleeplessness, etc. In conclusion, he says: "There are two points upon which I think it right to add some observations, so as to prevent my being misunderstood.

1. "The facts which are day by day being accumulated point to the existence of local vaso-motor mechanisms, which are subordinated to the general vaso-motor centre, (if such a centre really exists apart from the former), so that it is not only conceivable, but certain, that an affection of, or a change in, a local vaso-motor mechanism might be induced without a necessary affection of the general vaso-motor mechanism. A general high tonus does not, for example, imply that its possessor should have a tendency to sleep, for sleep is doubtless due to a positive cerebral anæmia, to the production of which the activity of a centre, specially connected with the cerebral circulation probably contributes.

2. "Whilst such counter-irritation as I have recommended appears to exert a tonic action on the vaso-motor centre, and on the body generally,

I am quite aware that several methods of counter-irritation produce an opposite effect. The shock and great depression which, especially in women, sometimes follow the application of blisters, seem to show that severe counter-irritation very frequently tends to depress rather than to raise the tone of the vascular system."

METALLOTHERAPY.—The following is the substance of a report of a commission appointed to examine the effects of the application of metals to anæsthetic surfaces, as given by M. Dumontpallier to the Soc. de Biologie, April 14, and abstracted in the *Progrès Médical.* The commission was composed of MM. Charcot, Luys and Dumontpallier, and they had added to their number MM. Landolt, Gellé and Regnard, for certain special researches. It had satisfied itself as to the correctness of the statements made by M. Burq. If in hemianæsthesic hysterical patients we apply to the anæsthetic skin pieces of gold, copper or zinc, the subject presently feels sensations of itching and of heat, and in a few moments we can ascertain the return of sensibility at this point, together with an increased temperature and strength. In the vicinity we also note symptoms of dysæsthesia. Special sensibility is influenced in the same way; thus the members of the commission have observed color blindness to disappear and deafness improved.

These remarkable phenomena are not produced alike in all subjects by the same metal; in some, gold alone is effective, in others there is an idiosyncracy for copper or for zinc. It is probable, moreover, that this action of the metal is due to electric currents developed in the skin. For instance, in one case the sensibility returned from the use of currents of the same strength as indicated by the galvanometer as those following the application of gold to the skin. The same phenomena occurred in organic hemianæsthesias, but curiously enough, the effects of the metallic applications were much more durable in these cases than in those of hysterical hemianæsthesia.

In the course of their experiments, the members of the commission had demonstrated one fact of very great physiological importance. According as, on the one side, the general or special sensibility reappears, the temperature increases and the muscular force is augmented, so on the homologous point on the sound side we observe a corresponding decrease of each of these. It appears as if one side lost what the other gained, that there was a veritable transfer of sensibility.

THE ANTAGONISM BETWEEN MORPHIA AND ATROPIA.—Prof. C. Binz, *Deutsche Med. Wochenschr,* Mar. 24, 1877.

Since the middle of the last decennium a series of investigators have occupied themselves with the question whether there exists a reciprocal antagonism between morphia and atropia. Bezold is the only one among

them that admits it to a limited degree. The results of the others' re-
searches are quite the reverse.

Clinical experience in the human subject is not in accord with them.
Even to the most critical reader the opposed view obtrudes itself; and, in-
deed, so far that serious cases of atropia poisoning may be treated with
the best hopes of success by the use of not too small doses of morphia and
serious morphia intoxications, with very minute doses of atropia, in both
cases hypodermically.

At the commencement of the present semester, in my lectures on experi-
mental toxicology, the necessity was met with of testing this vexed ques-
tion myself. I performed the simple experiment of poisoning a very
young dog with morphia, so far as to completely paralyze its sensorium;
its pulse, normal at 140, was reduced to 42, and was scarcely perceptible
through the thoracic walls; its respiration at 22 and quite superficial, and
the temperature in the rectum fell between three and four degrees (R.) be-
low the normal. When .0005 gramme of sulphate of atropia in half a
gramme of water was subcutaneously injected into the neck during the
collapse. Within ten minutes the animal gave signs of resuscitation. The
heart-stroke became easy to feel, and rose to 140; the respiration was 52 to
the minute and powerful, only the bodily temperature showed no increase.
The improvement in circulation and breathing was lasting, and the animal
recovered. A long needle inserted into the ventricle of the heart, and an
easily movable lever in the region of the diaphragm, both made con-
spicuous by means of pieces of white paper, enabled the audience to easily
see the experiment.

The same result was repeated in an experiment upon a somewhat older
dog, which also served as the subject of a farther series of experiments
which I undertook with the assistance of Dr. Heubach, and with the use
of the haemodynamometer and the kymographion.

The improvement in the blood tension, lowered by the morphia, is very
striking. It sometimes doubles the weight of the mercurial column, thus,
for example, it raises it from 70 to 140 millimetres. This always begins a
few minutes after the injection of the antagonist poison. Its cause is the
paresis of the cardiac inhibitory fibres of the vagus. Therefore the pulse
is frequent and the separate excursions of the ventricles low. The heart,
dilated in diastole, contains a large amount of blood, and is therefore
readily felt through the ribs. These effects were manometrically observed
for over an hour. The dog, then set free upon the ground, staggered off
to a warm corner, where recovery always took place.

Our experiments have led me to the conviction that the objection to the
use of atropia in morphia poisoning, raised by some seven experiments, is
not well founded. I have not only never seen any combination of the
effects of the two poisons in any hurtful sense, but the indirect elevation
of the blood pressure from the heart, and the direct improvement of the
respiration by excitation of the respiratory centres, are two remarkably
favorable circumstances for recovery. The subject of morphia poisoning
dies from immediate paralysis of both these functions. If they are sus-
tained for a sufficiently long time, the organism has the opportunity to

free itself of the poison through the urine, and perhaps through decompo-
sition. And both of these results are much more likely to occur if sub-
stance change in the system is in good condition, and this cannot be the
case when the blood pressure and respiration are below the normal.

Experiments on the measurable influence of morphia as an antidote in
atropia poisoning are in progress. They seem to give less pronounced
results, since dogs react unreliably to large doses of atropia. There is
also less to be said, for the reason that the counter-action of the well-
known excitation due to atropia by hypodermic administration of large
doses of morphia has been less gainsaid.

It may be questioned whether, in all this, we have an " antagonism" or
not. This word indicates a conception which may be taken in a broad or
narrow, absolute or relative sense, as best suits, and according to his own
stand-point every one will adopt or reject the term. But the fact that, un-
der certain circumstances, the one poison lessens the effects of the other,
does not depend at all upon this question.

XANTHIUM SPINOSUM.—Some months ago a Russian physician, Dr.
Grzymala, announced that the leaves of a certain plant, the Xanthium
spinosum, were a certain cure for hydrophobia, and adduced a hundred
or more cases in his own experience in which it had proved efficacious.
Of course this statement excited considerable interest, and some very
natural disbelief among veterinarians. Accordingly MM. Trasbot and
Nocard, of the Veterinary College of Alfort, France, instituted a series
of experiments to test this reputed virtue of xanthium. They inoculated
on the twenty-third day of last April, eleven dogs with the hydrophobic
saliva, from an animal then living, but suffering from the disease. Six of
these dogs were put upon a regular treatment with the remedy as advised
by Dr. Grzymala, the other five were left without medication. Thirteen
days after the inoculation, one of the dogs treated regularly with the xan-
thium presented all the symptoms of hydrophobia, and bit repeatedly a
bitch that was confined with him. He died next day, and the bitch was
immediately put upon a course of the drug, but died rabid twenty days
later.

The other dogs inoculated were very young, and all died in a month or
two with obscure nervous symptoms, too little characteristic to be stated
as certainly hydrophobic. Not regarding these cases, however, there re-
mains the fact that the two adult animals inoculated, died of well char-
acterized rabies, is alone a sufficient proof that as an antidote, xanthium
is a failure.

ELECTRICITY IN ORGANIC HEMI-ANÆSTHESIAS.—M. Magnan reported to
the Soc. de Biologie April 2, (abstr. in *Gaz. des Hopitaux*) that he had been
studying the effects of electric currents on hemi-anæsthesias of cerebral
origin. The first day, in the same patient, he had to use thirty elements

of the Trouvé battery to recall the sensibility; the next day twenty brought about the same result; the next fifteen, and so on. In these cases M. Magnan observed no transfer of sensibility like that noticed in hysterical patients.

ATROPIA IN EPILEPSY.—Dr. Suettin, *Mitth. der Vereins der Aerzte in Niederoesterreich* (abstr. by Seeligmuller in *Deutsch. Med. Wochenschr*). According to the author's experiments on guinea pigs artificially rendered epileptic, atropia in small doses acts absolutely as an inhibitor of reflex action, but in large doses it increases it. Therefore he recommends that in epilepsy in the human subject, which "depends on a reflex cramp of the vaso-motor centres" sulphate of atropia in small doses, a pill containing one-fifth of a milligramme to be taken five times a day. The author believes that the absence of the symptoms of atropia intoxication that are not unfrequently present when this quantity is given in solution, to be due to the slowness of absorption of the drug in the pill form. (Seeligmuller states that he has not infrequently observed slight intoxication symptoms follow the use of one milligramme of atropia administered in the form of a pill for the nocturnal sweats of phthisis.) This medication must be carried on for months in order to produce results.

THE EFFECTS OF THE HYPODERMIC INJECTION OF CERTAIN ALKALOIDS IN THE DOG.—At the meeting of the Soc. de Biologie, May 19, (rep. in *Gaz. des Hopitaux*) M. Laborde presented three dogs on which he had practiced subcutaneous injections in the one, of sulphate of quinine in the dose of one gramme, in the other of sulphate of cinchonine, dose seventy-five centigrammes, and in a third, sulphate of cinchonitine, the same quantity. The first, under the influence of quinine had attacks of vomiting and presented a complete loss of general sensibility.

The second, under the influence of cinchonine, offered a series of true epileptic attacks, absolutely similar to those we observe in man, the fall, initial cry, tonic convulsions, followed by clonic ones, foam at the mouth, biting of the tongue and stertor. M. Laborde observed that he had never produced so clear an epileptic attack, one so like to that in the human subject.

Finally, the third dog, under the influence of cinchonitine, after some convulsive attacks, more or less resembling the epileptiform attack, was siezed with a tremor analogous to that of paralysis agitans.

These experiments enables us to determine very clearly the different physiological effects of these alkaloids, quinine, cinchonine, and cinchonitine. They permit us, moreover, to affirm that, in a certain number of cases, where we administer to patients rather large doses of these substances, we render ourselves liable to produce in them these accidents.

NICOTINE AND STRYCHNIA.—Dr. F. L. Haynes, *Phil. Med. Times*, May 12, gives an account of the results of a series of experiments upon the action of nicotine and strychnia in relation to one another. The animals employed were rats, cats, rabbits and dogs. The minimum fatal dose of

each agent upon each species of animal was first ascertained, then their combined action was tested. The conclusions arrived at are as follows:

1. Strychnia and nicotine are in no degree antagonistic poisons.

2. Strychnia increases the convulsive action and does not diminish the motor paralysis of nicotine.

3. Nicotine (even in paralyzing doses) increases the convulsive action of strychnia.

4. Both poisons cause death by paralyzing the respiratory apparatus. They may effect respiration in different ways, but the result is the same.

5. Animals may be killed by injecting together doses of the two drugs which, singly, are not fatal.

The author adds: "There is no reason to suppose that the above deductions are not applicable to the human animal. The symptoms of poisoning by the two drugs are identical in man or the lower animals. As regards strychnia, this is too well known to need further remark. In regard to nicotine, it is only necessary to refer the reader to the recorded cases of poisoning by that drug.

"It may not be out of place to mention the fact that experimentation has proven that nicotine and strychnia show a remarkable similarity in their intimate action on the nervous system, both being excitants of the spinal cord and paralyzers of the motor and efferent nerves. For the explanations of these assertions, and for many other deeply interesting facts connected with this subject, the reader is referred to other sources."

MASSAGE.—The following is an extract from the address of Dr. S. Weir Mitchell, of Philadelphia, before the Medical and Chirurgical Faculty of Maryland in Baltimore last April. The class of cases of which he speaks must be familiar to most physicians in large practice, and his successful experience is therefore of especial value. Dr. Mitchell says:

" In our great cities there exist a host of influences for evil which result in all classes, and especially in women, in the gradual creation of patients who, having lost weight and become anæmic, find it hard to regain that competency of capital in fat and blood without which the business of life is carried on at a dangerous cost. We search in vain in these cases for organic changes which may explain their conditon. No function is well performed, but it is useless to correct digestion or treat an ulcerated womb or order exercise. The blood is lacking to aid in the little gains we win, and exercise is valueless or worse when it exhausts tissues which lack the means of being rebuilded.

" I need not dwell on points so obvious to educated physicians. For many years past I have had my thoughts directed to this subject, and, like every one here, I have gone on month after month treating such cases with no better, and I hope, no worse fortune than has fallen to others. A moment of happy thought and much reflection since, led me to a method of treatment which has rewarded me over and over with successes so brilliant that, as the plan of cure involves some of those extreme measures

of which I have been speaking, I may be pardoned for calling them to your attention.

" And perhaps also the path by which I reached my conclusions may not lack interest.

" Some years ago I saw a woman who was like half a dozen any of you can now recall, a pallid, feeble creature, who had menstruated irregularly until two years before, and then stopped at the age of thirty. She was the type of a class. Everything wearied her,—to walk, to read, to drive, to sew. She was the woman with a back, and a shawl on her shoulders, and a sofa for a home, and hysterics for a diversion. She had tired out the doctors, and exhausted drug shops and spas and travel, and outlived a nurse or two. The deformity man had found a spinal curvature and put on a brace; the gynæcologist had had his turn; the quacks had had their share; and she wore blue glasses to keep out the blessings of daylight. She was five feet four, and weighed ninety-four pounds, and had as much figure as a hat rack, and had no more breast than the average chicken of a boarding-house table. Nature had wisely prohibited this being from increasing her breed. How many of you have stood helpless before this woman? Like you, I had had my failures with such cases, and I was driven to reflect as to what new device I could try. Because everything tired her, I put her at rest in bed. I made rest despotic, absolute. Then I fed her with milk at brief intervals. But in a few days my plan failed. Rest she took to well enough, but attempts to feed resulted in sick stomach and diarrhœa and new loathing of food. Then I said I must find some way to give exercise without exertion. I had seen in Europe how much use was made of massage or kneading of the muscles. I knew that under its use the feeble limbs of ataxics strengthen for a time, so that hopeful friend even dream of a cure, and I was aware that it improved the local blood circulation in a remarkable way and gave to feeble and flabby tissues increase of tone and firm plumpness. It seemed to me that it could take the place of exercise for persons at rest.

" I had also in electricity another means of causing muscles to contract without tha action of will or the exhausting use of nerve-force.

" For the first time, then, I used on a woman at rest, through massage and the abrupt muscle-stirring of an induction current.

" To my great pleasure, I found in a few days a return of appetite and digestion. But is kneading the muscles a mere fetich also? What scientific test have we of its activity? One, and a sure one, which I have lately found. In weakly people, despite the exposure to the air it involves, this process raises the general temperature $\frac{1}{2}°$ to $1\frac{1}{2}°$ F. And as I discovered this winter, to my surprise and pleasure, an induction current, either localized or merely allowed to pass to and fro from neck to feet, does precisely the same. They effect tissue metamorphosis for the patient in tissues little used in bed.

" I have employed every degree of rest: but in this woman's case, as usually, I permitted no exertion which could be avoided, and I carried it to such an extreme as to have the patient fed by hand, because it is tiresome while recumbent to use the arms, and because I have found that

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human beings, like turkeys, can be made to eat more when fed by another agent.

" To this treatment in a few days may be added raw soup and butter, and malt extracts and iron in large doses.

" I fed this woman, with growing surprise at her power to digest as she reddened and fattened. And how did she fatten and redden? The nails became pink; the veins began to show in the limbs. At first, as always, the extremities became cold under massage, then they grew warm, and at last, when she was well, the massage no longer elevated her temperature. And this is the rule. And as to fat, it comes first on the face and neck, and then on the back and belly, and last on the limbs.

" By absolute rest, with massage and induction currents, you acquire power to overfeed, and the tissues are enabled to reclothe themselves with fat and, what is better, you can thus refill the blood vessels. I have quoted one real case—my first. But this is no place or occasion to relate cases, or to enter into details, as I shall elsewhere; but I may venture again to say a word as to two facts, even at the risk of being minute. During the treatment, slight hemorrhages from the nose are not uncommon, but the return of regular menstruation is a better test of the rapid gain in blood. It nearly always becomes regular, and in three cases has returned during the first month of treatment, after absence respectively of three, five, and eight years.

" The gain of fat is sometimes at the rate of one-half pound a day. I have seen it reach three-fourths of a pound a day, but these rates are rare.

" The applications of this treatment are many. I have used it in num_ bers of cases, selecting at first such as had no hopeless organic disease. I have also used it to prepare feeble people for surgical operations, and within a year I have ventured to treat in this peculiar way people in the early stages of pulmonary phthisis. I have seen as to these, some notable facts, and have learned that in some such cases rest and over-feeding are of true curative value; for this is one of the doctor's best lessons that there may be one way or several to a cure. In the early stages of phthisis we have all come to think air and exercise and out-door life the one thing needful or hopeful, and I may be thought insane to propose to treat such cases by rest and excessive feeding; but I promised at the outset to give you personal and practical experience, and this is one; and now and then I have seen it do good service."

It will be noticed that it is intimated above that Dr. Mitchell proposes to give the details, etc., of this treatment more fully elsewhere. There is no doubt of the value of the method in many cases that are otherwise among the opprobia of therapeutics and full accounts of his experience with it, by so eminent and accomplished a physician, will be of very decided value to the profession.

MASSAGE IN WRITERS CRAMP.—Dr. Douglas Graham, *N. Y. Med. ecord,* April 28, besides three cases quoted from foreign journals, reports several cases of muscular trouble from over-exercise of certain sets of muscles,

and one or two of incipient writers cramp that were cured or materially benefited by massage. In one case, that of a pianist, it was of no benefit as far as applied.

As a statement of the indications for the use of massage in these and other cases, he quotes the following from Althaus, which though made in regard to the constant galvanic current, apply also to this method of treatment: " A really effective treatment of scriveners palsy must be an agent which is at the same time both tonic and sedative in its neuropathical effects, which must have the power of restoring the circulation of the blood in the suffering parts to its proper condition; which is capable of promoting the absorption of serous effusions, and will thus cause the nutrition of the maimed ganglia to be raised to a normal standard." This, Dr. Graham thinks, is the real action of massage, and the favorable results he here reports, together with those of the Scandinavian and German physicians whom he quotes, certainly indicates the reasonableness of its thorough trial in these often so obstinate affections.

The following are the titles of a few of the articles that have recently appeared on the Therapeutics of the Nervous System and Mind:

ANGRISANI, Bromids of Potish in the Neuroses and other affections of the Heart, *Rivista Clinica De Bologna*, March; LIEBREICH, On Chloral, *Practitioner*, June; WOOD, On the Action of Drugs on the Motor System of Animals, *Phil Med. Times*, Jan. 20; MACDONALD, Hydrate of Chloral, *St. Louis Clin. Record*, July; FUERSTNER, on the Treatment of Drunkards, *Allg. Zeitschr f. Psychiatrie* XXXIV; FISCHER, On the Hypnotic Action of Lactic Acid, *Ibid*, XXXIII 5 and 6.

BOOKS, ETC., RECEIVED.

The West Riding Lunatic Asylum Medical Reports. Edited by J. Crichton Browne, M. D., F. R. S. E., and Herbert C. Major, M. D. Vol. VI., London: Smith, Elder & Co., 1876. 309 pp.

The Question of Rest for Women during Menstruation. By Mary Putnam Jacobi, M. D. The Boylston Prize Essay, of Harvard University for 1876. Illustrated. New York: G. P. Putnam's Sons, 1877, 202 pages.

Atlas of Skin Diseases. By Louis A. Duhring, M. D. Part II. Acne Rosacea, Ichthyosis (simplex) Tinea Versicolor, Sycosis Non-Parasitica, Philadelphia, 1877. J. B. Lippincott & Co.

Fourth Annual Report of the State Board of Health of the State of Michigan, for the Fiscal Year ending, Sept. 30, 1876. By Authority. Lansing, 1876.

General Index to the New York Medical Journal, From April, 1865 to June, 1876 (Twenty-three volumes). By James B. Hunter, M. D. New York: D. Appleton & Co., 1877.

Transactions of the 79th Annual Session of the Medical and Chirurgical Faculty of Maryland. Held at Baltimore, April, 1877.

Transactions of the American Gynecological Society, Vol. I. For the Year 1876. Boston: H. O. Houghton & Co., 1877. 396 pp.

Transactions of the Medical Society of the State of New York. For the year 1876. Albany, 1876, 412 pp.

Transactions of the Pathological Society of Philadelphia. Volume Sixth, containing the Report of the Proceedings for the session from Sept., 1875, to July, 1876. Edited by James Tyson, M. D., Philadelphia, 1877, 157 pp.

The Curability of Insanity. By Pliny Earle, A. M., M. D. Read before the New England Pathological Society, on retiring from office as its President, Dec. 14, 1876; and published by that Society. Utica, N. Y. 1877, 52 pp.

The Toner Lectures. Lecture V. On the Surgical Complications and Sequels of the Continued Fevers. By William W. Kean, M. D. (of Philadelphia). Delivered Feb. 17, 1876. Washington, Smithsonian Institution, 1877, 68 pp.

The Borderland of Insanity; with examples from the Illustrious Insane; being a popular lecture delivered by invitation. By Eugene Grissom, M. D., Supt. Ins. Asyl. of N. C. Raleigh, 1877, 44 pp.

Viburnum prunifolium (Black Haw). Its uses in the treatment of the Diseases of Women. By Edward W. Jenks, M. D. (Repr. from vol. I., Gynecological Transactions, 1876.)

Notes on the History and Climate of New Mexico. By Dr. Thos. A. McParlin, Surgeon U. S. Army. (From the Smithsonian Report for 1876.) Washington, 1877, 28 pp.

The Mortality of Surgical Operations in the Upper Lake States, compared with that of other regions. By Edward Andrews, A. M., M. D., assisted by Thos. B. Lacey, M. D., Chicago, 1877, 123 pp.

Ein Beitrag zur Aetiologie der Epilepsie. Von Dr. Neftel in New York. (Separatabdruck aus dem Archiv. f. Psychiatrie.)

Poisonous Effects of Cyanide of Potassium. By Joseph Jones, M. D. (From "The New Orleans Medical and Surgical Journal," May, 1877.)

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Review of Nervous Disorders and Insanity. By Prof. D. A. Morse of Columbus, O. (Repr. from Cincinnati Lancet and Observer, April, 1877.)

Therapeutic Use of Faradaic and Galvanic Currents in the Electro-Thermal Bath, with History of Cases. By Justin Hayes, M. D., Chicago: Jansen, McClurg & Co., 1877, 112 pp.

On the Importance of the Uterine Ebb as a Factor in Pelvic Surgery. By Horatio R. Storer, M. D., of Boston. (Repr. from the Edinburgh Med. Journal, Jan. 1877, 13 pp.)

Restriction and Prevention of Scarlet Fever. Document issued by the State Board of Health of Michigan.

A case of Abdominal Pregnancy Treated by Laparotomy. By T. Gaillard Thomas, M. D. (Repr. from Volume I., Gynecological Transactions, 1876.)

The Prophylactic Treatment of Placenta Prævia. By T. Gaillard Thomas, M. D. (Rep. from American Practitioner, May 1877.)

Alcohol as a Food and Medicine. A paper from the Transactions of the International Medical Congress, at Philadelphia, Sept. 1876. By Ezra M. Hunt, A., M. M. D. New York, 1877. National Temperance Society and Publication House, 137 pp.

On the Diagnosis of Urethral Stricture by Bulbous Bougies, with Illustrative cases. By J. William White, M. D. (Repr. from the Philadelphia Medical Times of May 26, 1877.)

The Women's Hospital in 1874. A reply to the printed circular of Dr. E. R. Peaslee, T. A. Emmet, and T. Gaillard Thomas, addressed "to the Medical Profession" "May 5, 1877." By J. Marion Sims, M. D.

THE FOLLOWING FOREIGN PERIODICALS HAVE BEEN RECEIVED SINCE OUR LAST ISSUE.

Allgemeine Zeitschrift fuer Psychiatrie und Psychisch. Gerichtl. Medicin.
Annales Médico-Psychologiques.
Archiv fuer Anatomie, Physiologie, und Wissenschaftl. Medicin.
Archiv fuer die Gesammte Physiologie der Menschen und Thiere.
Archiv fuer Psychiatrie.
British Medical Journal.
Bulletin Générale de Thérapeutique.
Dublin Journal of Medicine and Surgery.
Deutsche Medicinische Wochenschrift.
Edinburgh Medical Journal.
Gazetta Medica de Roma.
Gazette des Hopitaux.
Gazette Médicale de Strasbourg.
Glasgow Medical Journal
Hygiea.
Hospitals Tidende.
Journal de Médicine et de Chirurgie Pratiques.
Journal of Mental Science.
La France Médicale.
Lancet.
Le Progrès Médical.
Lo Sperimentale.
L'Union Médicale.
Mind.
Nordiskt Medicinskt Arkiv.
Norsk Magazin for Lagensvidenselskabens.
Psychiatrisches Centralblatt.
Rivista Clinica di Bologna.
Rivista Sperimentale di Freniatria e de Medicina Legale.
Rivista Médico Quiringica.
Revue des Sciences Medicales.
Revue Medicale du Nord-Est.
Revue Scientifique.
Revue Mensuelle de Médecine et de Chirurgie.
St. Petersburger Med. Wochenschrift.
ThePractitioner.

Upsala Lakareforenings Forehandlinger.
Vierteljahreschrift fuer die Prakt. Heilkunde.

The following domestic exchanges have been received:

American Journal of Insanity.
American Journal of Medical Sciences.
American Journal of Obstetrics.
American Journal of Pharmacy.
American Medical Weekly.
American Naturalist.
American Practitioner.
Archives of Dermatology,
Atlanta Medical and Surgical Journal.
Boston Medical and Surgical Journal.
Canada Medical Record.
Chicago Medical Journal and Examiner.
Clinic.
Cincinnati Lancet and Observer.
Detroit Medical Journal.
Maryland Medical Journal.
Medical Brief.
Medical News and Library.
Medical Record.
Medical and Surgical Reporter.
Nashville Journal of Medicine.
New York Medical Journal.
Proceedings of the Medical Society of Kings Co., N. Y.
Pacific Medical and Surgical Journal.
Pharmacist.
Philadelphia Medical Times.
Richmond and Louisville Medical Journal.
St. Louis Medical and Surgical Journal.
St. Louis Clinical Record.
Toledo Medical Journal.
Virginia Medical Monthly.

Art. I.—MORAL INSANITY.

By H. M. Bannister, M. D., Chicago.

SINCE early in the present century, the existence of such
a form of mental disease as moral insanity has been one
of the most unsettled questions of psychological medicine. At
the present day there seem to be two prominent ten-
dencies of opinion in regard to this subject. On the one
hand, a large number, and in this country certainly a major-
ity, of alienists deny its existence, and declare that all the
cases that have been adduced of so-called moral insanity, are
either instances of responsible depravity, or else of ordinary
insanity with intellectual aberrations in which the moral
symptoms were only predominant. On the other hand, there
is a very marked tendency among another class of investi-
gators and writers to refer all or a large part of the criminality
in this world to physical deficiencies rendering their subjects
to a greater or less extent irresponsible. Between these two
extremes stand a class respectable in numbers, and still more

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so in scientific authority, who, admitting the existence of moral responsibility in the great majority of cases of crime, still feel compelled to recognize that under a certain circumstances it may be lacking while the intellectual power may be unimpaired. The great importance of the subject in very many practical relations, as well as its scientific interest, renders this lack of agreement among authorities the more deplorable, and furnishes ample excuse for any honest attempt to throw light upon the subject. It is the object of the present paper to present some arguments, perhaps not altogether new, that seem to favor the existence of this form of disease, and to notice some of the objections that have been urged against it.

It has been usual in the treatment of this question, for writers to depend upon what I may call the clinical method, to bring forward illustrative cases and to analyze and discuss them, and to attempt to establish the theory on the strength of the facts. This method is, I think, responsible for the prevalent rejection of the doctrine of moral insanity at the present day. To depend upon facts not absolutely conclusive in themselves, for the establishment of the existence of a condition so ambiguous in many respects as is what we call moral insanity, when the interpretation of these facts is subject, in the case of every one who attempts it, to be vitiated by mental idiosyncracies, prepossessions, and prejudices, is hardly the best method of insuring the general acceptance of a doctrine against which, at first sight to many minds, society seems to be on its self-defence. There is comparatively little deductive reasoning met with in these discussions, and, though the psychological aspects of the question are not altogether neglected, they are too often introduced only secondarily, and are sometimes accompanied with an apology, as if they were an altogether needless digression. But if the possibility of the occurrence of moral insanity can be established on unquestionable psychological and physiological grounds, the question is decided, whether a single one of the cases reported was a genuine instance of the disease or not. And to shift the burden of proof on the other side, I think that at the very worst, it cannot be demonstrated that there is any *a priori* impossibility of its occurrence.

The first essential in the discussion of any question at all abstract in its nature, is a definition of the terms employed. This is especially needful in the present case. One has only to listen to a discussion on moral insanity by able men, to become painfully aware of the confusion of terms, and consequently, of ideas, that is extant in regard to the subject. The phrase itself is a somewhat unfortunate one, and has been often objected to as ambiguous and ill-chosen. It is, however, sanctioned by long usage, and is better than many of the substitutes that have been proposed for it, in that it is sufficiently indefinite to cover the whole of the very comprehensive conception, and it is only by a degree of mental strain that it can be made to cover any other. No one thinks we mean depravity when we speak of moral insanity, for the word insanity carries with it in the average mind the idea of legal irresponsibility, the reverse of which is the essential condition of depravity as ordinarily understood. For present purposes, the word "insanity" may be defined as disease of the brain, producing disordered action of the mind. In this definition the word mind is employed in its generic sense, including all our mental faculties, and the brain is considered as its instrument through which it is acted upon by, and reacts upon matter. The somatic theory of insanity here indicated, is the one now generally accepted by alienists,—at all events, it is not among its opponents that we find some of the strongest objectors to the doctrine of moral insanity.

The word "moral" is even vaguer and more difficult to define than insanity. Its meaning has been stated to be "relating to conscience or duty, to right and wrong." In a secondary sense derived from the above it is often used as opposed to "intellectual;" thus we speak of "moral character," "moral habits," "moral nature," "moral faculties," as opposed to "intellectual nature," "intellectual faculties," etc. It is in this sense that it is used as a prefix to insanity,—by moral insanity we mean something different from intellectual insanity, an insanity affecting our moral faculties. We mean by this term a disease of the brain affecting alone its functions as the organ of the moral nature, disordering the capacity to receive moral impressions and the ability to control conduct for moral ends.

This includes the so-called impulsive insanity, in which the intellect is unaffected, as well as that form in which the moral impressibility is diseased.

In order to demonstrate the possibility of disease affecting the moral faculty alone, it is needful first to prove that this faculty exists, and that it exists separate from those of the intellect. If, as is often asserted by psychologists, and this is one of the main supports of the disbelievers in this form of mental disease, the moral and intellectual portions of our nature are so inseparably connected that one cannot be affected in any way without the implication of the other, there can of course be no such thing as moral insanity. Our intellectual faculties, however, are only modes of mental activity, logical processes based upon our sense impressions, our feelings or our emotions. We can hardly imagine any great degree of intellectual development with the absence or suppression of the more important routes of sensations to our consciousness, the occasional observation of a Caspar Hauser or a Laura Bridgeman, shows both the possibilities and impossibilities in this direction. I do not use the word "intuitions" here for I am not sure that we can consider that, properly speaking, we have such, or at least, that an intuition is anything else than a form of intellectual activity based upon some precedent feeling or mental state, and to use the term here would introduce a knotty question of no profit in the present discussion. To possess an intuition seems to me to require a certain degree of mental activity, the essential distinction of everything intellectual, from the simplest cognition up to the most abstract thought. The word " intellectual" always implies action of the mind. It is different, however, with our sensations and our simpler emotions and feelings, which are essentially passive—they are mental states which we may endure without actually calling anything into action.[1] They are the causes of mental activity, however, and primarily the sole causes, for if we trace back the motives of the activities of the human intellect, we are certain to find somethig else than intellectual

1 This, of course, only applies to our rudimentary emotions, which, indeed, are very difficult to separate from our sensations. Our ordinary emotions, as they are usually experienced, are very complex in their nature and largely made up of intellectual elements.

action as the ultimate cause, there is nothing in psychology that requires us to consider the human intellect as a self-originating and self-perpetuating activity, dependent upon self alone, the one example of perpetual motion in the universe outside of the attributes of Deity. Yet this seems to be the necessary conclusion, if we maintain the dependency, or the intimate primary union of the moral faculty and the intellect. We cannot speak of a moral perception without first inferring a moral impression, or of moral judgments without admitting premises upon which they are based. And as we can have no perception varying from the impression that produced it, and no conclusion that is not contained in its premises, it seems impossible to escape the admission that we have among our senses and feelings some special faculty that arouses in us a perception of right and wrong, and which we may call a moral sense, analogous to our sensations in many respects, but belonging probably to that class of inner senses or feelings, or rudimentary emotions which, as we have said before, are very difficult to separate from our external sensations in any classification of our mental states.

Whether we consider this moral sense as a primary feeling, as seems probable, for many reasons, or as a derivative one composed of still more elementary feelings, or as a necessary sequent of some other state, it does not materially alter the case, as regards the present question of moral insanity. The principle, that there can be no conclusion that is not contained in its premises, is valid in every case. But leaving this out of the account, the question is a practical one, and the sense of moral rightness, as possessed by every normal individual, is to him primary, and it is only by an intellectual effort and a somewhat elaborate process of reasoning, founded in part on assumptions, that it can be made to appear otherwise to him. The latest advocates of the utilitarian theory of morals, for example, do not hold that every man evolves his own ideas of right and wrong from his own sense of happiness or utility, but that our present sense of right and wrong is the result of the long experience of the race, and is beyond the power of invention of the individual, or, in the language of Mr. Spencer, " the experiences of utility, organized and consolidated

through all past generations of the human race, have been producing corresponding modifications, which, by continued transmission and accumulation, have become in us certain faculties of moral intuition, certain emotions responding to right and wrong conduct, which have no apparent basis in the individual experiences of utility." Admitting this, we have then here the materials for moral judgments, but not the judgments themselves, feelings acquired, it may be, originally through intellectual action, but now mere mental states, involving in the individual no intellection, properly speaking, in the mere fact of experiencing them. Moreover, according to the evolution theory, with which this utilitarian doctrine is intimately connected, they are necessitated by inherited modifications of structure, and belong to the same class of phenomena as animal instincts, which are also to be considered as inherited experiences, and, at the same time, physiological necessities induced by structure. Certainly, no one, in the ordinary use of language, as in writing or speaking on medical subjects, will call the instincts a part of our intellectual nature.

I have probably said enough in regard to the distinction between the moral and intellectual faculties, but for the sake of clearness, it will be well to review the psychological argument. It may be summarized as follows: The intellect is essentially active; activity is the sole condition of its existence. The simplest intellectual product is an idea, or a judgment, which must be based on premises necessarily antecedent to the act. These premises we find in our sensations and feelings, which are as essentially passive as the intellect is active, and which, being primarily antecedent to intellectual activity, must be independent of it. We cannot speak of moral perceptions or moral judgments without inferring preceding moral impressions or premises, which we find in the moral feeling, or sense that is found in every normal individual. Whether this feeling is primary, or the result of development in the race, the case is the same; it is primary in the individual and can not be subjectively analyzed into simpler elements, any more than any other of our simple feelings or sensations.

I have not mentioned the physiological and pathological

evidence of the independence of the intellectual and moral faculties. They will be noticed when I come to speak of their disorders. But on psychological grounds, merely, it appears to me there is no valid argument in favor of their mutual interdependence. The intellect depends upon the sensations and feelings, including in the more highly developed beings, the moral sense, but the latter may be passively endured without any intellectual action whatever.

If we have a distinct moral sense, coming into the general category of our sensations and simpler feelings, there is every reason, from analogy, to believe that it may suffer disorders, be pathologically exalted, suppressed, or perverted; that we may have either moral hyperæsthesias, moral anæsthesias or hallucinations. If, on the other hand, our so-called moral sense is an acquired instinct, the result of inherited experience, the probabilities are very little altered, instincts are also liable to undergo alterations from disease, and we have, in this case, almost an absolute certainty of occasional pathological reversions toward the original undeveloped type, and congenital deficiencies from defect of development. The parallelism between this moral sense and our other senses will, perhaps, be more apparent when we consider that in cases of perversion of these latter it is not usually the functions of the external apparatus that are disordered, but those of the inner organs, between the external ones and the perceptive centers. These organs are not as yet matters of anatomical demonstration, but it is a physiological certainty that they exist. For example, when a leg is amputated, its sensational organs still remain in the central nervous system, and the subject may have a tactual hallucination referred to the toe of the lost member, with, at the same time, a perfect visual perception and intellectual consciousness of the absence of the limb. Hallucinations of sight, very vivid in their appearance of reality, may often be voluntarily produced by opening the eyes in a perfectly dark cave or mine, where the physical conditions are equally such as to absolutely prevent any exercise of the functions of the external organs of the sense involved.

Now as our simpler emotions, or feelings, or inner sensations, precede our perceptions of them, whatever their source

may be, they must necessarily act through some cerebral organs below (in a physiological sense,) the perceptive centres, and these organs, like every other portion of the brain, must at times necessarily be subject to disease. The organs being diseased the result must be disordered function, a morbid product of emotion, or sensation, being the consequence of the imperfection of the instrument. The existence of these organs ought not to require any elaborate argument for its proof. No one will think of denying that the body generally is the instrument of the mind, in all the relations of the latter with the world about us, the bones forming its frame-work and all its active organs, the muscles, viscera, etc., performing their functions under the direction of the nerves, and these again receiving their controlling impulses from the highest nervous centre, the brain, which, in one sense, is the microcosm of the whole physical organism, and it would be an altogether unreasonable error to stop at first sight of this great mechanism, this complex of cells and fibres, the arrangement of which we can follow out with the microscope, and to introduce here an element of mystery. Mechanism always implies function, and *vice versa,* and as we consider the brain the organ of the mind, and necessarily of all its functions, the greater including the less, so in this complication of ganglion cells and commissural fibres, etc., we naturally look for the apparatus by means of which the receptive capacity and motive power of the mind is put in relation with the external world. We have, indeed, an approximative idea of the parts of the brain in which these receptive centres are to be looked for; physiologists, or at least the majority of them, are inclined at present to consider the cortex of the middle and posterior lobes of the brain as the seat of emotion and sensation. The *corpora vili,* which we are obliged to use for our experiments, are not, it is true, such as will best serve for the elucidation of the cerebral localities directly exercised by the higher feelings, or simpler emotions, though we are able to localize the sensory centres. The only emotional centre (or centre that could be called in any way emotional,) that Ferrier was able to localize was that for the sexual feeling; but this is a very important one. The result of the removal of the occipital lobes in monkeys was

apparently a loss of the sense of hunger, hence he concludes that the organic sensations (as Bain has called them) which differ from the tactile sensations, and of which hunger is one of the chief, may have here their cerebral seat. Now these organic or visceral sensations are, apparently, much more nearly connected by a reciprocal relation with our emotional than our intellectual states, and to the former, than are our general tactile or special senses. We cannot, for example, stop our heart or derange our digestion directly by an act of the will, but an emotional shock may do either, and, on the other hand, more real mental depression and emotional aberration is produced by the vague and often almost imperceptible discomforts from dyspepsia, or uterine irritation, than by the most ferocious trigeminal neuralgia or other severely painful excitation of peripheral nerves. In fact, mental or, to speak more exactly, moral or emotional symptoms are not infrequently the only prominent subjective phenomena of indigestion. Hughlings Jackson, in speaking of the sensations of the aura of epilepsy[1], makes the following remarks which are to the point. " It is probable that the aura from the neighborhood of the epigastrium (sensation referred there, that is) is a crude and excessive development of visceral and other systemic sensations. However, if so, it seems strange that these sensations should, as is most common, occur in those cases of epilepsy in which loss of consciousness is, next to such warning, the first event in the paroxysm. For it implies that systemic sensations are first and most represented in the highest processes. Epilepsy, in which loss of consciousness is the first, or one of the first events, is often preceded not only by development of systemic sensations, but is attended by pallor of the face. Indeed, the experiments of disease seem to show that the very highest processes (those underlying consciousness) sum up and represent all lower processes of the body. The epigastric sensation, so-called ' aura ' is variously described by patients. Some speak of it as a ' fear;' a woman, nineteen years of age, said it was 'a frightened feeling, as if I had done something wrong;' another patient said it was ' an undescribable feeling of horror.' Women at the change of

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life, and other persons, will complain that they feel depressed, and as if they had done something wrong, and when asked the seemingly ludicrous question ' where do you feel it?' will put their hand over the epigastrium."

Without attributing any too much value to such cases, as those above cited, it may be safely said that they indicate very plainly the close relations of our subjective feelings with our systemic sensations, and are especially suggestive, as regards the moral sense, which, as we have shown before, must be regarded as a primary feeling of the individual. In all these cases, of the aura in epilepsy, women at the climacteric period, etc., the simultaneous experience of the feeling of horror, fear, or morbid sense of wrong-doing, and its reference to some distant viscus, show the intimate connection and the probable contiguity of the cerebral centres for these feelings, and those for the organic sensations from the said viscus, and seem to show that in localizing the one, we can also approximately locate the other. At all events, a locality is made to seem probable, or at least collections of ganglion cells serving these particular functions. If we take, in addition to the above, the sexual feeling, the one which has been experimentally located with some approach to exactness, we find abundant support for the proposition that the emotional faculties may be diseased, independently of the intellect. Reduced to its simplest expression, there is nothing properly emotional in this feeling; it is merely the subjective expression of a physiological want,—a *besoin*, as the French say,—and as such, it probably exists in the lowest animals that have a sexual existence. But, as we ascend in the scale of being, we find it essentially a cerebral endowment, dependent, to some extent, upon the reaction of peripheral organs in its development, but having a central seat, and liable to be disordered by central disease. It is peculiar in that it is closely allied to our feelings which have only a central organ, and it is also directly connected with an external and, as it were, a special sense. It, therefore, shows the close relations between our feelings and our sensations, if not their actual identity, as I hold. In its higher phases this feeling has a wider range of relations, and more to do with the motives of human action than any other, ex-

cept, perhaps, the moral sense. Now there is no other that is more plainly disordered at times, without any apparent intellectual trouble accompanying it. Besides the form or forms of insanity known under the names of aidoiomania, erotomania, satyriasis, etc., and which need not be accompanied by any intellectual aberration, we have some most curious and outrageous perversions of this impulse in otherwise sane individuals, indicating very serious derangement of the affections, such, for example, as the "contrary sexual feeling," or the passion that is sometimes displayed by persons for others of their own sex, etc. Much more might be said on this subject; it is a very extensive one, but it has been sufficiently alluded to for our present purpose.

In what has been said, I have tried to present the evidences that lead us to the conclusion that our inner feelings, among which I include the sense of moral rightness, may become independently disordered from disease of the brain. To prove more conclusively that such a condition as moral insanity may exist, it is worth while to look more particularly into the question whether this special form of feeling may alone be subject to alteration from disease. It may be admitted that certain of the feelings which man possesses as a thinking animal, may be altered or suppressed, and yet objection be made to the supposition that those higher qualities which especially invest him with the character of a moral and responsible being, the "image of his maker," can be disordered through physical derangement, and that without any serious implication of his thought power or intellectual capacity. I have endeavored, while avoiding any indorsement of necessarily materialistic doctrines, to still so state the case that the argument might be equally valid if such were adopted. The somatic theory of insanity does not imply materialism; it would be truly unfortunate if we had to accept any doctrine involving the conclusion that the immaterial immortal part of our natures could suffer disease apart from its physical instrument, the brain. The moral nature is either the highest direct endowment of mankind, or it is the latest and best result of his long continued culture, and in either case its exercise is the highest function of his complicated cerebral machine. Is it not natu-

ral that such a mechanism might become just so much disordered as to fail in its more delicate performances while still doing good work of a lower grade ? The argument based on the doctrine of cerebral localizations, in which I thoroughly believe, holds good in the case of this moral faculty or sense as in all the others. Still, in its stricter signification it is not an essential argument. The moral sense may be allowed to have its seat all over the cerebral cortex and in the basal ganglia, down the spinal cord and throughout the peripheral nervous system, and yet it may be affected separately from all other faculties, so far as the absolutely essential physiological and pathological conditions are concerned. Admitting that the exercise of this sense depends upon structure, and no other hypothesis seems possible, all that is really necessary are certain special receptive ganglion cells, and the power to transmit, it may be, special molecular vibrations over nerve fibres to the perceptive and intellectual centres, either of which, the ganglion cells or the conduction, may be liable to disorder.

There are some pathological and, indeed, I may say, some physiological conditions that are extremely suggestive as regards the possibility of disorder of this moral sense. Our dreams may be fantastic and incoherent, but they may be the reverse, the intellect may act in sleep with greater power and exactness than during our waking hours. The instances of remarkable mental performances during sleep are sufficiently well attested and, indeed, are not so extremely rare as to be beyond the experience of ordinary individuals. I have known a gentleman who, to commit to memory a passage even of some length, claimed that he had only to read it over once or twice at bedtime to have it well fixed in his memory upon awakening. I presume that very many individuals can relate much more remarkable instances than this in their own experience. The intellect is certainly active and often normal in its action; the feelings of joy, pain, fear, etc., are exercised as in the waking condition; all the animal and intellectual nature seems alive, but the conscience is practically dead, or only very exceptionally active. even in those usually most attentive to its dictates. We do things in our dreams without a compunction that would horrify us in any other condition, and I think there

are comparatively few dreamers who cannot personally support this statement. This immoral character of dreams has been noticed by many writers, from the old philosophers and Christian fathers, down to the present time. Dr. Laycock[1] accounts for it by supposing it to be due to a partial reversion; in sleep we are in the condition of the morally imbecile; the later acquired higher sentiments, less deeply implanted in our organism, are inactive and unable to repress the immoralities due to the excitation of the lower instincts. But, no matter in what way we attempt to account for the fact, it is a very interesting and suggestive one in this connection, and it possesses a certain value as supporting the view that it is possible to have a complete absence of the moral sense with perfect integrity of the intellectual functions.

In their relations to this question the facts of trance and somnambulism are much the same as those of dreaming, but they are even more instructive, in that they show how completely one or more faculties may be separately locked up and put out of action, and also in showing the connectedness and coherence of the intellectual operations during these conditions. Dreams are purely subjective, moreover, but these conditions may be studied objectively, a real advantage in this investigation. In these cases also the same absence of the moral sense as in dreams has been noticed; in the famous case of the French sergeant reported by Dr. Mesnet[2] a tendency to steal, a real kleptomania, was one of the prominent symptoms of the attack. Somnambulism is nothing but acted dreaming, and the same condition of the higher feelings is to be expected in one as in the other. The phenomena of the intoxication produced by various drugs are of much the same nature. By their use we throw the machine of our mind out of gear for the performance of its highest work—the reception of the dictates of our moral sense, and in the exercise of its lower functions of intellection and baser feelings it runs away, until even these are finally disordered and the whole apparatus breaks down. Certain stages of opium and haschisch intoxication are moral insanity in everything but the ele-

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1. On some Organic Laws of Personal and Ancestral Memory. *Jour. Mental Sci.*, July, 1875.

2. *L.'Union Médicale*, July, 1874. Transl. in this Journal, Jan. 1875.

ment of irresponsibility for its inception. Their short dura-
tion and voluntary cause put them in a legal sense out of this
category, since we cannot hold a man legally irresponsible for
being temporarily in an irresponsible condition, when he has
voluntarily placed himself in it from unworthy motives.

In all these cases in which the theory applies of the repres-
sion of the moral sense by injury or suppression of function
of its cerebral organ, like that which may occur to any of the
external senses by lesion of their inner receptive ganglia, or
like the loss of the signs of ideas in aphasia from injury to the
third left frontal convolution, the subject may be absolutely
immoral, may have no receptive capacity for moral impres-
sions, as seems to be often the case in dreaming. Moral
insanity, as generally understood, however, includes another
form of mental disease producing irresponsibility in which
the moral impressions may be even acute, as such, but still be
entirely without influence upon actions. The conditions of
moral insanity, as I have defined it, exclude from considera-
tion here all the cases of delusion, and also those of masked
epilepsy which are really accountable for a large part of the
cases of so-called homicidal, and otherwise criminal, impulse
that are met with in the records of our courts of law. The
subject of impulsive insanity must simply be under the power
of an impulse too strong to be controlled by his will, guided
by his reason. If his moral sense is involved, there is no
reason to call the case one of impulsive insanity; the element
of sudden impulse is not an essential one, and the explanation
we have already given of moral insanity will suffice. Still, in
these cases the lack of the check of conscience weakens the
power to resist natural evil impulses, as we have already indi-
cated, when speaking of the lack of the moral sense in dreams.
There is, therefore, in the one case no necessary increase of
force in the impulse, the lack of the most important stimulus
to restraint is enough to influence conduct; in the other the
morbid impulse is so pathologically intensified as to overcome
the, perhaps, still normal moral will, and this latter condition
constitutes the so-called impulsive insanity, properly speaking.
There are relations and, perhaps, gradations between the two
forms, but the principal feature which they possess in common
is that the intellectual powers may remain unimpaired in either;

that disorder of intelligence, is not an essential part of the insanity.

To prove that this kind of insanity may occur, I shall have to use evidence derived from nearly every one's personal experience. We are all, or most of us, aware occasionally of irrational impulses which, however, are not sufficient to overcome our reason or sense of propriety. It is not necessary to give examples, they occur so often in normal individuals. When we try to analyze these impulses we may find some of them originating in an idea, or thought, not necessarily abnormal in its nature, but from which they have most completely cut loose, others may be connected with animal appetites, and still others are absolutely unaccountable, unless we adopt the views of the evolutionists and consider them as connected with ancestral traits, reversions due to the calling into action of some antique structure in the brain now normally past its usefulness, but retained as a useless inheritance from some forgotten ancestry. We seem to have such organs in other parts of our body, and it is not assuming too much to consider it possible that there are such in the brain. Then, in the lower animals, we notice what appear to be such ancestral tendencies; domestic cattle, that have never snuffed danger from wild beasts will go wild at the sight of blood, or become frantic at the mere odor of a menagerie. The objection is often made that it is a dominant idea that exists in these cases and, therefore, that we cannot consider it other than a disorder of the intellect. Some writers, who uphold fully the doctrine of moral insanity, Dr. Maudsley[1], for example, have, by a somewhat unfortunate use of language, given, inadvertently, some support to this objection. To it I would reply that it is the dominant impulse, not the idea, that constitutes the disease; the latter may occur to any one without the first trace of the former, or if the impulse should exist, but remain under the control of the will, the case would not call for consideration here. The objection is a psychological error, and is a good example of the loose reasoning and confusion in regard to the use of words so often met with in the discussion of this subject. I cannot do better than quote the words

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1. Body and Mind, p.

of Maudsley[1], when speaking of this insane impulse.   He says : " The physician who studies insanity as a disease finds, then, that he has mainly to do with the reflex action of the spinal cord, of the sensory ganglia, and of the ganglionic cells of the cerebral hemispheres, as causes of the morbid phenomena.   There may be a consciousness of the reflex actions of these different reacting centres, and yet an inability to resist them, as there is notably a consciousness of the reflex action of the spinal cord with an inability to resist it.   By an act of the will, a person may prevent the involuntary movement of his limbs when the soles of his feet are tickled, but the strongest-minded mortal could not prevent spasms of his limbs on the application of a stimulus if the excitability of the cord were increased by a dose of strychnia.   A similar condition of the ganglionic cells which minister to sensation or idea, may be brought about by physical causes, and an idea or impulse, of which there is consciousness, may then become uncontrollable."

With the exception of the use of the word "idea," which seeems to bring in intellection, the above passage indicates very correctly, it appears to me, the true explanation of incontrollable morbid impulse.   Just as the excitability of the cord may be increased by strychnia or in tetanus, so may that of the brain be augmented by drugs or various processes of disease.   The pathology of this condition is not at all difficult to understand, and its possibility certainly cannot be denied on any *a priori* grounds.[2]

1. Homicidal Insanity. *Jour. of Mental Sci.*, Oct., 1863.

2. I have said nothing of the derangements of the moral sense in the direction of its exaltation or morbid excitation. Such cases have not the same forensic importance as those in which it is diminished or suppressed, and it is only following common usage to leave them out of consideration when speaking of moral insanity. Yet they undoubtedly occur, and have really as good a right to be considered in this class as the alterations in the other direction. Many cases of incipient melancholia, before any intellectual trouble appears, are accompanied with a morbid conscientiousness, —an excessive sensitiveness to moral impressions, to express it physiologically. But melancholia in all its phases is so commonly reckoned as an ordinary form of insanity, that its relations to that which forms the subject of this paper are hardly ever practically recognized. It seems probable for some reasons, moreover, that these cases of moral hyperæsthesia tend

Before I begin to especially take up and notice the objections that have been made to the recognition of such a disease as moral insanity, it may be well to offer a condensed statement of the points already made in this paper. They may be summed up as follows:

I. The brain is the instrument of the mind, the immaterial part of our nature, through which it receives impressions from, and reacts upon its surroundings.

II. Its functions may be classified as receptive and dynamic, the former comprising feeling (including sensation), and the latter intellection, volition, and the control of our other bodily organs.

III. The simplest product of intellection is an idea or a judgment based upon premises found in our feelings. These being thus antecedent in the order of development to intellection, must necessarily be independent of it.

IV. Among our feelings we find the moral sense, which, whether it is considered as a direct endowment from the Creator, or as existing in the species as a derivative from still other feelings, must nevertheless be admitted to be primary as far as the individual is concerned.

V. Like all the other senses or feelings, this moral sense must have its special mechanism in the brain for the reception of moral impressions. (For various reasons it appears that this apparatus is localized in some particular part of the brain. This, however, is not, in its stricter sense, an essential point; all that is requisite is that it have its special ganglion cells and connections.)

VI. This mechanism, like every other portion of the brain, is liable to be disordered, thus producing disorder of its functions. That this may occur without implication of the organs concerned in intellection is probable from the following reasons:

a. From analogy, since we know that other special faculties or senses may be separately affected;

to a more rapid development of intellectual trouble, than the other perversions of this moral sense which put their subjects back, so to speak, into a state of nature, rather than prematurely advance them to an unnatural and irregular state of moral development.

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b. Because the reception of moral impressions is the highest and best capacity of the human mind, the functions of its cerebral organ are consequently the highest and most delicate in the whole economy, and are not developed, at least to any extent, in any of the lower animals, which nevertheless give very decided indications of intellectual development. It is natural, therefore, to think that our brain mechanism may fail in this its most delicate work, while still meeting all lower demands.

c. The facts of dreaming, somnambulism and trance directly indicate that the moral sense may be suppressed or weakened without affecting the other mental faculties, or, at least, without directly embarrassing the intellectual powers, in so far as they are exercised.

VII. In the peculiar phase of mental disorder known as impulsive insanity, the disease consists in an increased excitability with diminished volitional control, increasing the force of certain morbid impulses to which even sane persons are liable, and diminishing the power of the patient to resist them. In persons of naturally weak will power, though not deficient in intellect, the condition of, at least partial, irresponsibility may be said to always exist. In this impulsive insanity, the moral sense may be acute, and the patients may strive to the utmost and yet be unable to resist the impulse.

In what has been said, I have not quoted a single illustrative case of the phase of insanity, the possibility of the existence of which I have tried to demonstrate. Such cases exist and are classic in our literature. The question, as stated in the beginning, is in regard to the interpretation of the facts. In many of the cases it is admitted that no traces of intellectual aberration were detected; that the test of insanity, by comparing the patient with his normal self, was fully met, and yet objection is made to their admission as evidence of the existence of the disease. To take this ground, it would naturally seem that the objectors ought to be able to present some valid *a priori* reason of its impossibility, a thing which, if there is any value to the arguments already adduced in this paper, is an impossibility itself. The objectors do not furnish anything as such, except the statement of the inseparableness of

the moral and intellectual faculties, which, as I have tried to
show, is not a valid one, as affecting the question before us. We
have abundance of observations of isolated lesions of faculties,
memory of special events, languages, of the loss of speech, etc.;
and the tendency of modern physiological investigation is to show
that all our mental faculties are separately localized in special
portions of the brain. The vagaries of unscientific phrenolo-
gists and quacks, which have so many times afforded an op-
portunity for a sneer to the opponents of the doctrine of moral
insanity, will, therefore, hardly serve any longer to throw dis-
credit upon the great principle of localization of cerebral
functions, a misapplication of which is the basis of modern
phrenology with all its quackish developments. But, as I have
tried to show, strict localization, in the sense of having special
functions limited to special corresponding regions of the brain,
is not essential; all that is required is that there shall be sepa-
rate ganglion cells, which may be disseminated all over the
nervous system, and which may become specially disordered.
Moreover, the moral sense, or whatever gives us the subjective
basis for moral perceptions and judgments, belongs to a class
of faculties that are to the mind what sensations are to the
physical organism; they are the incitants of the mind's activi-
ties, the intellect is dependent upon them; but the relation is
not a mutual one. There is nothing, therefore, to show the
necessary unity in the sense implied in the objection, but
every reason from analogy to lead us to conclude that the
moral sense may be independently affected by disease.

Another objection that has been frequently adduced lately,
is, that statistics do not show the occurrence of this form of
mental disease; that in the records of our asylums there are
no cases of moral insanity. According to the plan laid down
in the beginning of this article, the argument being only for
the pathological possibility of the occurrence of moral insanity,
or, rather that it could not be proven an impossible occurrence,
it is not essential to my case to meet this objection. I am very
willing, however, to accept it for all it is worth, and see no
difficulty in replying to it. In the first place, there is so much
depravity in the world, that a case of moral insanity will pass
without general remark, until by some delinquency it is

brought before the courts. Even when a man of high social
standing suddenly changes completely in his character, with-
out apparent cause, and from being an estimable citizen,
becomes a perfect devil amongst his family and friends, the
nervous symptoms are usually, if any exist, overlooked, and the
past history of an insane temperament, hereditary psychoses
and neuroses, disease of brain, etc., is ignored. It is only after
marked delusions appear, or the morbid process in the brain
has so extended as to implicate the organs concerned in in-
tellection, and render the subject less vigilant and methodic
in his madness, less mindful of the dictates of prudence, that
he is committed to an asylum. I will admit that in cases of
moral insanity there is generally, if not always, a tendency
toward ultimate disorder of the intellect. Hence I consider
the statement so often triumphantly made, that close and long
continued observation of cases of so-called moral insanity will
finally reveal intellectual disorder, as having no point whatever.
The asylum physician is naturally hesitant about classifying a
case as one of so dubious a form of disease as moral insanity,
and hence, when one appears in which intellectual trouble is
wanting, he keeps it under observation for an indefinite period,
and when, at length, the delusion appears, it is straightway
concluded that it had existed all the time. It might be just
as reasonable to presume that it had existed from birth. When
recovery takes place without this manifesting itself, it is
considered, on the strength of these other cases, that intel-
lectual disturbance was there, all the same, though it could
not be discovered. In fact, it is much more a matter of sur-
prise, under the circumstances, that, at the present time, a
case of moral insanity is ever recognized in our asylums, than
that so few are reported. Nevertheless, such cases do seem to
be met with by perfectly competent observers ; and some even
of the disbelievers in moral insanity have reported cases that
have cost them much laborious argument to prove that they
were not instances of the disease.

We ought not, of course, to apply any severer test of intel-
lectual soundness to these cases than to ordinary individuals;
yet I fear this has often been done by alienists to relieve them-
selves from the necessity of admitting the existence of this

form of psychosis. When we consider how little difficult it
would be to discover in perfectly sane persons, intellectual
idiosyncracies which, in connection with special emotional
disturbances, might suggest doubts as to their mental sound-
ness, it is easy to see how they may be met with in the mor-
ally insane.   I once knew a gentleman in good social position,
and of more than ordinary general intelligence and informa-
tion, who, like the John Hampden of the present century,
steadfastly maintained that the world was flat.   He was per-
fectly sane, a good reasoner, but on this point purely induc-
tive and, like many other people, he was unwilling to general-
ize except from particulars within his own range of personal
observation.   His argument, however, appeared to me quite
as valid as those against moral insanity based upon statistics.
One case that cannot be overthrown is ample upon which to
found the nosological species.   If a single uncontestable in-
stance ought to justify a belief in miracles to any reasonable
skeptic, notwithstanding the fact that they seem to him to be
opposed to all natural laws, how many cases are required to
prove the existence of such a disease as moral insanity, the
occurrence of which is perfectly in accordance with what we
know of psychology and pathology.

The great sources of all objections, however, are the misuse
of terms and misconceptions.   One does not have to look far
to find these, they are apparent in nearly every leading text-
book.   I have already noticed the carelessness with which
Maudsley uses the word "idea."   I will now pay a little
attention to what is said by one or two others on the question
in ambiguous language, or such as shows misapprehension of
the subject.   In my definition of the term " moral insanity,"
I tried to so express it as to exclude any possibility of any
ones understanding by the words anything at all necessarily
involving intellection.   It was, in what followed, attempted to
demonstrate that it is a perfectly legitimate physiological and
psychological presumption that disease of the brain might so
involve the cerebral centre for the reception of moral impres-
sions as to disorder its functions, or even altogether suppress
them.   Intellection would of itself be unimpaired, but it would
have only one less stimulus—it would be just as correct to call
a congenitally blind man intellectually impaired, because he

could form no idea of color, and only a partial notion of form. Yet, in the latest edition of Wharton & Stillé's Medical Jurisprudence, §§ 531 *et seq.*, I find a lengthy argument, the summing up of which is that insanity cannot be psychologically shown unless it involves thought. If the authors would go a little further, and say it cannot be shown unless it involves acts, I would agree with them, but I would not then speak of it as essentially "physical insanity." These gentlemen say that the pyromaniac, for example, must form the idea of the act of setting fire to the house, that this involves intellection, and that the trouble is therefore an intellectual one. In this they completely ignore the source of the conduct, the insanity is not in the idea of setting fire, or in the act, but in the primary cause of both, the morbid impulse, or the lack of capacity to be morally impressed with the quality of the idea. To the same class of misapprehensions belong such as are indicated in the remark of Balfour Browne,[1] that "if a man does not know right from wrong he reasons badly," a remark that would seem to imply that no premises at all are required for moral judgments, and one which is a contradiction in itself, in so far as there is any difference whatever between knowing and reasoning. If there are any matters of which we have immediate knowledge, right and wrong must be among them. I certainly see no propriety in interposing any process of reasoning before their perception. Right and wrong are certainly not inherent qualities of acts, or even of thoughts in any essential sense; the importance of this question of moral insanity is in its forensic relations, in them the case turns on the question of responsibility, and responsibility depends solely upon the motives. There is no use, therefore, in speaking of the conceptions of criminal acts as constituting the disease, as do Wharton and Stillé, or as if right and wrong depended upon the correctness of the reasoning processes. I might quote many other passages from writers on this subject, were it of any use to·do so ; hardly any one is free from them. Of the quotations made use of in this article, there is hardly one, the language of which can be endorsed as sufficiently exact and definite.

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1 Medical Jurisprudence of Insanity, § 168.

I stated in the beginning of this article that to many minds society seemed to be put upon its self defence against this doctrine of moral insanity. There is an unworthy tendency, from which even scientific men are not altogether free, to apply to all such questions, where the general welfare of society seems to be in any way liable to become affected, the rule of expediency, and to avoid expressing apparently dangerous truths, and even to suppress them as far as possible. Whatever useful purpose this propensity may have served, and indeed it appears to be in the order of Providence that it exists, it certainly cannot command much respect as conducing to a very high standard of moral culture in the individual. To this propensity of human nature are to be credited the pleas that the recognition of such a form of disease as moral insanity can do no good and much harm, by elevating crime to the position of mental disease, and by affording a dangerous means of defeating the ends of justice. The first part of this plea— that the recognition of this form of disease will do no good— is an appeal to one's easy good nature to yield the point as one of no consequence. But if the disease really exists, the statement is not true, for irresponsible parties may suffer wrong by the non-recognition. The second part of the plea—that it elevates crime and defeats justice—is plainly a begging of the question, since it assumes that what is called moral insanity is always really crime, an assumption which, as I have tried to show, cannot be proven, and one which the objectors are not willing to invariably assert.

As for the court decisions and the opinions of lawyers in regard to this subject, which are reverenced to a certain extent by some writers, I allow them, simply as such, whether favorable or unfavorable, no authority whatever. The question, as here considered, is one of psychological medicine, it is not one of English or American law, and it does not recognize their fictions. Decisions are not law in medicine, and authority is only presumption.

In conclusion, I will state the case as it appears to me. There are on record, and come under observation from time to time, certain cases of apparent mental disorder, in which the symptoms point solely to a derangement of the moral faculties

without any intellectual impairment whatever. There appear
to be no valid physiological or psychological reasons why this
state of affairs may not exist due to disease of the brain. Still
it is affirmed that these cases are not what they appear to be,
that every case is either one of responsible moral depravity or
that, whether detected or not, there exists some intellectual trouble
of such a kind as to place it in the catagory of intellectual insan-
ity. But until this is proven, and on those who make this
wholesale affirmation that there is no such thing as moral in-
sanity rests the burden of proof. I cannot positively refuse to
admit that this form of mental disease may and does exist.

Art. II.—CONTRIBUTIONS TO ENCEPHALIC ANATOMY.

By Edward C. Spitzka, M. D.

I.

The Methods employed in, and Deductions admissible from, Cerebral Anatomy.

AS Henle has well said, the tracing of each nerve root
offers sufficient occupation for any single observer, and
the suggestion, thus thrown out, of a division of labor in the
intricate and difficult field of cerebral anatomy, is certainly a
good one. But as long as the various investigators in this
field have not come to a mutual agreement, according to each
monographist his separate and appropriate field, they will, as
heretofore, be forced to content themselves with furnishing
scattered and fragmentary contributions to the existing stock
of knowledge. May this serve as an apology for the discon-
nected character of the present communication.

Neuro-anatomy boasts of more numerous, as well as more
elaborate methods, and enjoys a far wider scope, than any

other branch of morphology. In its methods it exhibits an intimate blending of the macroscopic and the microscopic; in its scope it includes not only the systemic relations of nerve-strands and topography of the gray masses, but also a comparative study of the brains of lower animals, the results of pathologically or experimentally induced secondary atrophies, as well as the embryonic development and teratological aberrations of the nervous system.

After hardening a hemisphere of the cerebrum in absolute alcohol, or in a combination of Mueller's fluid and methyl-alcohol, we obtain an insight into the coarser relations of the larger nerve strands. In studying the connections of the larger associating garlands (fasciculus longitudinalis sup. and inf. f. uncinatus), this is the *only* available method, and I must here insist, that in the hands of an expert dissector, who is able to supplement the defibrillation of these coarse fasciculi by a microscopic examination of their terminal portions, trustworthy and valuable results are obtainable, and consequently, take a most decided exception to Forel's objections to the method of defibrillation. It is certainly impossible to obtain the whole length of any bundle of fibres in the corona radiata within one microscopic section, for the simple reason, that not one of these bundles runs in an even plane. In the peduncular tracts, however, as well as in the basal ganglia, various fasciculi are so closely interwoven, and so frequently interrupted in their course by intercalar or foreign gray matter, that nothing less than an accurate microscopic study of successive sections will assist us in unraveling this labyrinth. It is not often our fortune to be able to trace any single nerve fibre through a large series of sections, but this is not as Forel seems to think, at all necessary. There is so much individual variation, that such painstaking investigation of individual axis cylinders might prove futile, in demonstration whereof I need but refer to the variations in the pyramidal decussation, in the development of the upper olive, as well as of the nuclei and fibrae arciformes, which latter are almost entirely absent in some subjects.

For the study of the anatomical relations of the peduncular tracts, I have for the last two years employed the following methods, which I can recommend.

The Island of Reil, basal ganglia, crus, pons, medulla, and
cerebellum, are separated entirely from the cerebral hemi-
spheres[1], and laid on cotton, in a flat-bottomed dish, filled with
a solution of bichromate of potash. The object of this is to
get the axis of the peduncular tracts into a straight line, in or-
der that sections vertical to this axis may permit of an insight
into the finer relations and connections of the nerve radicles.
Sections made in any other direction fail to exhibit the latter in
their whole length, and it is this fact which renders Gudden's
sections of comparatively little value, for in them the nerve
roots are not continuous, but interrupted. The preservative
solution may be dense or dilute, according to the temperature
in which the parts are to be kept. If the apartment is warm,
rapid hardening is desirable, and the solution may be almost a
saturated one; if it is cool, from one to two per cent. solutions
can be employed, and a longer time is given to the hardening
process. That the latter method yields by far the best results
I need not add. If the investigator has the whole body at his
disposal, he can ensure the best results, and not only accelerate,
but also equalize the hardening process, by injecting the inter-
nal carotid and vertebral arteries, or either, with a two per
cent. solution of the salt. This is continued under *exceed-
ingly gentle pressure*, until the fluid running from the jugulars
is no longer tinged with blood.

Where time or circumstances do not permit the employ-
ment of this circumstantial method, I usually do not preserve
the peduncular tracts as a whole, but divide them into seg-
ments freshly. It is a matter of experience that these seg-
ments harden much more rapidly, and equally, than the whole
ambitus, and a previously prepared series of sections, through
an ambitus, hardened as a whole, will assist in localizing the
altitude to which a section obtained from any part, separately
hardened, belongs. For histological purposes, the sections
obtained from parts hardened in bichromate of potash, to
which a little chromic acid may be added, are preferable.

[1] This procedure, first employed by Meynert, is now becoming a part
of the autopsy routine, where accurate results are desired. I described
it last year, in the introduction to a paper on the methods of making autop-
sies in insane subjects, read before the International Medical Congress, in
Philadelphia.

Those hardened in alcohol stain too diffusely in carmine, and the absolute alcohol, which can alone harden brain tissue well, possesses additional disadvantages, which forbid its use in histological investigations of nerve tissue, namely, it extracts *leucin* and other chemical bodies from the nerve substance, and precipitates them diffusely throughout the basis substance in larger and smaller spheres, or even in the perivascular channels. Many descriptions of colloid and fatty granules in the brains of those dying insane, or even hydrophobic, have no other basis than these artefacts.

Having thoroughly explored the topography of the ganglia, nerve nuclei, strands, and roots, as these are seen in such a series of transverse sections, it becomes desirable to study the longitudinal fasciculi in profile, that their terminal points and origin may be determined. As a rule the purely sagittal section series will suffice for the determination of most longitudinal strands; for many, however, an oblique sagittal, oblique frontal (I always speak of the peduncular axis as if it were a continuation upwards of the spinal, which is far from being the case, but which renders our topographical designation far simpler than if we were to follow the inflections of the basi-cranial axis), or other direction is necessary. It would lead me too far in this place to describe the procedures necessary for each fascicular-tracing, but shall briefly refer to them when describing the fasciculi themselves.

Where a nerve bundle does not run in a straight line, or at least in a plane, I am sometimes able to represent its whole length in a single microtome section, by adopting the following procedure: the bundle is superficially exposed in its whole length by a cut with the section knife, made on the fresh brain, a wedge-shaped piece is cut out from the opposite side of the specimen, and then the cut surface, in which the desired bundle is, placed downwards, so as to become flattened on the floor of a flat dish filled with preservative fluid. The floor of the dish is covered with an even layer of cotton, or some other permeable material, as otherwise the preservative re-agent might not penetrate to the under (required) surface of the specimen. The object of cutting out a wedge-shaped piece is to remove the elastic tension on the intact surface, which

would prevent our converting the natural curve of the surface required, into an artificial plane. After hardening the specimen, a microtome will furnish the desired sections containing the full length of the fasciculus.

To study the minute histological details of a given region it is desirable to examine fresh specimens, and small segments hardened in chromic acid or Mueller's fluid.

While by the above methods we obtain specimens, which stain sufficiently well, and exhibit the parts in a nearly natural condition of preservation, yet they do not possess that clear demarcation of fasciculi, and nerve nuclei, which is desirable in specimens intended for demonstration to students and beginners. Here histological niceties can be dispensed with, and the combined use of alcohol and bichromate of potash will yield the finest school specimens. The procedure which I have adopted in preparing objects for demonstration in my private courses, has been as follows: The ambitus is immersed in proof spirit for twenty-eight hours, from this it is transferred to a five grain solution of bichromate, on which solution it will float until it has become permeated to its centre by the latter. As aethyl alcohol and the chromic salts when combined give a green color, it is well to pour off the first fluid and replace it by a stronger solution every other day for a week. At the end of six weeks I wash out the specimen in water, until the latter does not take on a yellow tinge, and again submit it to the action of alcohol, the same routine is gone through again, only that now chromic acid in a one per cent. solution takes the place of the bichromate. The specimen is imbedded for cutting, after having been in alcohol for the third time. This procedure yields objects, which, seen with a *low* power, are far more instructive than those obtained from either alcohol or the chromic salts alone.

Whether we employ one or the other of the methods mentioned, a good microtome is essential to the obtaining of complete and even sections of the ambitus.

The best microtome, of which descriptions have been published, is unquestionably that of Gudden. Without any knowledge of his instrument, and in want of a better method than any with which I was at that time acquainted, I in-

dependently devised an apparatus which has worked very satisfactorily, and differs materially from the instrument of Gudden.

It consists primarily of a movable and an immovable portion, the former has the section-knife affixed to it, the latter holds the specimen to be cut. The description falls naturally under two heads:

A. The movable portion, consists of a large thick plate of glass 16" square, two holes bored through it, receive and support, two brass pillars which, when the apparatus is in operation, stand on the lower surface of the glass plate. On its upper surface are two corresponding screws, by means of which the pillars may be tightened or loosened. Both brass pillars are beveled off on the free end at an angle of about 7.5°. It is essential that the angle should be as nearly the same on both pillars as the human hand can make it, for on this depends the equal inclination of the knife, which is fixed to these pillars.

The knife, which must be ground on a perfectly level stone, is about ten inches long, and fixed to the two pillars by screws. The degree of inclination which the knife assumes by virtue of the beveled off extremities of the pillars on which it rests, has been found by experience to be the best for a microtome knife.

B. The part that holds the specimen, consists of an oblong box with appliances for introducing or removing the water, under whose surface the sections are cut. At the floor of this box is a brass plate *whose surface must be perfectly parallel with the upper edges of the sides of the box*.[1] Here there are fixed appliances for attaching three brass cylinders, of a bore respectively of one, two and a half, and five inches. The plate is perforated in the centre for a large screw, with a regular spiral turn. To manipulate this screw whose *milled*

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1 For the accomplishment of this, as well as many other difficult mechanical problems connected with the construction of this instrument, I am indebted to my father. The accuracy of the apparatus is such that when worked on a level table with the knife ground perfectly level, the deviation in a run of ten inches does not exceed one three thousandth of an inch.

head is worked *under* the box, the latter is raised on four legs, thus giving the requisite working room. A spring click under the box is placed, by the side of the milled head, and registers the movement upward of each one twelve hundreth of an inch with an audible click. This enables the operator to know the approximate thickness of his sections; thus two clicks would signify a thickness of one six hundreth; four, one of one three hundreth of an inch, etc. The imbedded specimen is pushed up by the microtome screws through the intervention of an accurately worked cylindrical piston.

The specimens are imbedded in a mold of paraffine and oil (9:1) cast in the respective cylinder. If care is taken to press the upper surface of the paraffine while it is becoming semi-solid, the very undesirable retraction of the cast from the walls of the cylinder will be avoided. Before I struck on this simple manipulation, I, like Forel, used wedge-shaped pieces of wood, or made a second cast to fix the loose paraffine cylinder. With Forel, I recognize and insist on the importance of not cutting any of the paraffine; there should be as little paraffine around the immediate part to be cut, as is consistent with its support during the cutting process.

The box is now filled with water to a height of at least one inch over the upper surface of the specimen. The glass plate is then placed on the upper border of the box with the attached knife downwards, whose edge is directed towards the specimen. It should be remarked here, that the knife does not touch the cylinder which holds the specimen at any point. That such a contact is avoided, is one of the chief excellencies of this apparatus. The cutting is done at one sweep ; where the specimen is very large, I am able to run back with the knife and make a second sweep, without materially rendering the section unequal.

I seldom harden the specimen, as completely as Gudden and his pupils do, but find that a little elasticity does no harm, and is of advantage in the subsequent manipulation of the fragile specimens. If the various fasciculi and fibrillæ should not be distinct in such semi-hardened specimens, the sections, as such, may be submitted to the influence of chromic acid or its salts.

I have found no difference between the action of bichromate of ammonia and bichromate of potash. I stain the specimens in dilute carmine (rendered perfectly neutral by exposure to the air, then repeatedly filtered), often leaving them for a week or more in this fluid. Hæmatoxylin in alum, is as applicable as carmine to the staining of a large series of sections. I must confess, that I have seen nothing in chloride of gold, osmium or silver salts which would lead me to prefer them to carmine and hæmatoxylin; on the contrary, I suspect them, like all metallic salts, of causing false appearances, by precipitation of the oxides of the respective metals.

I need not enter into the well known details of mounting the sections, which is done in Canada balsam, after preliminary de-hydrization by alcohol, and rendering the cuts transparent with ol. caryophyllæ.

The brains of smaller animals may be stained as a whole, previous to cutting in a manner similar to that employed on embryos. I usually add a neutral, and dilute solution of carmine to the hardening fluid, after the brain has been subjected to the action of the latter *alone* for a week or two. Such specimens may be cut with turpentine, and *mounted as cut.*

The object of having the sliding plate, which carries the knife in the apparatus just described, made of glass, is evident. If made of any other material, it would prevent the operator from watching the cutting process.

Before closing these preliminary remarks, and proceeding to my subject proper, I would offer a few suggestions on the extent to which anatomical data may justify physiological deductions.

Assuming the anatomy of the peripheral nerves as known, it is fair to suppose that the development of the central tubular gray matter, in other words, the nerve nuclei, will rise and sink with the development and functional importance of the corresponding peripheries. This relation is embodied in the projection-theory of Meynert, to whom we owe the greatest advance made in modern times in our field.

It finds one of its most striking confirmations in the almost total absence of the trochlearis, abducens and oculo motor nerve nuclei in the mole, and their relative atrophy which I have found in the bat.

In so far as an intellectual element plays the part of a higher controlling factor, it does not increase the development of a given area in the central tubular gray matter. Thus the hypoglossal nerve nucleus is not so much more developed in the human species, as compared with the dog, when we consider to what intricate co-ordinations the tongue is subservient in the former, as contrasted with the latter! Here the *same gray matter* is under a more multilocular cortical subjection in the human being, its relations are more complicated, its mass, however, not necessarily larger. The same statements apply to the second category of gray matter, namely, the ganglia, especially the automatic, as the thalamus and corpora quadrigemina. Again, a periphery may remain the same in a large series of animals, and yet the central projection area, may vary exceedingly. This is the case with the cerebellum, which embryologically and morphologically is an excrescence of the auditory nuclei, just as it, physiologically speaking, is the centre for the semi-circular canals. Here, as we rise in the animal scale, the cerebellum does not develop from the simple valvule of gray matter found in the fish, to the massive folia of the arbor vitæ in the primates, because of a higher development of the semicircular canals, but because of the more manifold relations into which it is thrown. Thus the powerful strands of the restiform column, internal division of the pedunculus cerebelli, brachium pontis, brachium conjunctivum, bring it into relation successively with the general sensory periphery, the hemispheres directly, and the subthalamic region, nucleus tegmenti, and so forth. Each fasciculus of nerve fibres necessitates the increase of the gray matter; and thus we have the massive development of the latter in the mammalian cerebellum.

Ganglionic matter rises and sinks in mass with the nerve bundles which terminate in it; and when we are tracing a fasciculus to a gray area which appears adequate to receive all its fibres, and can exclude a further extension of the fasciculus by a reticular breaking-up, we are justified in concluding that the fasciculus in question terminates in that special gray area.

Some of our most valuable conclusions can be drawn from comparative anatomy; from the relative diameter of a fascicu-

lus we can judge of its peripheral relations and often surmise its central termination. The mole, bat, cetacea generally, and the Macropidæ furnish instances of physiological atrophies or hypertrophies, which deserve a careful study. It is important, as Forel suggests, that only such animals should be compared as are zoologically related, for otherwise purely zoological differences may be erroneously classed as physiological. Of this fact Meynert had lost sight; but, barring a few erroneous surmises into which he was led by this, his principle remains unaltered, and is destined, no doubt, to materially advance our knowledge of that anatomical basis on which experimental physiology should rely for its confirmation. So firmly convinced am I of the correctness of this principle, that I do not hesitate to attribute the difference in size existing between the lobi optici of two species of American frogs, to the difference of the size of their eyes. Where the anatomical connections are once clearly established, and comparative anatomy as well as embryological development, support the conclusion, I believe that we may make physiological deductions, without resorting to doubtful experiments or impure pathological cases. That the sources of error, mentioned by Forel, should be eliminated, stands to reason; but the *principle* of the projection theory is not affected thereby, nor can all Forel's objections to Meynert's statements be sustained. If we are to insist on tracing *every* individual fibril to the cell in which it terminates, or by which it is interrupted, we bar all progress in cerebral anatomy.

<div align="center">(TO BE CONTINUED.)</div>

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Art. III.—CASE OF UNILATERAL CEREBELLAR AB-
SCESS AND TUMORS WITHOUT PERSISTENCE
OF SYMPTOMS.   REMARKS ON UNILATER-
AL DISEASE OF THE CEREBELLUM.
OTHER CASES CITED.

By C. H. Hughes, M. D., Late Supt. and Physician Missouri
State Lunatic Asylum.

ASIDE from the comparative rarity of abscess of the cere-
bellum, an especial interest attaches to this case because
of the remarkable improvement which took place in the
patient's symptoms, and the light which a careful study of its
history in connection with the *post mortem* examination
helps to throw upon the still conjectural functions of the cere-
bellum.

During the paroxysms of most severe suffering, there was
inability, without external aid, to entirely control the mus-
cular movements essential to the maintenance of equilibrium.
He could walk when supported at the elbow by a friend, and
minister to his wants in any way in which the arms or hands
serve us, though he was sometimes tremulous in attempting
to convey food to his mouth.   He could feed himself, wash
his face and hands, robe and disrobe, etc.   It was the bal-
ancing power, which failed him.

His gait was not shuffling, nor had he at any time in his
history the slightest sign of motor paralysis.

After he had been a short time under treatment he went,
unaccompanied by any one, about the city, sometimes walking
considerable distances, and getting on and off the street cars
without help.

When we first saw him he had headache, stagger-

ing and vertiginous sensations, some hesitancy in comprehension and speech, a slow, full, regular pulse, and drowsiness, but only the exaggeration of the occipital pain and the pulsation served to locate the trouble in the cerebellum. Clearing the bowels twice daily and restoring the depressed brain circulation by proper medication, dispelled all the symptoms. To such an extent did he improve that he went, free from all pain or other head-symptom, to his home in Bloomfield, Ill., to vote for President, whence he returned, after two days absence, only to die of the extensive and not recently formed abscess and tumors shown in the diagram and brain before you. On the night following the last presidential election, he went down town in the cars to see the returns as they were announced by the different city newspapers, and was much interested but not at all abnormally excited. He had a history of malarial poisoning which led to the administration of ten grains of quinia, and a fortieth of a grain of arsenious acid each morning.

The history thus far given, and as follows, tends to confirm the view that the whole cerebellum is not necessary to perfect voluntary muscular co-ordination, and to excite the reasonable suspicion that the hemispheres, and parts of a single hemisphere may, under certain circumstances, perform a vicarious function. And why, in the wonderful economy of nature, always conservative of vital function and power as we know nature to be, should it not be the case here as it is in the lungs and kidneys, the eyes and ears, and in the hemispheres and probably some of the convolutions of the cerebrum.

Jacob Schoene, in September and October of eighteen hundred and seventy-two, first came under the treatment of Dr. J. H. Hewitt, a reputable and skillful physician of Summerfield, Ills. He then had malarial fever and obtained prompt relief, requiring no further treatment until February 22d, 1873, when the doctor treated him for *neuralgia cerebri* of malarial origin. Schoene suffered more or less from pain until the seventeenth of the following March. He was prescribed for twice in the succeeding April, and on the first, fourth, eighth and tenth of May and the thirteenth and fifteenth

of June, for the same trouble. The last prescriptions made by Dr. Hewitt were on September 7th, for an attack of remittent fever, and September 22d for pain in the head. He had been treated also by a homeopathist of this city. He came under my observation October 31st, 1876, and remained with me, except the two days he was gone to vote, until he died, on the thirteenth of the following November.

The following is what we recorded from observation and his wife's statement of his history at the time we began to treat him.

He has a sense of fullness in the head, headaches daily, with intensified pain and throbbing in the occipital region, especially severe in the morning after breakfast. He has a ravenous appetite; vomits often, especially after eating, and has dizzy spells.

His wife thinks he is somewhat different from what he used to be. She says: " He talks childish a good deal," and he is obstinately constipated.

Before the headaches came on he would sometimes sleep twenty-four hours without waking. His pulse is now sixty beats a minute, regular and full. When attempting to walk, he often staggers as though he were drunk.

He sometimes hesitates for words to express his ideas, but not enough to be called aphasic.

Three weeks before coming under my treatment, he was much out of his head. He became wild and delirious, and engaged in an imaginary fight with his wife and boy, taking down his gun from over the door to shoot them, saying he must defend himself. He had but a confused remembrance of the fact afterwards. He complains of a sound as of hissing steam in his ears.

The patient was born Feb. 6th, 1835. He had been twice married, and by his first wife had several healthy sons.

His sexual appetite was neither absent nor inordinate, so far as we could discover. His mind was clear up to the hour of his death, and a few hours before that event he walked, though somewhat clumsily, about his room. A few minutes before he died he sat up in bed, clasping his hands to his head and crying out with intense pain. Until the last agony, we had

always relieved him with applications of sulphuric ether to the top and back part of the head. He became comatose without convulsive or other premonition, and fell back on his pillow and in a few moments expired.

The superficial wall of the abscess had probably suddenly given way. On removing the cerebellum, pus and serum escaped through a small opening in the membrane not caused by laceration or scalpel puncture.
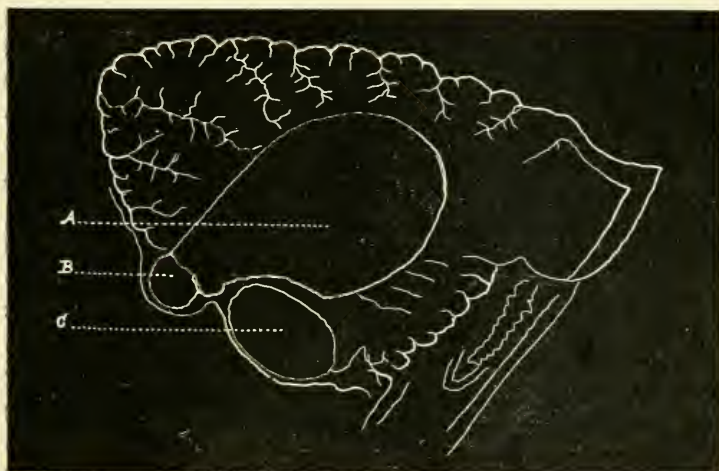
A, Abscess. *B*, Cyst containing serum. *C*, Organized apoplectic clot.

The abscess, as you see, occupies the lower half of the left hemisphere of the cerebellum, extending forwards and upwards, so as to obliterate all traces of the corpus dentatum, and backward and downward, so as to communicate with an apoplectic cell, about the size of a hazel-nut, filled with serum.

This cell extended from the surface through the arbor vitæ arrangement, and opened into the abscess.

The cavity of the abscess was immediately above and contiguous to the organized apoplectic cyst, located just beneath the arachnoid membrane, and occupying the striated structure at the extreme posterior inferior part of the left cerebellar hemisphere, and just within the median line.

This organized blood-clot, though now a little shrunken

from long immersion in alcohol, was about the size and shape of a butter-bean.

The apoplectic products did not invade the right hemisphere. The abscess did not implicate any part nearer the middle of the tuber annulare than one and a quarter inches, and of course did implicate the crus cerebelli

The cavity of the abscess was large enough to envelop a large sized almond, and was filled with pus.

A careful examination revealed no lesion of the cerebrum.

The weight of the brain, including the pons varolii, medulla oblongata and membranes, was forty-eight ounces and a half. The weight of the cerebellum, medulla and pons, after evacuating the abscess and cell of their pus and serum, was four and one half ounces.

The opposite cerebellar hemisphere appeared neither congested nor in any other manner diseased.

In the ninety-three cases of disease of the cerebellum collected by Andral, occur nine examples of morbid implication of a single hemisphere, in which no disturbance of motion was noted.

Of these nine cases, one was an *apoplectic clot in the right lobe, with a history of apoplexy two years before death, but without any lack of co-ordination or paralysis.*

Another consisted of five small tubercles in one hemisphere.

Another was a tuberculous mass, the size of a hazel-nut ; another was that of a cyst of similar size.

Referring to these cases, Dr. Austin Flint, Jr., says :[1]

"They do not present sufficient destruction of the cerebellar substance to lead us to expect any disorder of the movements."

Discussing the remaining five cases,[2] among which was an *abscess involving one of the lateral lobes,* the same writer notes the fact, that *in animals recovery of co-ordinating power* takes place *when half of the cerebellum has been removed ;* and by way of explanation, makes a statement quite *apropos* to our case; viz., "the abscesses were probably of slow development, and if they did not destroy a sufficiently large propor-

1. Human Phys. Ed. 1875, p. 714.
2. Op. Cit.

tion of the cerebellum to influence the co-ordinating power permanently, it is not probable that the functions of this organ would be at all affected, *as there would be no shock, such as occurs in the sudden removal of substance by an operation;*[1] and he might have added there would not be so great circulatory disturbance throughout the cerebellum in consequence of a slowly forming abscess. There certainly could not have been in the case we present.

Dr. Flint also cites a case from Bouvier, in which there was an extensive cavity in the two outer thirds of the left lobe of the cerebellum, containing several tablespoonfuls of pus, though during the patient's life no symptoms led to the suspicion of its existence.

This extensive lesion, in Dr. Flint's opinion, was not sufficient to necessarily disturb co-ordination, and referring to two of Larrey's cases, one being an abscess pervading the whole substance of the right hemisphere, he thinks "there was *not enough* injury, judging from the results of experiments on animals, to necessarily influence the power of co-ordination."[2]

In the case of Schoene it can not be maintained that the course of treatment pursued had any possible restorative influence on the cyst, the organized clot or the abcess. The symptoms were plainly attributable to the disturbed cerebral and cerebellar circulation, for its restoration dissipated for a time all evidences of disease.

This and the other recorded examples of unilateral cerebellar disease, without corresponding physiological disturbance persisting, compel us to concede to the opposite sides of the cerebellum, and perhaps to other portions within the same hemisphere, under gradually invading disease, the probability of vicarious power.

The treatment adopted being unknown twenty years ago, suggests the reasonable presumption that the results in some of Andral's cases might have been different had the power of the bromides and other agents in regulating and controlling intra-cerebral capillary states, been then as well understood as now.

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1. Op. Cit. p. 714.
2. Op. cit., pp. 717 and 718.

One of Flint's cases[1] taken from Vulpian was somewhat similar to ours, though not so extensive.

There was softening the size of a hazel-nut in one of the cerebellar hemispheres, and the corpus dentatum was entirely destroyed. " The woman walked well, but vacillated slightly, without, however, a tendency to fall." A slightly drunken person might do the same. Restore the cerebral circulation and all unsteadiness departs.

A case no less interesting than the one we present, "involving no disturbance of motion or locomotion, except such as would come from debility," occurred some years since in the practice of Prof. Jno. T. Hodgen, who kindly permits me to here produce it.

The case is given as transcribed from the doctor's case book by his associate, Dr. Henry T. Madd.

The patient, a physician of good constitution, robust form, aged forty-one years, was, on 4th of February, 1873, attacked with pain in the left side of head and temple ; also, in back and limbs. He was sick at his stomach and thought he had small-pox. February 6th and 7th he was out attending to business. On the eighth of February he suffered with severe pain in left temple and mastoid region. About the 1st of March dulness of hearing was apparent in the left ear, and paralysis of portio-dura of left side supervened. During latter part of February he had pleuro-pneumonia, first on one side and then on the other—the attack extending into latter part of March. During this time he suffered severely with pain in his chest; pain worse in afternoon.

Patient had also periosteal inflammation of the left leg, followed by nodes on the tibia; rheumatic pains in his shoulder also; took iodide of potash, and nodes disappeared and paralysis improved.

In May patient was able to be about the city—went to country for his health, improved rapidly, and returned to the city, but losing strength, he again went to the country on June 30th, 1873, at which time he was suffering from sick stomach, feebleness, lack of appetite and was jaundiced.

Pain again returned in his temple; deafness recurred and he

1. Op. cit.

was seized with persistent vomiting; double vision, more marked when the rod was held obliquely before him, was present. He could not lie for an instant on his left side without vomiting, but if kept on his right side could retain some food. He was nourished by injections, but rapidly emaciated. Intellect remained clear, except occasional delirium a few days before death, which occurred August 7th, 1873. Six hours before death the skin became very red and hot.

No history of any prior disease. The patient had suffered slightly from indigestion, and had had hæmorrhoids.

P. M. August 10th, 1873. Brain healthy, except pneumogastric lobule of left side of cerebellum, which was occupied by abscess, holding about 1½ drachms of green tenacious pus. There was partial thickening of the basilar artery.

Up to those memorable days in March and April of 1822, when the then young experimental observer, M. Flourens, of whom and from whom the physiological and medical world have since heard so much, submitted to the Royal Academy of France his celebrated memoir "On the Determination of the Properties of the Nervous System, or Physical Researches on Irritability and Sensibility," no one had yet supposed, said the most eminent men of that day,[1] "that the cerebellum was in any manner the balancer, the regulator of the locomotive movements of the animal," though Rolando, as early as 1809, had timidly ventured the conjecture, based upon experiments in some respects resembling those of Flourens, that the cerebellum is in some way connected with the power of locomotion.

Notwithstanding the various explanations which the phenomena, observed by Flourens, have elicited from Foville, who reasoned that the cerebellum is the central point of convergence of the sensations, and Sir Charles Bell, who sought to harmonize all the facts with his doctrine of a muscular sense, to the more recent conclusions of M. Onimus, that the cerebellum serves more for preserving the equilibrium, than for the co-ordination of movements, and to the undoubtedly accurate experiments in physiology which give to the semi-

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1. Report of Portal, the Count Bertollet, Dumeril, Pinel and the Baron Cuvier on the Memoir, to the Academy, July 22d, 1822.

circular canals of the internal ear and to the anterior white columns of the spinal cord, at least, an auxiliary function in maintaining equilibrium, the observations of Flourens have been verified by all succeeding physiologists; though Flourens was undoubtedly in error, as one of his cotempories[1] believed, in regarding the cerebellum as the co-ordinator of *all* the movements called voluntary.

M. Onimus, while insisting that the cerebellum alone maintains equilibrium, also contends, as the result of his late very elaborate experiments, that for co-ordinate movements, the conjoined action of the encephalic isthmus and cerebellum is essential.

We are not aware of any physiologist having yet hinted at the probable possession of the power of vicarious function residing in the hemispheres of the cerebellum, in the same manner as has been asserted for the hemispheres of the cerebrum.

A concession, closely approximating such an admission is found in the concession, which facts of physiological and pathological observation have extorted from all observers, that a limited portion of the cerebellum is capable of performing the function of the whole organ. That within the cerebellum reside motor centres capable of having their power gradually increased, so as to perform twice or thrice their usual function.

"After extirpation of even one-half or two-thirds of the cerebellum, the disturbances in co-ordination immediately following the operation may disappear, and the animal may entirely recover, without any regeneration of the extirpated nerve substance."[2] This is a concession by physiology which medical observation also verifies, of the vicarious function of the hemispheres, though it may yet have to be extended a little up and down the cerebro-spinal axis, in order to harmonize with physiological and pathological revelations.

The fact is, that man's organism is pretty much a dual machine, joined at the median line.

After a careful consideration of Andral's staggering analysis of ninety-three observations and a dozen other more recent

1. M. Bouillaud.
2. Austin Flint's Human Phys. Ed. 1876, p. 711.

cases, Dr. Flint[1] concludes that, "when the disorganization of the nerve tissue is slow and gradual there may never be any disorder of the movements."

The deduction of the physiologist from the facts furnished by pathology are correct, but how are we to explain other cases which, from time to time, obtrude upon our attention ?

Take the celebrated case noted by Delamere, and quoted by Andral from Lallemand, where the patient, M. Gueren, forty years of age, though for a year he had vertigo and vomiting with staggering, and was often near falling forward, had, at death, really no cerebellum at all. "It had become entirely transformed into a sac filled with pus," and that of Alexandra Labross, reported in 1831 by Combette, who, though she walked in an uncertain manner, had neither cerebellum nor pons Varolii. Petiet's patient, reported in 1826, though he rose with difficulty from his seat, walking at first with lateral movements, and finally, from before, backwards, and could only walk in this way to an adjoining room in the ward, a distance of about six feet, "was found to have his cerebellum entirely destroyed, its tissue being broken down into a sort of *bouillie*."

These exceptional cases appear to overthrow the views here maintained, but the presumption is that the lesion was not so extensive when progression and equilibration were possible, or in explanation we may assume that other parts of the cerebro-spinal axis may, under the gradual demand made upon them, have come to take on, to a limited extent, the co-ordinating power of the cerebellum.

The anatomical connections of the cerebellum would seem to point to community of action with portions of the cerebrum and cord, and while acting in their truly physiological condition they have special and defined functions, it would not be strange if, under circumstances of disability, their functions might be extended. The neighboring parts doing gradually the work of the whole, just as three fingers after a time learn to do the work of four and, to some extent, the dexterity of the fingers may, under certain circumstances, be acquired by the toes.

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1. Op. cit. p. 711.

In estimating functional capability, the conservatism of nature, under stress of disease in vital parts, should not be overlooked.

There are obvious difficulties in the way of precisely determining function by physiological experiment, chief among them being the circulatory disturbances throughout the whole organ, and sympathetically in the contiguous cord and cerebrum.

The slicing process of Flourens appeared to demonstrate "that the integrity of the cerebellum is necessary to the regularity of the locomotive movements," while pathological observations have so modified this conclusion as to establish the absolute necessity for a very little, if any, of the organ as at all times an indispensable essential to regular movements.

Notwithstanding all we have learned since the beginning of the present century from Rolando, Flourens, Magendie and their cotemporaries, down to Onimus and Brown-Séquard, and not omitting the investigations of Ferrier, Hitzig and others on the functions of the cerebral convolutions which indirectly, we think, throw some light on the subject, yet something remains to be discovered concerning the cerebellum, and a part of that something consists in a satisfactory explanation of the facts set forth in this paper.

The facts thus far collected may not be deemed sufficient to sustain the view that the cerebellar hemispheres are capable of a dual action, and under certain circumstances of vicarious function, but all the facts harmonize with the conjecture, and they are equally as numerous as those which support a similar view respecting the hemispheres of the cerebrum.

Art. IV.—A CONTRIBUTION TO OUR KNOWLEDGE
OF PHYTOLACCA DECANDRA, AND
OF GRINDELIA ROBUSTA.

By Prof. Roberts Bartholow, M. D.

I.—PHYTOLACCA.

EVERYBODY is familiar with the appearance of the despised Poke, but few are aware of its really valuable properties. No systematic study has hitherto been made of its physiological actions. The accidental administration of poke-berries, in sufficient quantity to cause serious symptoms, has furnished information of great value. I have studied these cases, to compare the symptoms produced in animals, with those which have occurred in man.

It has long been known that poke is a nauseant and emetic, and that vertigo, convulsions and coma have been observed in cases of poisoning by this substance. Referring to these symptoms, the U. S. Dispensatory remarks that it must possess " unsuspected powers " as a " spinal irritant." The accounts which have been published of cases of accidental poisoning, show that poke affects the functions of both brain and spinal cord.

My experiments were made on frogs and rabbits. The most characteristic reactions are produced in these animals ; and as these reactions correspond to the symptoms observed in man, we may accept the results without hesitation.

Phytolacca is a most depressing and nauseating emetic, and has been proposed as a substitute for ipecacuanha ; but the suggestion has never been acted on, because the action is so intensely disagreeable. Both in frogs and rabbits the nauseant effect is very obvious. When the frog is nauseated by it, the under-jaw drops, and he presents the most ludicrous appearance of intense disgust. Phytolacca is a specific emetic—

that is, whether taken into the stomach or thrown under the skin, it causes nausea and vomiting. The gastro-intestinal secretions are increased by phytolacca. Diffusion into the blood takes place with facility ; but in what form, and what changes, if any, are induced by it in the composition of the blood, are quite unknown. It slows the cardiac movements and lowers the arterial tension. The action of the heart continues after the entire cessation of the respiratory movements. This can be easily seen as follows: Paralyze a frog with it until all external signs of life have ceased, when, on opening the chest, the heart is still seen in action. But it is a heart poison as well as a respiratory poison, although it affects the respiration more than the heart. Phytolacca affects warm-blooded animals similarly. When paralytic symptoms are produced in a rabbit, pass into the heart, through the chest-wall a long, strong needle having a slender rod of pine-wood attached. Every movement of the heart is then represented to the eye by the movement of the rod. When all agitation has subsided, a metronome is adjusted to beat in unison with the rod. It is obvious that any subsequent changes in the cardiac movements will rupture the synchronism between the rod and the beat of the metronome. I have made a number of observations on the action of the frog's heart, with the metronome, of which the following experiment is an example :

Injected a large frog, intended for the table, with 3j of the fluid extract. The first effect was sluggishness in all the voluntary movements, with increasing paresis. When a fore extremity was doubled up under the chest, he made no attempt to remove it. The paresis increased, so that he could not jump, and the hind extremities were partly extended. The lower jaw dropped, and the mouth remained widely open. When now, an extremity was sharply pinched, he manifested no sign of pain. In an hour there was complete muscular relaxation, and all external signs of life had ceased. On opening the chest, by dividing the sternum, the heart was found to be in action feebly, beating eighteen per minute. Adjusting the metronome to strike at the same rate, the action of the heart was ascertained to be slowing and its pulsations feebler.

At the end of two hours the effect of the dose had declined

considerably ; the frog made violent efforts to turn over, and at last succeeded. The action of the heart then rose to 30. When he had turned over, and assumed the usual sitting posture, he looked quite natural notwithstanding the opening in the chest.

The experiment just narrated apparently demonstrated that phytolacca is a paralyzer of motility and sensibility. Exact observation on the sciatic nerve, the limb being isolated and separated entirely from the body, except by the nerve, showed that the sensibility of the sensory and motor nerves to their appropriate stimuli, was unaffected.

The paralysis of motion and of sensation is therefore due to an impression made by phytolacca on the spinal cord.

In a young rabbit weighing 46 ounces, a drachm of fluid extract of phytolaccoa administered subcutaneously, caused at first excitement followed by stupor, weakness of the extremities, trembling of all the voluntary muscles, and especially trembling of the ears ; increasing paralysis, both motor and sensory, contracted pupils, convulsions, death ensuing from failure of the respiration. A number of trials on rabbits produced essentially the same results. I need not, therefore, occupy time in detailing them.

The therapeutical applications of phytolacca are, as yet, strictly empirical. A good deal of clinical experience has been accumulated—most of it to be found in the U. S. Dispensatory—showing that poke possesses properties to which the old and meaningless term, *alterative,* has been applied. The berries, usually steeped in whiskey, have been employed with success in the treatment of *chronic rheumatism.* Whether berries or whiskey has been most influential in bringing about a cure, has not yet been determined. So many instances of good effects following its use in chronic rheumatism, have now been reported, that it must be regarded as having some special power over this affection. Cases of *constitutional syphilis* have, it is said, been arrested by the use of phytolacca. (Resources of the Southern Fields and Forests, p. 402.) Locally applied in the form of decoction, *parasitic skin diseases,* unhealthy *wounds, ulcers,* and even *cancer* have been reported cured.

While many of these alleged cures are no doubt apocryphal,

there must be some fact as a basis for pretensions so positive. As respects the supposed alterative powers of poke, they may be regarded as similar to those of guaiac. Indeed, all remedies which increase the secretions of the intestinal canal, kidneys and skin promote elimination of all foreign and excrementitious materials which seek an outlet through these channels. The good results which have followed the application of a decoction of poke to unhealthy wounds, may well be admitted as genuine, for decoctions of other astringents are used with advantage in such cases.

Thus far, it must be admitted that the empirical knowledge of the uses of phytolacca is not extensive, and doubtless far from accurate. There is, however, another application of this agent, which, if sustained by future experience, must be regarded as extremely important. I refer to its use in *mastitis*, inflammation of the mamma. Dr. Tidd, not long since, published in *The Clinic*, a paper showing the utility of phytolacca in this disease—showing, indeed, that it has the remarkable power to prevent suppuration. Others have published similar experiences, and I am favored with a verbal communication from my colleague, Prof. Palmer, in which he expressed a strong conviction that phytolacca really does possess this remarkable property. If it shall be settled that mastitis may be arrested, and suppuration prevented by the use of this remedy, it is plain that we have in poke a drug of very unusual power. I do not conceal from myself the great difficulty of discriminating between the *post hoc* and the *propter hoc* in such a malady as an inflamed breast proceeding to suppuration. Numerous observations must be made, and under conditions that admit of no misconception, before this question can be settled.

II.—GRINDELIA.

This remedy comes to us from California. Its botanical name is Grindelia robusta, and the leaves, stems and flowers are the parts employed in preparing pharmaceutical products. The most eligible preparation is the fluid extract.

Grindelia has not, as yet, been subjected to thorough chemical investigation. Partial examination has shown that it con-

tains an alkaloid, having basic properties. The oleo-resin is probably an important constituent also.

As considerable demand has sprung up, already some sophistication has been practised, other members of the same family having been used for this purpose.

My experiments were made on frogs and rabbits, chiefly, and I supplemented these by some observations on myself to a limited extent. Besides these sources of information, I have consulted all the papers which have been written in regard to its effects on certain diseases, and have prescribed it in maladies, for the relief of which it is so highly commended by our Californian colleagues.

Grindelia is by no means actively toxic, since a half-ounce of the fluid extract was required to kill a small rabbit. The taste is bitter, rather acrid, and is decidedly persistent. In the stomach, in any considerable quantity, it gives rise to a sensation of warmth, which diffuses throughout the system. The action of the heart becomes more energetic, the tonus of the vascular system rises, and increased secretion, salivary and cutaneous, takes place. The increased blood pressure is not long maintained, but descends a little; the respiratory movements, at first hastened a little, become slower and fuller.

The most important action of grindelia, unquestionably, is its influence on the cerebrum. The first impression is to increase the activity of the cerebrum, but this stage of excitation is soon succeeded by a condition of mental calm, lapsing into sleep, but not into coma. During this stage of sopor the pupils are somewhat dilated, the respirations becomes slow, but are deeper, and the rhythm continues normal. In rabbits the condition of sopor is accompanied by a marked degree of muscular paresis, beginning in the hind extremities. The contractility of the muscles, and the irritability of the nerves, both motor and sensory, remain unimpaired, so that the paralytic symptoms are due to the action of the agent on the brain or spinal cord, or on both.

Notwithstanding grindelia produces distinct paralytic effects, it exalts the reflex functions of the spinal cord in a remarkable manner. When the frog lies with all the limbs extended, and deprived of all power of voluntary movements, a gentle tap on

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any part of the body is followed by rapid, tetanic contractions
of all the voluntary muscles, beginning in the upper extremi-
ties, next extending to the spinal muscles, and reaching in a
second of time the muscles of the leg.   These tetanic spasms
produced by peripheric irritation, seem to me to be of the
same character as those which occur in frogs after some hours
of complete muscular resolution, caused by lethal doses of
atropia.

Death ensues by paralysis of the muscles of respiration, and
cavities of the heart (frogs) are distended with blood.   The
elimination of grindelia takes place through the kidneys
chiefly.   The oleo-resin is more or less irritating to these
organs ; more frequent irritation results, and the amount of
urine discharged is increased above the normal.   As respects
the action of grindelia on the kidneys, it is comparable to that
produced by buchu, uva ursi, eucalyptus, and other agents
whose activity is due to the presence of an oleo-resin.   A
peculiar odor is imparted to the urine by the oleo-resin of
grindelia.

The therapeutical applications of grindelia might be de-
duced from a study of its physiological actions.   The com-
bination of an oleo-resin with a narcotic principle, is one
especially advantageous in the treatment of the respiratory
neuroses.   The same result has been reached empirically.   Our
Californian colleagues have used grindelia with success in the
treatment of the asthmatic paroxysm.   If I may form an
opinion from an experience with a few cases, I must admit
that the California reports are well founded.   In several cases
of asthma, in cough maintained by habit, and in spasmodic
cough from reflex causes, it has afforded decided relief.   Like
other so-called expectorants and diuretics, which contain an
oleo-resin, grindelia is serviceable in chronic bronchitis, and
bronchorrhœa, and in catarrhal states of the genito-urinary
mucous membrane.

The effects which grindelia produces on the cerebrum, as a
hypnotic and anodyne, are strongly suggestive of a sphere of
usefulness in the future.   How far the anodyne and hypnotic
action, so conspicuous in animals, will occur in man, remains
to be seen.

ART. V.—ON MIND.

By J. L. TEED, M. D., Kansas City, Mo.

ENQUIRIES into the nature of mind have attracted the attention of thinking men in all ages of which we have any record. The phenomena of consciousness, of reflection, of memory, and of volition, differ in their manifestations so immensely from all the phenomena of surrounding nature, that it would be indeed strange if they had not attracted this attention; while the apparent difficulties of the problem, the scanty knowledge of the phenomena of nature, the generally imperfect methods of investigation, and the dominance of certain hypotheses, have caused most enquirers to make the facts observed square with the preconceived ideas, rather than to simply observe the facts, and make their preconceived ideas harmonize with them.

One source of error has been commingling religion with science. The ban of materialism, of infidelity, or of atheism has for ages been unsparingly pronounced against those who have by patient study, and the examination of natural phenomena, attempted to solve the mystery of mind. It is doubtless true that materialists, infidels and atheists have been attracted by the intricacy of the subject, and have fondly but vainly hoped to find its solution; but they have also investigated the problems of mathematics, as well as those of mind, and the mere fact, that the former have been investigated by them, has been no deterrent to other investigations, nor need we be deterred on such grounds from scientific investigations into the latter. Yet it does seem strange, that while all the other works of the Creator are considered objects of legitimate study, this should be placed beyond the pale of legitimate enquiry.

Another source of error lies in the use of the terms mind, soul, and spirit, as synonymous; for as we have no means by which we can investigate the two latter, by such an idea we prevent any investigation of the former; no greater obstacle lies in the way of our accomplishing any object, than the firm conviction that such a result is impossible.

A kindred source of error is the idea that the mind properly so called is an attribute of the soul and not of the body; and this is largely prevalent at the present day. Observation daily teaches that the lower animals possess many of those qualities the aggregate of which make up that complex which we call mind; and as it has not been possible to allow to these lower animals the possession of souls, the same phenomena when manifested by them have been termed instinct, when manifested by man, mind; and thus still more confusion has been introduced into the subject.

The mode of investigation followed has been a frequent source of error. No one is likely to be attracted by this subject, until he has reached years of mature thought. He then commences to analyze his own thoughts, his own feelings. He finds he can at will direct his thoughts into this or that channel; he can recall events, persons, and things by memory; he can learn one subject after another by application; he perceives external objects; he forms abstract ideas, or rather concrete ideas with such faint boundary lines as to appear to him abstract; he measures time and space, and the size and composition of distant worlds; and he naturally enquires, how and by what means are these things done? Instead of attempting the problem in its earliest, he tries to grasp it in its most fully developed and expanded state. Is it any wonder that he fails? Look at the works of art scattered throughout the length and breadth of the world! Examine the treasures of science in the cumulated libraries of nations, all the products of mighty mind; and is it any wonder that the investigation of this power in its most developed form should be as much beyond his comprehension, as it would be impossible for him to produce all these results?

As one observer fails by introspection to solve the problem, another attempts its solution by analyzing the character of

others. He studies biography, history, and perhaps also works of fiction devoted to the portrayment of character. He divides the phenomena of mind into fractional parts, and endeavors to assign to each its share in the elucidation of the subject. But he is studying results of mental action, not mind; he may learn the relation between external conditions and states of mind, but he gains no knowledge of the nature of mind itself.

Others again have sought to investigate the subject by observing it in its more simple or lower forms, as manifested by the lower animals; but these investigations have only served hitherto to render the subject more obscure, from the difficulty of determining where mind begins, and from the absence of any term whereby we can express those conditions of activity, in which the development of mind has not been reached, but which lie, as it were, in the border-land between the mental and the non-mental.

The study of mind in those unfortunates who are the subjects of mental alienation, has hitherto thrown no light on the subject; nor, indeed, should we expect that disordered manifestations of mind could be more easily explained than are its healthy and normal manifestations. It is a general rule, that the healthy condition must be understood before the diseased condition can be comprehended, and the subject of mind forms no exception to this rule. Yet it is in this direction that we may expect the greatest advances will be made in the future; and the explorer will come, who shall discover the connection between diseased states and abnormal mental action, between healthy states and normal mental action.

But the greatest obstacle to the comprehension of mind has hitherto lain in the extremely slight acquaintance previously had with the general phenomena of nature. In fact, it is only since attention has been directed to the study of force, as well as to the study of matter, that the clue to a correct idea of the phenomena of mind has been placed within our reach; while by the application of the phenomena of force the problem receives a simple solution which is fitted to all its conditions, involves no contradictions, raises no conflict with religion, and promotes the highest object of humanity, our endeavors to

discover how, by suitable treatment, we may restore the sanity of health to a mind diseased.

Many are the scenes of sorrow and suffering on which the sun shines in his daily course, but the saddest of all are those groups of poor unfortunates collected within the walls of our lunatic asylums. The fond father, the affectionate wife, the loving child, the accomplished ornament of society, all stripped of their attractions, and transformed by a terrible disease into objects of dread, requiring incarceration for the general protection of life and property. And surely, any investigation, however humble it may be, as is this one now before us, deserves an examination, if by any means it may be found to throw the least light on the nature of the malady with which they are affected, or to indicate a direction for its pathological or its clinical investigation.

The term mind may be used correctly in two senses; one potential, the other actual. Mind, in the potential sense, indicates the capability of forming an idea; in the actual sense, the idea formed. It may be also used in a general or comprehensive sense to mean an aggregation of ideas.

The fundamental facts in the phenomena of mind are therefore, first, an idea; second, the formation of an idea. If, therefore, we can arrive at a scientific comprehension of one idea, and can trace the process of its formation, through its various stages, we gain an insight into all mental operations, and thus reduce this subject to the ordinary condition of our present knowledge of all abstract subjects.

An idea is not the thing itself, but it is a representation of the thing; it therefore requires for its formation the previous existence of the thing itself of which it is the representation; this representation is accomplished within us, and is the result of the dynamic transmission to us of the qualities possessed by the object; it may be defined as the perceived image of any object; the term image being used to express the same relation to the other senses as in the limited sense it has to vision.

In order to trace the formation of an idea, it is best to study the process in a being in whom ideation (or mind) exists potentially but not actually; in one whose mind is as yet undeveloped, who, not yet possessed of ideas, is, however, capable

of forming them; and such a being we find in the human infant.

When born, the infant has no ideas, no knowledge; no impressions have been made on its sensory organs, for these avenues to the brain have all been closed, or placed beyond the reach of such impressions. Directly after birth, however, it comes in contact with external agencies, of which the cold atmosphere is one. The consequent rapid conduction of heat from its general surface produces the impression of cold; this produces contraction of the muscles of respiration, a breath and a cry follow, and this cry is the result of the first idea.

Although cold is a negative, and therefore does not actually exist, yet in common language we use it as a positive, and speak of its effects. The effects of cold and heat on the extremities of sensory nerves are similar to one another, only in the first case the current of the molecular motion produced in the neurine is from within outwards, in the second case from without inwards; according as heat is either given off or absorbed.

This molecular motion thus produced in the infant is transmitted through the neurine of its nerve trunks to certain collections of nerve cells reaching through the entire length of its spinal cord to the basal ganglia in its brain; it is thence transmitted to the cells of its cerebral hemispheres, and in these cells the impression becomes a perception or idea, which then, as all through life, is of the unpleasant kind, and produces those muscular contractions which give rise to ejaculatory expressions and jactitations of the limbs. If the infant be now wrapped in flannel, and lain in a warm place, it soon ceases its crying and lies quiet; the molecular motion is succeeded by molecular rest, and a feeling of well being is produced.

The first idea, therefore, formed in the infant is the difference between cold and heat, and with it the emotional difference between comfort and discomfort; and it manifests its cognizance of these conditions by its positive cries or its negative quiet, the only means it has of expressing its feelings.

After some time the infants' eyes become accustomed to the stimulus of sight, and it begins to look at surrounding objects.

The rays of light reflected from the surfaces of these bodies fall on its retina, and their motion becomes the source of motion in the nervous expansion of this membrane. The motion of the ray of light, which motion we will designate A, infringing on the retina the motion in which B becomes the impression, thence the motion is continued to the corpora quadrigemina, and there as C becomes the sensation; from this point the motion is transmitted to the angular gyrus, in which as D it becomes perception of vision or light, which is attended with consciousness, and is, therefore, the idea of the thing seen. The first and the last terms of the series A and D are therefore similars, the difference between them being due to the matter by or through which the motion is manifested, and not to any change in the intrinsic nature of the force itself; and thus the idea of vision or sight *is* motion in the neurine of the angular gyrus.

We may trace the formation of ideas in the brain through each avenue of the five senses, and we shall find the process just the same in each. An external motion is arrested by the extremity of a sensory nerve, in which it produces a similar motion—that is, a motion vibratory in its character; this motion is then transmitted or continued along the neurine of the nerve trunk to the vesicular cells in which it terminates; the motion is thence transmitted to more distant, or as we say, higher centres, and thus reaching the acme of the trajectory becomes the phenomenon of mind.

But each one of the senses has its own special line of neurine from the periphery to the centre, and these different tracts of neurine are not adapted for the reception, transmission, or perception of any but one particular form or mode of motion. Thus the angular gyrus can interpret the motion of the ray of light into sight but no other motion; the superior temporosphenoidal gyrus can interpret the motion of sound into hearing but no other, and so on with the other senses. And just as in-going currents, giving rise to perceptions, are found to have definite tracts, each for each, so the out-going currents, giving rise to volitional motions, are also found to have definite tracts, each for each; and thus we may conclude that an idea, of whatever kind it may be, is motion in a correspond-

ing group of cortical nerve cells in the hemisphere of the brain.

The cells of the brain centers are of three kinds—sensory, ideational and motor; therefore the only proof of the possession of mind is the presence of ideational nerve cells, for as cross lines of communication are frequently passing from the sensory to the motor tracts, movement in response to sensation may be of the automatic or reflex kind; and this may be the highest point reached in development by animals low in the scale of being.

Let us now briefly examine the sources of ideas.

What is sight? A vibratory motion among the particles of the luminous body.

What is sound? A vibratory motion among the particles of the sonorous body.

What is heat? A vibratory motion among the particles of the candescent body.

Taste, smell and touch are in the same way special modes or forms of motion. Abolish motion and there is no longer any manifestation of force of any kind, nor can there then be any means through which by us can any idea be formed. But as the first transmutation of the external motion constitutes the impression, so the last transmutation of this motion constitutes the idea, and if this last transmutation can not, from any cause, be accomplished, the idea can never be formed.

No idea can be formed except as the result of its antecedent motion. We can no more create a new idea than we could create the material basis of which this idea would be the type, for all our ideas are representative types or images of external things, and they all require a material basis for their formation. The idea of a horse requires the horse ; the idea of love, of hope or of joy, requires the person loving, hopeful or joyful. Of an abstract we can form no idea, for the abstract is absolute; and all our knowledge being relative, the abstract has neither differences nor resemblances; these belong to the concrete, and the concrete always has a material personification.

No idea can be separated from its external motion, whether causative or derivative; we cannot think of an external object, without such object being brought into internal consciousness

or imagery; nor can we think of any movement without such movement being brought into internal imagery. We cannot think of motion, except of a body moving; of rest, except of a body resting; thus again the abstract to be thought of has to be clothed in concrete form.

The transmission of these molecular movements is not always accomplished with the same facility, and thus we find a greater readiness of comprehension in some than in others, and in the same person at different times and in different periods of his existence. And not only is there a difference in the inception of these movements, but the same is found in the re-establishment of these movements, whence arises a difference in the memory of different persons.

The vividness of the idea will bear a direct relation to the intensity of the motion produced in ideational nerve cells; while of course the nature of the idea will depend upon the group of nerve cells thrown into activity.

By an act of volition we call these definite groups of nerve cells into activity, whether sensory, ideational or motor; and so also by act of volition we can repress this activity; and we find this power of volitional ideation in all the forms of animal life possessed of what we term mind. We also find that these conditions of molecular movement are closely connected with other somatic conditions. The timidity of some forms of cardiac disease, the hopefulness of some forms of pulmonary disease, and the melancholy of some forms of gastric and hepatic disease have long been subjects of observation. They are also much influenced by conditions of the blood, whether such take the form of abnormal quantity or of abnormal quality. They are also much influenced by the introduction of certain material substances into the blood, such as haschish, opium, alcohol, tea, coffee, etc., etc. And we also find that they are much influenced by the intrinsic conditions of the nerve cells themselves. Thus, the *mens sana in corpore sano* is no fiction; it is one of those realities, the importance of which is hardly thought of until a disturbance of this function brings it vividly into view. Yet there never was a case of insanity, or of delirium, that did not have its inception in some form of somatic disturbance.

Another fact, now too well established to admit of doubt, is the transmission of neurotic disorder, which often assumes the ideational form, by a parent to the offspring, after a change from sober to drunken habits; those children born during the sober period not being so affected. While another fact equally striking, but which, like the one just mentioned, has never received its due consideration in the investigations on this subject, is the one, that transmitted neurotic disorder may assume either the sensory form or the motor form, as well as the ideational form, or be compounded of all these forms, thus showing the intimate relation existing between the various functions of nerve matter, sensation, ideation and motion.

In offering this explanation of the phenomena of mind, it must be remembered that we are considering them as we find them manifested by living men and living animals. Allowing that there are modes of existence purely spiritual, unclothed with the habiliments of matter, the phenomena, which they exhibit, analogous to those of mind in us and in the lower animals, must rest on a basis adapted to their particular mode of existence, as the phenomena of mind rest on a basis adapted to that particular mode of existence which we possess in common with the lower animals. But those analogous phenomena cannot be literally included under the term mind, unless we also suppose that those modes of existence possess ideational nerve cells, by which such phenomena may be manifested, although figuratively we use this word mind by which to express them, because we have no other word in our language adapted to this purpose.

The peculiar differences manifested by different classes of the lower animals depend doubtless upon certain definite variances in the cortical nerve cells of their brains; and in the same way we may account for those innate variances in character, co-equal in number with the individuals of the human family. But innate character must not be confounded with acquired character. As we found in volition the power to act, so we also found in it the power to restrain from acting. If a man exercise these powers in certain directions he acquires a character for honesty and sobriety; if he refuse to exercise these powers, he acquires a character directly the reverse; and thus we speak

of men as depraved or of no moral worth. Thus the explanation of the phenomena of mind here offered gives no argument in favor either of immorality, or of unaccountability for our actions. Our duties in these directions are left untouched by it. If it should be that the difficulty of so restraining is greater in one than in another, it is no more than we observe in the other mental phenomena, for though one man can exercise his brain better on some subjects than on others, yet by perseverance he can acquire a considerable degree of proficiency on those subjects which he finds the greatest difficulty in acquiring.

Neither does this explanation of the phenomena of mind give any argument in favor of materialism, nor is it opposed in any way to the generally received doctrines of religion. For by separating the mind from all controversies respecting the soul, and restricting its meaning to a function of the body, or of a part of it, we absolutely exclude all religions matter from being involved in the discussion, as well as all questions of a metaphysical character. Although the word mind may be used metaphorically, as is the word heart, such use of a word is never supposed to convey its absolute meaning.

But this explanation brings us into a more humanitarian relation with all forms of insanity and delirium, whether they are presented to us in a medico-legal, or in a purely medical form. It is no more a disgrace to a family to have one of its members insane, than it is to another family to have one of *its* members blind, or deaf, or paralytic. The calamity, it is true, is greater; but that should only more deeply rouse our human sympathies. While if it be true that insanity is on the increase, it only teaches every one more strongly to look well to his habits of life, and to the preservation of his general health.

But the great value of this explanation is in its truth, for all advance in science is chiefly accomplished by correction of error, and the establishment of relations between known facts.

Neurological Correspondence.

Mr. Editor:—It is with pleasure that I am able to forward you, notwithstanding the panic and the exceedingly dry weather, evidences of action of brain tissue which I think will compare favorably with any material forwarded in more lively times.

That ripe scholar and distinguished medical-jurist, Prof. Ray, of Philadelphia, not long since paid us a visit, and left with us a few thoughts upon Testamentary Capacity. This is an important subject, and any writing which will assist the medical and legal professions to justly carry out post mortem wishes and bequests, should be heartily welcomed. This, therefore, will form a fitting supplement to what was forwarded you about one year ago on the same subject.

Dr. Spitzka contributes a valuable essay on the localization of cerebral diseases, which at this present time, when the subject of localization of centers is so extensively discussed, is exceedingly interesting, while our friend, Dr. Kinman, has given us what we might consider the medical phase of the great temperance discussion now sweeping over many portions of our country. But as the lawyers say, the documents will speak for themselves. Therefore, without further ado, we proceed:

The Medico-Legal Society.

On the 13th June last, Prof. Isaac Ray, of Philadelphia, delivered an address before the Medico-Legal Society on Testamentary Capacity, and as this paper is not only intrinsically valuable, but also discusses the mind in health and disease, I am constrained to forward very full notes.

Prof. Ray spoke about as follows:

To the question, what shall become of a man's property after death, the law of our own and of every civilized people,

in recognition of what was deemed by the old jurists as a *jus naturale*, replies that he may dispose of it himself before he dies, under some limitations required by a regard for the rights of others. It only requires that when exercising this right he should be of sound mind, and this leads to the inquiry, what is meant by a sound mind. At first blush this would seem to be a strictly medical question, and so regarding it, the physician considers it as a matter of health or disease, of normal or abnormal condition. The jurist, on the other hand, ignoring entirely the physical element implied in the question, considers it solely as a matter of mental capacity. As in cases that come into litigation both parties urge their views as the proper guide to the ultimate decision, discrepancy and conflict must naturally follow. Much of this diversity of views arose, on both sides, undoubtedly, from a very imperfect knowledge of the subject matter itself, as the slightest examination will show.

From the earliest times, the law has recognized insanity as a disturbing element in human conduct, and allowed it to modify more or less its administration of justice. Therefore, the question of questions on this subject is, how far and in what manner this disturbing influence really extends. To answer this question, resort has been had to two sources of instruction. One is that observation of the insane which any one may make, in the ordinary walks of life. While we admit that much may, possibly, be learned in this way, yet it is no less certain that impressions thus obtained are quite unreliable for any practical purpose. A very few cases only can come under the cognizance of any individual, and the notice taken of them must necessarily be hasty and superficial. The more demonstrative symptoms only will arrest the attention, while the less obvious, though perhaps more significant, may be overlooked altogether. Neither in this way is there any element of progress. No amount of attainment leads to fresh attainment. It lives and dies with him who makes it. Observers more sagacious than Shakespeare and Walter Scott can scarcely be expected. And yet the current ideas respecting insanity are chiefly derived from this source.

The other source of information is that where insanity is

studied as a disease, in the spirit and methods of a scientific inquiry. Here, not a few only, but all its phenomena are observed. Its various phases as presented from day to day, from month to month, and from year to year, in the different grades and stages, are carefully studied with the abundant facilities afforded by the modern hospital for the insane. The study of the physiological and pathological laws by which it is governed, of the ancestral defects in which it originates, and of the adverse influences by which it is developed, throws light on the character of each individual case, without which we can scarcely avoid mistake.

Ideas derived from this quarter have always determined to some extent the rules and practices of the English law, not, however, without great resistance, especially in modern times. The classification of mental diseases, the terms and phrases, found in medical books, were freely adopted into the civil law, with such practical consequences as they would naturally suggest. The psychological relations of the different forms of insanity were but little considered by the ancient physicians, and of course they could furnish the lawyers with little aid in the work respecting it which fell to them. Indeed, it could not have been otherwise, for their means of learning about insanity were hardly better than those possessed by the world in general. Their ignorance of anatomy and physiology led to the crudest theories on the nature of disease, and their observation of its phenomena was directed by a false pathology rather than by any methods of exact and patient inquiry. The only advantage they possessed over others in the study of insanity consisted in the more close and continuous observation of individual cases which their professional duties required. Through the whole mediæval period, and long after the revival of learning, medical writers were content to abide by the notions of Celsus and Galen, and these were all they could offer to aid the lawyer and the law-giver in the administration of justice. By this aid, such as it was, the latter undertook to say how far and in what way insanity modifies the legal consequences of human conduct. To simplify the process, they found it convenient to consider the disease in its different forms, or more correctly speaking, in its different grades of apparent severity. The

first systematic attempt of this kind was made by Lord Coke, who recognized four descriptions of persons as being *non compos mentis*, viz., idiots; those who by sickness, grief, or other accident, have wholly lost their memory and understanding; those called lunatics who have sometimes their understanding and sometimes not; and lastly, those who by their own vicious act have deprived themselves for a time of their memory and understanding, such as drunkards. Rude as this classification is, it has some semblance of correctness, but like the more elaborate rules and tests of more recent times, it labors under the defect of being difficult of application. It affords no help to settle the question whether, in a given case, the person concerned belongs to either of these classes. Regarded as a scientific formulary it is quite defective, because it ignores completely some well recognized forms of mental disease. Many years afterwards, Lord Hale, availing himself of the more advanced medical knowledge of his time, corrected this defect in some degree, and ventured upon some rules for the settlement of the practical question. But with all their defects, and in the face of the better knowledge of our times, the doctrines of these men still govern, in a considerable degree, the decisions of English and American courts.

Unquestionably, they reflected and confirmed the prevalent sentiment which regarded the last will and testament as an instrument too sacred to be easily disturbed. And we know, as a matter of fact, how jealously it was guarded from all assaults, and especially from such as were directed against the mental competence of the testator. Let us now examine the decisions of the courts in the new light shed upon the subject by the progress of medical science during the last two centuries, for within that period have occurred the two great discoveries of the circulation of the blood and of the functions of the nerves. And the same period has witnessed the establishment of multitudes of hospitals for the insane, affording opportunities for the clinical study of the disease and for the observation of the ways, manners, conduct and conversation of the insane, never enjoyed before.

In that form of mental disorder called acute mania, in which the mental movements are continuously strange, wild and inco-

herent—madness without method—the testamentary incompetence of the patient has never been questioned. Indeed, the condition is one in which we should hardly expect any disposition to make a will, and I am not aware that such a one has ever been offered for probate. Lord Coke, you observe, speaks of a description of lunatics who have sometimes understanding and sometimes not. This statement has reference to a phenomenon once supposed to be a very common occurrence—I mean that of lucid intervals, as they were called. So common were they thought to be that in every case of alleged insanity the question of a lucid interval was always raised as a matter of course. In fact, the practice has not yet entirely ceased. The idea is, that in most insane people reason returns at intervals, and with it their original competence and responsibility. The law not only supposes the probability of such a period, but the party availing himself of the plea of insanity was obliged to show, even though the burden of proof may not always have been put upon him directly, that there was no lucid interval. Considering how small a foundation this whole doctrine of lucid intervals has, it is difficult to account for its prevalence. Unquestionably, it sometimes happens that an insane person comes to himself, manifesting his natural propriety of conduct and conversation, his memory and perception apparently clear, the cloud returning after a few days, as dark and dense as before. Exactly how far the mind, in this condition, is free from the influence of disease, we never can know. Considering, however, the suddenness of the change, the brevity of its duration, and the long continuance of the disease, when it occurs in chronic cases, it is not likely that the mind is restored to its normal degree of clearness. It must be borne in mind, also, that, as described by distinguished legal authorities, they are of very rare occurrence. I have not seen more than half a dozen cases in all my experience. All nervous diseases are subject, more or less, to a certain law of periodicity, by reason of which at intervals their regular course is changed, and other incidents come and go in a certain order of succession. The change thus produced may sometimes amount to an entire disappearance of the signs of disease. This phenomena is not unfrequently witnessed in the early stage of

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acute mania.   Within the first month, there may occur a rather
sudden cessation of the manifestations of disease, in which the
patient is calm, quiet, talks and behaves sensibly, though, if
closely observed, there will be found some indistinctness of
memory, and confusion of thought, especially with reference to
the circumstances of the attack.   This condition has often been
confounded with lucid intervals, especially by lawyers, who
find it difficult to see a distinction which can be visible only
to the long practised observer.   To the common eye, any re-
mission in which the patient is tolerably calm after being vio-
lent, and answers a few questions rationally, seems like a lucid
interval.

Admitting, as I do, that a valid will may be made in the
lucid interval, it is so likely that some shadow of disease may
rest upon it, that any testamentary act during that period
should be very closely scrutinized.   Some qualifications for
the act are required, not otherwise needed.   It should be
shown that changes in the circumstances and conditions of
those whom the testator is disposed to benefit, having oc-
curred when he was incapable of understanding and appreci-
ating them, were brought to his knowledge and comprehen-
sion, since such things would naturally affect the dispositions
of the will.   Because it must be borne in mind that the past,
for months or years, may have been a complete blank, or filled
with strange and deceptive images.   In short, we may con-
ceive of a case where every testamentary qualification was
possessed, but, certainly, the fact must be of rare occurrence,
and difficult of proof.   I know of only one case reported of a
will made in what was improperly called a lucid interval, and
established—that of Cartright *v.* Cartright, 1 Phillimore, 90.
The testator was in an asylum, and so severe was the grade of
her disease, that she had restraint on her limbs at the moment
when she called for pen, ink and paper, in order to write her
will.   This she did, at last, after writing on several pieces of
paper, which she tore up and threw into the grate, and walk-
ing up and down wildly, and muttering to herself.   The will
was established, the court deciding that it was made during a
lucid interval, on the strength of the internal evidence, as it
made a natural and consistent distribution of her property.

This fact the court considered conclusive proof that a lucid interval had taken place. The result may have been right, but it was reached by a sort of logic known as reasoning in a circle. The correctness of the will proves the interval, and the interval being proved, makes the will valid. Had the court, while squarely admitting the insanity of the testator, declared that the character of the act showed that she was still rational enough to make a valid will, it would have uttered good sense and good science. Here we see the binding influence of the old law, as expounded by Coke. It being proved that the testator was a lunatic, she was necessarily *non compos*, and could become otherwise only by recovery or a lucid interval.

This influence has not yet entirely lost its force, for I observe that lawyers are not content with proving competence sufficient for the act in question, but also labor hard to prove also perfect soundness of mind. In a case that came under notice, two or three years since, an attempt was made to void the contracts of a person recently deceased, on the ground of insanity. It was shown that the transaction—the sale of coal lands—was just and fair in every particular; that he obtained a price pronounced by his neighbors and advisors to be a fair one; and that he sold then and there without waiting for a prospective rise in value, simply because he needed the money. That he was a lawyer in full practice; a leader at his bar almost up to the day of his death; and all the while, the trusted counsellor of several large corporations—all this needed no proof, for it was seen by everybody. And yet because of the admitted fact that this man had always been remarkably eccentric in his ways and manners, week after week was spent in endeavoring to show either that he was or was not technically insane. If the administration of the law has for its object the promotion of justice among men, we may venture to say that the means it used in this instance were signally irrelevant.

Further examination of Coke's classification of the *non compos mentis* furnishes fresh proof how little help it gave the medical jurist in settling the questions that came before him.

One class, he says, consists of those who by sickness, grief, or other accident, have wholly lost their memory and under-standing. The word *wholly* is probably used inadvertantly, because it would refer to persons in the very last stage of de-mentia, whose acts would scarcely become matter of litigation, whereas, he had in mind, no doubt, a numerous class who, while moving about among men and taking some part in the affairs of life, are, nevertheless, laboring under considerable mental infirmity. We have reason to think that this class was meant to embrace the subjects of senile dementia, of the weakness of old age, and of the damage inflicted by paralysis and other cerebral affection. His allusion to grief and ac-cidents implies, probably, a mistaken notion he had conceived respecting the cause of the mental affection. Taken as a whole, this class, unquestionably, has always furnished the courts with a greater amount of litigation than all others put together, in the matter of wills. The more exact and well defined are our notions of mental capacity, the more foun-dation they have in close, personal observation of this class of persons, the better will be our administration of justice. Much of the apparent conflict in the adjudication of their cases has arisen, I think, from a faulty appreciation of the mental qualities chiefly concerned, and therefore I invite your attention, for a moment, to this point.

Old age is usually accompanied by a certain enfeeblement of the mental as well as the bodily powers. This condition does not imply unsoundness or incompetence. It merely means a diminished power of endurance, an incapacity for those long-sustained efforts once comparatively easy, more difficulty in grasping obscure and remote relations. The mind has lost none of its characteristic tastes, and none of its fond-ness for its accustomed pursuits, but it is satisfied with easier tasks, and welcomes longer intervals of rest. The only mental faculty obviously involved in this condition is the memory, especially of recent incidents, even while old ones are well retained. The forgetfulness of young people comes from carelessness and a predominant interest in other thoughts. With the aged, the new impression, however vivid at first, fades away from a lack of power in the brain to retain it. It

must be quickly and frequently repeated, before it will endure. So far the change may be considered as the normal result of old age, and destitute of any legal consequences. Occasionally, indeed, in the closing years, the mind displays even more than its wonted vigor and brilliancy. It was these exceptional instances that led the poet Waller to say,

> " The soul's dark cottage battered and decayed,
> Lets in new light through chinks which time has made."

Sometimes the change here described proceeds still farther, and induces a condition that is abnormal, ending in *senile dementia.* The memory becomes less and less tenacious; the perceptions less exact and clear, one person or thing being mistaken for another, and their relations misunderstood. Then the judgment—that is, the power of discerning the relations of cause and effect, of distinguishing between the specious and true, of taking in the remoter considerations germane to the case in hand, of weighing and rightly appreciating conflicting claims—loses its vigor and is easily led astray by false lights. And so the process of decay goes on until it reaches its utmost limit in virtual extinction of the mental powers. Now, what we would like to know is the precise point at which testamentary capacity ends, and this, of course, is beyond our reach. As to the effect of this condition, both in its earliest and its latest stage, there can be no diversity of opinion. It is during the intermediate stage that it gives rise to obscurity, doubt, conflicting evidence, and abundant litigation.

To arrive at a correct decision we must first understand what are the intellectual powers necessary to testamentary capacity. In the first place, the memory must be active enough to bring up to mind all those who have natural claims on the bounty of the testator; to make him aware of the nature of his property, its location, the encumbrances upon it, and his debts. If he makes bequests in certain sums of money, he should know with some degree of exactness the value of his property; and if he has made previous wills, he should be aware of their contents. Whether he actually had such a memory will generally be determined, for the most part, by the circumstances of the transaction. In most cases

evidence is given respecting the general condition of the memory, as manifested in the ordinary discourse, and this, coming as it does in a loose, detached, fragmentary manner, from persons usually unaccustomed to observe mental phenomena closely, requires to be carefully and intelligently examined. The lapses of memory exhibited by all old people must be distinguished from that utter loss of memory that no effort can retrieve, even for a moment. The former is chiefly in regard to recent things, which are readily brought back to mind, and are retained for a while. The latter embraces old as well as recent incidents, impressions customary as well as casual, ideas the most as well as the least familiar. The old man who is constantly mislaying his spectacles, forgetting the face of the person to whom he was introduced the day before, and marches up the broad aisle of the church, holding up the umbrella over his head, may be found when his attention is specially directed to a subject, to remember its prominent points, understand them well, and govern himself accordingly If, on the other hand, a person has utterly forgotten the events of his earlier age; if he cannot tell his own age, or the year of our national independence, is unable to tell how many six and six make, and has forgotten whether his estate is in lands, or houses or stocks, he surely has lost his testamentary capacity.

Let it be observed, in this connection, that many of those old people so forgetful of passing events, and so careless of little proprieties, need only to have their attention fixed on the matter in hand to display no lack of memory or understanding.

The mental infirmity, most often the source of testamentary incapacity, is impaired judgment. To make an equitable distribution of his estate among those connected with him by blood or affection,—such a one, I mean, as he would have made while confessedly sound—implies, on the part of the testator, a variety of considerations that cannot safely be overlooked. He should be able to appreciate properly, the nature of their claims, their present and prospective necessities and the favors they have already received; and all this, not to mention other considerations, requires a nice discrimination and

the power of looking before and after. If the bequests indicate any deficiency in these respects, it certainly furnishes ground of suspicion.

There is a large class, you observe, still unprovided for in the schedule of Coke—I refer to that of persons neither idiots, lunatics, nor the victims of sickness, grief or old age—persons having by nature a deficient mental endowment, and embraced under the appellation of imbeciles. The wills of such persons often come into dispute, and though their disposal is determined by the same principles as those last referred to, yet they give rise to a larger range of speculation and doubt. In the one case, the question on which the result may depend, is how much mental power has been lost; while, in the other, the question is how much was ever possessed. The difficulty of the question is increased by the fact that in many, if not most, imbeciles, there is much inequality in the strength and development of the several mental faculties. The same person may be shrewd, even sharp in some transactions, dull and foolish in others; at one moment uttering a pithy remark, at another leaving no doubt of his native simplicity. With shrewdness and folly thus displayed, side by side, it is not strange that different observers are sometimes very differently impressed by what they witness. One who has sold him a gun, or a fishing-rod, or made for him some little article of use or ornament, and listened to his comments, is ready to pronounce him about as sensible as the generality of men. Another who has witnessed his conduct in affairs requiring some prudence and judgment, is strongly impressed with the depth and breadth of his simplicity. And this is a specimen of the evidence heard in our courts when they are called to adjudicate in cases involving the persons or property of simple-minded people. It is also a lamentable fact, that the disposition to form positive conclusions on the strength of a partial, one-sided observation, is about as common among men of some culture, as it is among those who are without pretensions to any. Until mental manifestations are better understood, we shall continue to witness these strangely conflicting conclusions in litigation involving the interest of imbecile persons.

To meet this state of things, the first thought was to fix upon some arbitrary, specific standard of mental power by which the minds of imbeciles should be measured. I know of only one that has survived the test of experience, and even that is seldom offered now. It has been said with some show of authority, that to make a valid will one must have capacity sufficient to make a contract. Had these conditions been reversed, and the proposition been that as much mind was required to make a contract as to make a will, it would have had as much support in the nature of things as the other, and that is none at all. Until we are satisfied as to the exact amount of mind necessary to make a contract, this measure will scarcely help us, even with the estimate I once heard given from the bench, that it is as much as is required in the ordinary business of life, which must remind us of that venerable measure of size with which we are all familiar, "as big as a piece of chalk." The only rule founded in reason and justice is, sufficient mind for the occasion. I hardly need to suggest that to distribute a large estate, equitably and judiciously, among a considerable number of persons and institutions, must require a stronger and wider exercise of mind, than would be needed, for the disposal of a small estate among the few legally entitled to a share of it. And as much may be said of a contract involving many contingencies as compared with one disposing of a few acres of land.

Thus far, it will be seen, the elements of testamentary capacity are strictly intellectual, pertaining to the pure reason. A testament, however, is not always the product exclusively of the understanding. The moral part of our nature—the sentiments, affections, and emotions—may be as potent an an agency in its production, as the intellectual. Hopes and fears, attachments new and old, a sense of dependence, a chronic habit of submission and deference, assiduous attentions, crafty insinuations,—these may greatly prevail over the most obvious claims created by the law of inheritance. Inducements of this character are not excluded by the law. All that the law requires in feeble-minded people is, that they be not excessive, calculated to drive the mind from well chosen, well matured arrangements, and divert the course of property into channels it would not otherwise have taken.

In examining these cases we must never forget that both the intellectual and the moral faculties may have been concerned in their production, because if either of these factors is left out of the account, we are greatly in danger of being misled. Nor must we forget while investigating their respective agency, that they may act with some degree of independence of each other. The same person who thinks correctly and sensibly so far as he goes, may readily yield to inducements strongly presented to his feelings. In a case lately tried in Massachusetts, this distinction was so sharply, and so pertinently made, that it may claim a moment's attention. A man, never married, confessedly feeble-minded and under guardianship, concluded to make his will, which he did without urging or hindrance from any quarter. In this he bequeathed his property—$200,000—to various charitable institutions, and to a few intimate friends from whom he had received much care and kindness, completely ignoring his sisters, for the reason, as it appeared in evidence, that they took no notice of him, and were wealthy enough already. In making the will he was aided by a lawyer of the highest moral standing, and the whole transaction was free from suspicious circumstances. Two or three years afterwards, while in company of certain persons whose relations to him gave them much influence over him, he became so much excited by their reproaches and solicitations, that he called for the will and drew his pen through his signature. The will was offered for probate, but the judge declined to approve it, and an appeal was made to the Supreme Judicial Court, in which a trial ended in the establishment of the will. The jury virtually said the will was a rational act, rationally and calmly done, with memory and judgment sufficient for the purpose, but the revocation was done in a moment of passion, excited by the suggestions of others, and too strong for his feeble mind to resist. The verdict of the jury was approved by the court.

The question of testamentary capacity in feeble-minded people is generally connected with that of outside influence. Sound-minded people may, and often do ask advice in the final disposition of their property, and the result is, very likely, all the better for it. Such advice may be needed all

the more where the mind is weakened by nature or disease,
but when obtained it is always, and very justly, viewed by the
law with suspicion, and the dominant question is whether or
not the testator has been subjected to what is called *undue
influence.* Because, sound or unsound, strong or weak, his
will must be his own will, and not another's.   If the influence
is such that the wishes and the interests of other parties rather
than the testator's, are represented, then the law supposes
that the will is really not his will.   In order to establish the
fact of undue influence, however, a foundation must be laid
by first proving the fact of mental deficiency.   The attempt
is sometimes made by lawyers aided by physicians, to reverse
the process.   Unless the testator has clearly manifested some
mental unsoundness by his previous acts, the proof of undue
influence should be strong enough to be unmistakable.   It
often happens that no such case can be shown; that up to the
moment of the making of the will, no indications of feeble-
ness, of delusion, or wandering, have been witnessed.   I do
not say that even under such circumstances, undue influence
may not be exerted.   When we consider the enfeebling effect
on body and mind of a long, last illness, of the many infirm-
ities that often attend this period, and of the utter prostration
of the will produced by pain and a sense of complete depend-
ence, we can scarcely conceive of conditions better fitted for
the exercise of an undue influence over testamentary dis-
positions.   The courts of our time have become quite familiar
with a certain class of cases presenting these traits.   An old
man marries a young wife, and within a year or two dies,
leaving a will greatly in her favor, much to the disappoint-
ment of relatives who would otherwise have received the
whole of the estate.   These cases are exceedingly embarrass-
ing, for we are often left without any clew to guide us to a
rightful conclusion.   We are sure it is such a will as the tes-
tator would not have made in the vigor and flush of health,
while we hesitate to say, under the conviction that a man has
a right to do what he pleases with his own, how far a sense of
gratitude for kindness and service may be allowed to shape
his decision.   Fortunately, perhaps, it frequently happens
that some circumstance sheds a little light upon the case, en-

abling the jury, if not to decide according to its legal merits, yet to do what, in their rude estimate, "is about right."

You observed, no doubt, that Coke, in his classification of mental disorders, ignores entirely a form of the disease, which is far from being very rare. The reason was, probably, that it seemed so partial in its operation, it left so much of the mind free apparently from its influence—that it was not supposed to impair the person's responsibility for civil or criminal acts. Nearly a hundred years went by before it was first formally recognized by Lord Hale, as well as another hundred years after him, before the law began to take it into the account as an element of excuse for human delinquency, or regard it as a claim on its protection. I refer to what is now called partial insanity, in which a person while entertaining some notions having no possible existence, in all other respects, talks and acts like other men. How to meet the difficulties suggested by this form of mental disease, was a problem entirely beyond the reach of this luminary of the English law, and he passed it along to his successors, many of whom even to this day, have been as unwilling as he to give it any practical effect in the administration of justice. The light shed upon the nature of disease, and especially of insanity, by our better methods and opportunities of study, has not been utterly disregarded, and though we are too often obliged to witness the display of the old ignorance rather than the new knowledge, yet the time is coming, it is to be hoped, when the law will be in entire accordance with science.

It is somewhat curious that up to the present century we hear nothing of partial insanity, strictly so called, in civil cases, and in criminal cases where it was sometimes offered in defence, it scarcely received a respectful hearing. The common idea that the monamaniac had still reason enough left for all practical purposes, protected his testamentary privileges, and generally, it may be supposed, the rule worked no injustice. Had the courts been brought face to face with a will, the manifest offspring of a gross delusion, they might have refused to sustain it. It is hardly conceivable that they would have approved a will devising a large estate to the building of a railway to the moon, (though hardly more a folly than many

exclusively mundane that have been built), however prudent, sagacious and intelligent the testator may have been in the ordinary affairs of life. It was not until 1828 that this question of the legal effect of partial insanity on the testamentary capacity was squarely met and rightly decided. I refer, of course, to the case of *Dew* v. *Clarke*, which came up for final adjudication into one of the English ecclesiastical courts, Sir John Nicholl presiding. The matter in issue was the validity of the will of a London surgeon, who bequeathed the bulk of his estate to his nephews, leaving only a life-interest in a small portion of it to a daughter, and only child. The testator had, for many years, very creditably practised his profession, and though regarded by his patients, and many others, as eccentric and irritable, was never suspected by them of laboring under any kind of mental derangement. On the other hand, it appeared that he had always entertained the strongest aversion towards this daughter, describing her to his friends and strangers as prone to all manner of vice, as a perfect fiend, an imp of Satan, charging her even with impossible crimes. His treatment of her was almost incredibly savage, not only compelling her to perform the most menial offices, but would often strike her with his clenched fists, cut her flesh with a horsewhip, and once when she was only ten or eleven years old, he stripped her naked, tied her to a bed-post, and after flogging her with a large rod intertwisted with brass wire, rubbed her back with brine. It appeared that there was no cause for this extraordinary antipathy. The girl was described by all who knew her as amiable and docile in her disposition, and perfectly correct in her deportment. Against this will the court pronounced an exhaustive and most elaborate judgment, untrammeled as he was by the precedents of the common law, and inspired by the larger spirit and freedom of the civil law. Remarkably well informed on the nature of insanity, he discussed its effect on the mind of the testator with a sagacity never before witnessed in a court of law, reaching to the conclusion that the mental disorder was fatal to the validity of the will. Against the doctrine there announced, novel and unprecedented as it was, no voice of dissent has ever been raised. It is one of the few things in the medical jurisprudence of insanity which may be considered as established.

Sir John Nicholl, be it observed, was careful to restrict the operation of the principle to the case before him. The mental disorder was sufficient to vitiate, not any will, but the will in question. Had the testator bequeathed his property to his daughter, he would, probably, have established the will, insane as he was. The insanity would have been no bar to a natural and proper distribution of his estate, and so, I apprehend, the matter is now generally regarded. The objection arises only when the distribution is not deemed to be natural and proper by various relations who find it for their interests to destroy the will. The principle being settled that insanity does not necessarily impair a man's capacity to make a will, any more than it destroys his power to do many other things as well as ever, its effect ought not to be determined by any arbitrary rule, but rather by that judicious consideration of the various circumstances of the case which is founded on correct views of the nature of insanity and the ordinary motives of human conduct. In accordance with these views a case was adjudicated in the court of Queen's bench, in 1870, *Banks* v. *Goodfellow*, L. R. 5, 2, 5,549, Chief Justice Cockburn delivering the opinion of the court. Here a will was established, notwithstanding the testator was proved to have entertained some gross delusions, for it was obvious that these delusions could not possibly have influenced the dispositions of the will. These two cases, I presume, have settled the rule of law in regard to the effect of delusions on testamentary competence, and thus, happily, brought the law of the land into harmony with the laws, physiological and pathological, of the mental constitution.

And here we see the injustice that might be committed by making insanity, abstractly speaking, incompatible with testamentary capacity, for if we say that a man who disinherits his heir at law under the delusion that they have attempted to poison him is thereby *non compos*, how shall we answer the question whether his will should be approved even if he had bequeathed his property to those heirs at law, notwithstanding his delusions?

And let me say in this connection, that the effect of mental impairments on the testamentary power is not to be estimated

solely by their demonstrative symptoms, for it may be greatest when scarcely seen by the world at large. I have never met with worse wills than some made under such circumstances. I have known a will made shortly after an apparently slight attack of paralysis, pre-eminently absurd, irrelevant to any worthy purpose, and almost if not quite impracticable of execution. And yet this man seemed to have completely recovered, and continued his ordinary pursuits till prostrated by a second attack.

Wise and proper as the doctrine may be, generally stated, that a delusion should vitiate any testamentary disposition made under its influence, cases can easily be imagined where it would be exceedingly embarrassing to determine the exact range of its application. We readily admit that the will of a man disinheriting all his heirs at law—brothers, sisters, nephews and nieces— in the belief that they have been attempting to take his life, should not stand. But supposing this delusion referred only to a single relative, the rest of them being properly remembered, I think we should hesitate to break the will for that reason alone.

In pursuing the progress of thought on this subject, we meet at last one of those extravagances of opinion, which, coming from men of commanding intellect, produce surprise if not admiration. Lord Brougham declared, on one occasion, that partial insanity, however limited apparently, should vitiate all the patient's civil acts, as well as the more general forms of the disease. He regards the mind as a single indivisible potency, and consequently that any impairment of it must be absolute, not partial. On this theory, of course, there is no place for the practice of dividing and subdividing the mind, some portions becoming unsound while others remain sound. Lord Brougham's doctrine is not without warrant, certainly, in the prevalent metaphysical theories of the last century, and, accepting them, it would be easier to reject it with feelings of wonder and surprise than to refute it. If inconsistency would furnish a conclusive argument against it, it may be found in the statement he once made, that a man might be so unsound as to be regarded by his Maker as irresponsible for criminal acts, while he might be justly held responsible by his fellow-men.

The effect on testamentary capacity of extraordinary beliefs, fanciful projects, or bequests for impracticable purposes, is frequently not very easily determined. Such things are suggestive of insanity, and the event has sometimes been made to turn on nice distinctions between insanity and eccentricity. In these cases the proper line of inquiry must depend on the circumstances of each particular case, and the decision should be governed more by the dictates of common sense than any arbitrary rules of law. In some cases there can be little difficulty in arriving at a satisfactory conclusion. If a man noted for some oddities of thinking and acting, but otherwise correct and shrewd, believes that Brandreth's pills are a certain cure for all diseases whatever, and that everybody who would take enough of them would live to a good old age, this notion would hardly vitiate a will making unexpected and unjust bequests having no connection with and traceable in no way to it. If, on the contrary, he had devoted a considerable portion of his estate to the maintenance of a fund for supplying the poor with Brandreth's pills, this, certainly, would be good reason for breaking the will. Take another case. In Massachusetts, lately, an elderly gentleman in failing health, and with diverse nervous ailments, was induced to try the movement cure, and came at last to conceive the most exaggerated notions of its medical efficacy, though it never helped him much. Indeed, some of these notions almost, if not quite, amounted to delusion. In this state of mind he made his will, by which he appropriated a great part of his estate to the establishment of an asylum for nervous invalids, to be treated by the movement method. I have no hesitation in saying that that will was the offspring of a morbid nervous condition, if not of delusion, and therefore not to be established. Whether certain mental manifestations are indicative of insanity or only eccentricity, is a point not always easily settled, and no splitting of hairs on the question will prove so satisfactory as the exercise of a little common sense. In many of these cases, where, apparently, the mental twist, is very limited, and of doubtful character, a close scrutiny of the conduct and conversation will show here and there traces of a more extensive influence, thus shedding additional light on the matter in hand.

In presenting the subject of testamentary capacity in the way I have, it was for the purpose of giving to the pathological element the prominence it rightfully deserves, and which consequently ought to secure it a controlling influence in disputed cases. And let me say, in conclusion, that the administration of justice in this particular must often be imperfect, until the light of medical science is freely admitted and used—not the light that has traveled down to us from the times of Coke and Hale, but that which we owe to the progress of knowledge during the present century, greater, far greater, indeed, than that of all other centuries together.

The Medical Society of the County of New York.

Before the county medical society Dr. E. C. Spitzka recently read a paper entitled, " The Localization of Cerebral Diseases, in the Light of recent Anatomical Discoveries." We are fortunate in being able to present you a *resumé* of the same, as follows:

There is no province in the body the interpretation of whose pathological conditions is open to so many sources of error as the encephalon, and these sources of error increase as we pass from the medulla through the peduncular tracts to the hemispheres.

While the relations between lesions and symptom are comparatively clear in the case of the pons and crus, they are, in the case of the higher, still involved in the uncertainties of experimental physiology.

There are still further sources of error, due partly to the complicated character of the various commissural connections existing between distant centres, partly to the fact that many of the sensory and motor peripheries enjoy a multiple representation in the brain, and also, that the complex character of our highest functions presupposes an equally complex anatomical basis.

Certainly the most exact method for determining the function of any ganglionic centre, as well as of its efferent and afferent fasciculi, is the anatomical, that is, the tracing of nervous strands to their central as well as their peripheral terminations. As soon as the function of all these elements shall have

been determined (and they are in a fair way of being determined at the present day), it will be possible to place most cerebral symptoms in connection with lesions of definite topographical areas.

I have on a previous occasion given physiological reasons for rejecting some of the conclusions drawn from the experiments of Hitzig and Ferrier. To-night I shall endeavor to disprove them on anatomico-pathological grounds. For this purpose I have collated the cases scattered through the literature of the last five years, and compared or contrasted the general result, with several observations of my own, and the inferences of anatomical investigators.

I exclude from consideration all imperfectly recorded instances, as well as such in which either multiple lesions, or lesions of great extent existed, and in which the reporter arbitrarily selects one out of many lesions as the cause of some single symptom. Almost all the cases reported by Ferrier, and some of those furnished by Hughlings Jackson, are of this character.

Let us first take up aphasia, that complex of symptoms which pathologists have long sought to bring into relation with a certain cortical area.

Of 23 reliable cases, scattered through the literature from 1872 to 1875, the lesion was *bilateral* in *five ;* in *six* there was an extensive lesion of the *left* fronto-sylvian region, including Broca's convolution; in *seven* there were various destructive lesions in the left hemisphere, leaving Broca's, and in some instances all the marginal gyri *intact ;* finally, in *one* case there was a large flat abscess, limited to the *right frontal lobe.* The remaining four cases illustrate the faultiness of many interpretations *pro* and *contra* Broca's theory.

Bourneville, a pupil of Charcot, for instance, finds multiple softenings of the frontal and occipital lobes, but attributes the aphasia to a small and superficial necrosis of Broca's gyrus. The case of Boinet is similar. The other two are used polemically against Broca, but with as little reason as the former were interpreted in favor of his view.

Even from this contradictory testimony, a few definite conclusions can be drawn: 1st, that aphasia, whether due to a

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right or left-sided lesion, is not necessarily associated with the island of Reil, or its contiguous territory, for the case of Troisier proves that softening of the occipital lobe *alone* may cause this symptom ; 2d, that a lesion of the Meynertian territory on one side alone is not always sufficient to produce aphasia; there are four very reliable cases on record where grave lesions of Broca's convolution, or this together with the island, failed to produce the slightest effect on speech. One of the most remarkable of these four cases, is that reported by Bergmann. A dragoon had received a kick from a horse on the frontal bone; on the autopsy, the whole left frontal lobe, island of Reil and marginal convolutions were found gangrenous ; the patient, however, had lived seven days after the accident, and had *never at any time during that period* manifested the slightest aphasic symptom. We also find that one-sided lesions, which a subsequent post-mortem has shown to have been destructive in character, remain permanent, and yet the aphasia which was due to that lesion gradually disappeared during life. This proves that however much the left hemisphere may predominate over the right, that there is a complete physiological symmetry between the two, and consequently that the symmetrical area of the intact hemisphere may vicariate for the affected region ; on no other grounds could the re-establishment of function, after its temporary abolition through a destructive lesion be explained; 3d, both clinical experience and inference from structure permit us to say, that bilateral destruction of Meynert's territory (that is, the *insula Reili* and the *marginal gyri,*) will produce permanent aphasia.

But how are we to account for the case of Troisier, and similar ones, in which aphasia resulted from an occipital lesion? While the fibres which ultimately abut in the hypoglossal and facial nerve nuclei can be traced into the operculum and island, giving us an anatomical basis for the aphasic symptom, no *motor* fasciculus of any size can be traced to the occipital lobe !

Our faculty of speech is certainly more complex than is generally supposed, and the terms amnesic and ataxic aphasia, by no means exhaust the possible pathological interferences with its delicate mechanism. The first step in the acquirement of

speech is its phonetic element, we hear a word or sound, and as far as it is a mere sound-impression it is registered in a sensory area of the cortex.

We then experiment, as it were, with our motor apparatus, until we find the combination requisite to repeat said word or sound. This motor innervation has its conscious seat in Meynert's region, while the sensory perception is located in a distant area (probably, though not certainly, the occipital lobe). Now, in order that the sensory perception may control the "correctness" of the motor expression, the two must be associated. It will then be indifferent, whether the sensory center, the motor center, or the associating band be destroyed, we will have aphasia in either case. And there are still more intricate relations which may be equally interfered with, causing either *aphasia, agraphia, alexia,* or a combination of any two of these, or all. (This was illustrated by the aid of a diagram.)

The contributions which I am able to offer to the patho-anatomy of aphasia, consist of four cases. Two of these I exclude on account of their impure character. Of the other two, the first was a negro, who was under observation for three weeks, and who, to within forty-eight hours of his death, manifested no other trouble with his speech than a great slowness; at the period mentioned it was found that he had forgotten the names of the tools used in his trade (carpenter), and did not recognize his own name, but could signify his meaning very well by symbols. He was not hemiplegic, the facial folds were obliterated on both sides, and his pupils were unequal; he was also incoherent and had loss of memory, but all these were symptoms dependent on the chronic affection from which the patient suffered (paralytic insanity).

He gradually sank and died, without any material change of symptoms.

On the autopsy, there was found general bloody suffusion of the pia mater, most marked over the convexity of the left hemisphere. There was a sub-meningeal extravasation of blood which had forced its way between the gyri, lifting the pia from the cortex, and extending in between the operculum and temporal lobe, covering the whole island of Reil. The blood was firmly coagulated, and the maroon-colored clot cov-

ered altogether a circular area, involving the first frontal part of the second frontal,[1] lower third of the præcentral, whole of the *gyrus angularis*, and all of the first temporal excepting its anterior end. There was a second hemorrhage in the caput of the corpus striatum, extending into the internal articulus of the lenticular nucleus, as well as part of the anterior portion of the internal capsule. This clot was firm and laminated, some of the laminæ being discolored, on its outside the blood had the appearance of a more recent clot, and this portion was continuous with the sub-meningeal extravasation, through a break in the cortical substance of the most anterior *gyrus opertus* of the island. Broca's convolution, as well as the contiguous portions of the island was compressed, and their medullary fasciculi were destroyed by the extravasation. This case, in which the aphasic symptoms were really minimal, shows that no matter how *extensive* a unilateral lesion may be, if its *production is gradual* (in this case a slow hemorrhage), it will give the opposite hemisphere time to accommodate itself to its vicarious duties, and the increased requirements thus thrown upon it.

The other case is in some measure an antithesis to the one just detailed. It was a man suffering from chronic mania, and who had been under surveillance for several years in our city asylum. My friend, Dr. Kiernan, described the patient as presenting no other symptoms during this period, particularly no convulsions or paralysis. But about three months before death he began to manifest symptoms of pachymeningitis; he had sharp localized cephalalgia, became occasionally stupid, and there were regular morning rises and evening remissions of the temperature. He then exhibited twitching of the facial muscles of both sides, and seven weeks after the initial symptoms had manifested themselves, he had general bilateral convulsions of an epileptiform character, without complete loss of consciousness. Of these attacks he had seven altogether, in the last he died. From the time of the occurrence of the first convulsive seizure, he became gradually and progressively aphasic, his aphasia being of the purely ataxic variety. At

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* In this as in previous papers, the more rational nomenclature of Meynert-Wernicke is adopted in preference to that of Ecker.

first he used wrong words, or spoke in broken sentences, finally
he could not speak at all, and this remained unchanged for the
remainder of his life. On the autopsy, the dura mater, which
elsewhere was perfectly healthy, was found thickened, infil-
trated with pus, and adherent to the skull on the one hand and
fused with the leptomeninges on the other, over the right fron-
tal lobe. Dense pseudo-membranes were intercalated between
the pia and cortex, and the sulci were filled with the same ma-
terial.

The whole right frontal lobe as far back as the præcentral
gyrus, and down to the level of the lowest frontal gyrus, was
softened and necrotic.

Thus much for the pathological side of the case; we had
nearly the same cerebral area involved on the right side as was
involved on the left side in the negro; the lesion was also one
of slow production, and yet we had complete aphasia, showing
that the left hemisphere had not vicariated for the right. The
reason for this was found on examing the teratological con-
dition present, the brain was completely asymmetrical. The
left cerebral hemisphere was atrophic, and the right crowded
it away from the median line.

(Other asymmetrical conditions as of the optic tracts,
pyramids and cerebellum were also referred to.) It was
obvious then that the left hemisphere was prevented from
vicariating for the right, on account of its congenital deficiency.
This patient was not ascertained to be left-handed during life.
The negro was dextral.

Let us now proceed to study the results of cerebral anatomy,
as well as the comparative value of various methods[1] em-
ployed, and then apply the results obtained to the elucidation
of pathological problems.

In the first place, it must be declared an error, to suppose
that any motor or sensory periphery has only one representa-
tive centre, in the central nervous system. If we trace any
fasciculus of the pes pedunculi, which appears to terminate in
one of the motor nerve nuclei, up into the corona radiata and

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[1] A fuller exposition of the methods employed by European investiga-
tors, as well as by the author, will be found in the present number of this
JOURNAL.

internal capsule, we find that its fibres become scattered, and interlace with other fasciculi of a very distant peripheral termination perhaps. Where it is possible to trace such a fasciculus to the cortex, it is never found passing into one convolution alone, but into several. We must believe that every point of the bodily periphery is represented in the cortex of the small cerebrum of the dog; if this is granted, how much more frequently must a given periphery be represented in the immensely more extensive human cortex? (The writer then gave reasons for considering the occipital lobe as mainly, *though not exclusively*, sensory, while the frontal was mainly though not exclusively motor, and that there were great anatomical probabilities for localizing the voluntary innervation of the face, tongue and larynx, perhaps in the operculum and island).

As in the operations of a telegraph, it is indifferent whether the telegraph station or the wires be destroyed, so far as the transmission of messages is concerned, so in the brain it will be difficult to judge from the results of a lesion to its seat in many cases. Hemianæsthesia, for instance, may be due to a comparatively small hemorrhage at the base of the corona radiata, (whose posterior portion, anatomical methods have traced to the posterior spinal column) or to a larger one in the medullary centre, or finally to an extensive cortical lesion.

Hemiopia may be due to a small lesion of one optic tract, of the posterior tubercle of the thalamus, or of the occipital lobe, [writer here referred to the interesting case of Dr. Pooley's which was reported in the JOURNAL, and in which the writer considered the cortical gumma (the primary lesion) as the cause of this symptom.] Two general and very important inferences can be drawn from the combination of pathological experience and anatomical inference. First, since the fasciculi which terminate in the central tubular gray matter on the one hand, and the basal ganglia or cortex on the other, are more scattered at the latter terminus than at the former, it follows that a comparatively limited lesion in the medulla or pons will produce as marked a paralysis of a certain muscular group, as will a much larger lesion of the hemispheres. Secondly, as the fasciculi, which represent the voluntary con-

trol or conscious transmission of motion and sensation in special areas of the bodily periphery, are separated from other fasciculi in the medulla, pons and crura, (partly) while they are intertwined with them in the hemispheres, it follows that the more limited a paralysis or anæsthesia is to any special territory, the more probably will the lesion be found in the peduncular tracts, while the more complicated the symptoms are, the nearer the cortex will we have to seek for the corresponding lesion. We accordingly find that very small tumors, cysts or apoplexies, are by no means infrequently present in the hemispheres, without having caused any appreciable symptom, while in the medulla relatively small lesions produce grave or even fatal results. An example of this was offered by a ̇patient who suffered from that variety of progressive paresis, in which the spinal symptoms predominate and are the first to appear, who became gradually but completely deprived of the use of his tongue, so that while its reflex motions, such as the automatic actions performed by it, during mastication, were perfect, and no atrophy could be observed, he was unable to protrude it when asked to do so, and he exhibited that symptom which is now called *alalia*, for he could not speak at all, only labial sounds being possible. At the autopsy no hemispheric lesion could be found to account for this; there was considerable degeneration of the posterior, and lesser of the lateral columns of the cord, and a marked but small sclerotic patch was found in the raphe of the medulla at the horizon of the hypoglossal nucleus. Through the spot affected by sclerosis in this patient, normally run those fibres which connect the pyramids with the hypoglossal nucleus. As these were destroyed here, no voluntary messages could be transmitted, but no trophic disturbance occurred, for the nucleus itself was intact.

One curious observation of recent times insisted on, in its physiological bearings most strongly by Brown-Séquard, is that a hemiplegia or a unilateral convulsion does not necessarily point to a lesion on the opposite side of the brain (where such lesion is hemispheric); that there are exceptional cases of this kind, there can be no doubt, they can no longer be explained away by supposing unrecognizable lesions to

have existed in the other hemisphere, and I have myself made
the autopsy of a case of this kind, which had been under the
clinical charge of my friend, Dr. T. R. Pooley.

For this there is an anatomical explanation. The pyram-
idal decussation, which is nearly a complete decussation in
ninety-nine individuals out of a hundred, in others is al-
most entirely absent, approximating the condition found in
other mammalia; where it is absent, voluntary motor impres-
sions must remain on the side where they originated in the
cortex. (The writer then described the inferential seat of
hallucinations, objected to the theories of Moos[1] and Jolly,
discussed the relations of the corpora quadrigemina, and
gave a digest of the views of Meynert on the anatomy and
pathology of the thalamus. He then discussed the relations
of the raphe and oculo-motor innervations.)

One of the most difficult chapters in the localization of
lesions, is the influence of pathologically affected localities
upon remoter intact regions, with which they are in commis-
sural relations. Thus abscesses in the cerebellum often
cause convulsions; it is difficult to believe that the original
motor impulse resides here, I should rather seek for a trans-
mission of the irritation to the cerebral cortex or ganglia ; it
is well known that a powerful fasciculus connects the cere-
bellum and cerebrum through the brachium conjunctivum
(Meynert, Luys), not to speak of the connections established
by the pons varolii.

In many cases of tumour of the pons where the tegmentum
was deeply involved, conjugated deviation of the eyes has been
noticed, a symptom which *a priori* would seem to point to the
corpora quadrigemina as its seat. What explanation has
anatomy to offer for this? Well, there is a band of fibres
known as the *posterior longitudinal fasciculus of the tegmen-
tum*, which runs close to the floor of the aqueduct and fourth
ventricle, whose lower terminus is unknown, but whose upper
is in and close to the corpora quadrigemina (Flechsig, Deiters,
Forel). We find that it rises and sinks in the animal scale
with the development of the visual apparatus, it is almost
absent in the mole, and I can add that it is rudimentary in the

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1. *Archives of Ophthalmology and Otology*, 1876.

bat. Irritation of any portion of this tract might be transmitted to the corpora quadrigemina, and if unilateral could thus produce conjugated deviation. The same reasons might be advanced for the occasionally similar results of cerebellar lesions. It is thus often impossible to localize a lesion on the strength of a single symptom, and where, as in a case of multiple syphilomas which I recently described, three lesions are present in the same tract of fibres, it is impossible to refer the disturbance of that co-ordination over which such tract presides, to any one of them.

There is another important conclusion to be drawn from the anatomically demonstrated multiplicity of tracts having the same function, viz., that destruction of one special tract does not necessarily imply loss of function—thus destruction of one anterior pyramid is not always followed by complete paresis of the opposite side of the body, as there are deeper fibres (Henle) having the same and similar central connections.

The pathology of the remainder of the medulla oblongata is so well known and has been brought into such close connection with the anatomy of this region that I need not here refer to it.

(The writer then discussed the anatomical connections of the cerebellum, its relations to the acoustic nerve which was most probably the basis of the sense of equilibrium, determined by the semicircular canals. He then showed that although clinical evidence was wanting, it might be expected that some difference would be found between superficial (cortical) and deep lesions of the cerebellum, for it was determined by anatomists that the *nucleus dentatus cerebelli* was connected with one great fasciculus which entered the cerebellum, the cortex being connected with the other two. A case of a patient who could only carry on his work (tailor) when leaning against some firm object with his head, or abdomen, and after whose death a cholesteatoma of the *vermis anterior cerebelli* was revealed, was then given.)

Returning to the hemispheres of the cerebrum once more, let us see how far we are permitted to localize symptoms in definite areas, always promising that the lesion is not an irritative one, but one which destroys slowly the centres or their

projection fibres. Hemiplegia, hemianæsthesic hemiopia, or a special form of aphasia, can be approximately referred to the disturbance of a special cerebral district; more limited symptoms can not be yet so referred. (Even clinical experience was conflicting on this point; cases from Samt, Fürstner, Bernhardt, Hughlings Jackson, himself, and others, were cited as testimony against Ferrier's conclusions.)

As to still more complex symptoms, disorder of the higher faculties for which no anatomical substratum has been found, to attempt to localize them in special areas would be nothing less than absurd. That such faculties depend on the association of numerous different and distant centres, is the only rational supposition that can be held, and any intricate intellectual processes *must* involve the greater part, or the whole, of one hemisphere.

I say this more especially because some recent writers have endeavored to associate such complex symptoms as moral insanity with deficiency of the occipital lobes, or the presence of certain aberrant convolutions, such as Benedikt, of Vienna, in answer to whose propositions Meynert and Heschl very properly insisted that while aberrant convolutions occurred in honest workingmen and in sages, the occipital lobe was relatively better developed in the anthropoid apes than in man, and that consequently if the moral sense depended on that lobe these apes were certainly superior to man !

(In conclusion the writer showed *why* a pathological basis could not be expected to exist for every symptom, he referred more especially to certain of the higher intellectual alienations.[1] Their very existence presumed an *intact* though a *perverted* anatomical basis; negative observations were of a positive value here.)

1. It would hardly be considered possible that within the last half-decade an American alienist should undertake to assert from the negative evidence of the autopsy made in the case of Waltz, that this murderer could not have been insane.

Although that alienist should be mildly judged in so far as he sought to demonstrate the correctness of a theory which he expressed during the criminal's life time, yet in view of the negative evidence of cerebral autopsies in many of the insane, such a conclusion must, always appear ill founded, and over-confident. Notwithstanding which, it has become incorporated in a recent compilation published in the May number of the N. Y. Neurological Journal, 1876.

After the reading of the papers of Dr. Spitzka, Dr. Geo. M. Beard remarked that it was rather warm weather to discuss such difficult themes.

His own theory of the physiology of the brain was a sort of compromise between the localizers and anti-localizers. It was that the different faculties, physical and psychical, are localized in definite centres or starting points, but that such center is capable of acting and being acted on by numerous and complex reflex irritations. This theory would account, he thought, for all the known phenomena of the brain and mind, in health and disease. It was stated, provisionally, as a working hypothesis; but if true, it must account for all facts relating to the brain that shall hereafter be discovered.

There have been three theories of the physiology of the brain; first, the old theological theory, that the mind is something quite outside of the body, and, so to speak, comes down upon it, making the brain its seat. This theory belongs to the pre-exploratory stage of the subject, and it is not for science to accept or reject it. It simply passes it by.

Among scientific men some have held with Flourens, that the brain acts as a whole, and others with Gall, that the separate faculties are separately and absolutely localized. Recently Brown-Séquard has been advocating the theory that there are no centres of function, but that the organs of the different faculties are scattered through the brain. By this theory he explains very satisfactorily the cases where paralysis occurs on the same side with the lesion, and other anomalies. But his theory will not account for other facts of the brain, and in science a theory must account for all the phenomena, else it is valueless only as scaffolding by which we rise to a true theory that will account for them.

The reflex theory of the brain, or the body theory as it might be called, has the strong support of analogy, since the body itself is but a bundle of reflex actions. When any irritation is applied to any part of the body, the effects of that irritation may be felt in that part or at some very distant part, or in many other and distant parts, according to the character of the irritation.

Take the irritations that give rise to tetanus, a subject that

Dr. Beard had been specially investigating during the past year. Fever ulcers, cancers, grave diseases of the skin, are not as likely to create tetanus as some slight affair—a tack, a splinter, or a nail in the finger or foot.

So all through the body and in various diseases. Take the cases recorded by Dr. Otis, where stricture of the urethra had caused neuralgic and various other morbid sensations. Take the cases of paralysis reflected from phymosis, observed by Dr. Sayre and others. Take the cases of headache that come from eye trouble. Take the case of Dr. Fisher, which Dr. Beard published two years ago, wherein chronic gleet resisted all treatment until a mechanical contrivance in the shoes relieved the ligaments in the bottoms of the feet that had been strained by excessive marching. A distinguished scientific man, now deceased, told him a few years ago, that while camping out in Florida he was bitten by a poisonous spider. The sore healed, but subsequently whenever he had a passage from the bowels a painful sensation was felt in the scar.

The symptoms of ataxy have been caused by a scar on the ball of the great toe, and cured by a removal of the scar: hiccough had been cured by pressure on the wrist; hysteria in women by compression of the ovaries, and hysteria in men by compression of the testicles; severe superficial burns may cause ulcers in the bowels; a nervous child may be quieted for hours by scratching the head, or the palm of the hand, or the soles of the feet. In one case of hay fever the symptoms were excited by single pressure over the left eye-brow, and in another case by the touch of the hair on the face when blown by the wind. Trifling lesions of the neck of the womb make up and keep up hysteria and loss of voice, while grave uterine diseases, as cancer, etc., rarely or never produce such reflex effects. Sick headache may be reflected from the stomach, the liver, the genital organs, or the spinal cord. Nervous dyspepsia, when severe, may cause pain in the calf of the leg, while neuralgia of the arm may accompany disease of the heart.

Who could, from our present knowledge of physiology and pathology, predict their reflex effects? Who can fully explain why they happen just as they do, instead of some other way?

I believe that the brain in this respect follows the analogy

of the rest of the body, and does not have, as many suppose, laws peculiar to itself. Accepting this theory, we can see that certain kinds of injuries or disease of the brain might act directly on the function of the part injured, while a different kind of injury might act wholly on one or many distant parts. An injury or lesion that partakes of the character of a slight local concussion, may cause molecular disturbance, is propagated along the line of least resistance to some one or several centres.

On this subject we are not left to analogy alone. The results of direct experiments on the brain are in harmony with this view. Mild galvanic irritation of definite areas of the cortex cerebri produces constant effects of various kinds. Strong galvanic irritation of the same areas produces various and complex effects on the same side or on both sides. The theory of diffusion to the central ganglia, by which it has been sought to explain the effects of *mild* galvanic irritation, is untenable, as I convinced myself when I repeated the experiments of Hitzig. All electro physics is against it. Without experimenting at all, we could tell from our knowledge of the physical laws of electricity that it is diffused in various directions, at a distance from the electrodes, and the galvanoscopic frog, that is sensitive to exceedingly small currents proves this, but then diffused currents are insufficient to excite the nerve.

The reflex or body theory thus explains the real fact observed by Gall, explains the experiments of Hitzig, and explains the anomalous pathological facts gathered by Brown-Séquard and others.

It may be objected to this theory that it makes it impossible to diagnose accurately in all cases the seat of disease in cerebral trouble. This is true enough, but it is a separate issue. That is the way the body is made all through and in every way if not in all of its functions. Physicians see every day cases of sick headache, of neuralgia, of paralysis, of hysteria, of spinal and cerebral irritation, of eye and throat trouble, that are reflected from some part of the body, and in many cases it is impossible to tell certainly from just what part. All living things are constructed on this principle. What reason have we to expect the brain to be an exception to the law of the body? It would be a very pleasant thing if the body and brain

were so made that one could tell always just where any disease
originates—from what part it is reflected ; but we are not so
made. It would also be a very pleasant thing if there were no
disease at all. We must take the body, in health and disease,
as we find it, not as we would have made it if we had had the
power. In the majority of cases of body disease, we can tell
where the seat of the trouble is, but, on account of the com-
plications of reflex action, not in all cases ; and in the majority
of cases of brain disease we can tell with reasonable accuracy
where the seat of the lesion is, but, on account of the compli-
cations of reflex action, not in all cases. I continually see and
study in detail cases of paralysis from brain disease, in which
I am not sure of the diagnosis; and the doubts and difficulties
increase rather than diminish with increased experience.
There are many other facts and considerations bearing on
this theory that might be discussed in a proper time and
place.

The Neurological Society.

At the June meeting of this society, Dr. James G. Kier-
nan read a paper entitled "The Patho-Psychology of Alco-
holism in its relation to Paresis and Epilepsy." We present
the following synopsis:

The real physiological and pathological action of alcohol in
the system, is as yet undetermined. There have been, it is
true, many dogmatic assertions on the subject, but these are
rather things wished for than strictly demonstrable facts.

This condition of things has resulted from the mingling of
sacerdotal and ethical influences, which, as usual, vitiate any
attempt to place matters on a sound, scientific basis. Taking
the view most prevalent in the United States, alcohol is a sub-·
stance foreign to the body, acting only as a spur to vital pro-
cesses, causing thereby more subsequent exhaustion, than the
original benefit, or as very meagre heat-producing substance.

It is a very frequent cause of paralysis, insanity and all
forms of disease and of all crimes. These statements are so
frequently and so strongly made, that it is not surprising that
they are accepted as truths.

The exact function of alcohol in the system is not settled.
It certainly, as has been shown by Anstie, acts as a true food,

and the heat-producing theory rests only on that very fallacious chemical therapeusis which ignored the great difference between simple inorganic, and the very complicated organic compounds. It is even yet doubtful whether alcohol is not a normal constituent of the blood.

Certain experiments are known which would, according to the same process of reasoning by which Headland shows the presence of quinine in the normal blood, show the presence of alcohol as a constituent of the blood in the normal condition. That life can be sustained on alcohol alone for some considerable time, is a fact known to every physician, but which is totally ignored by the temperance physiologists, or, at best, has a very dubious explanation given to it.

That alcohol does cause certain diseases is, of course, beyond a doubt; but that it exerts an influence of the nature frequently ascribed to it sufficient to exclude the action of any other cause, is certainly not well established, there being so few cases, so purely due to alcohol, as to admit of no doubt that alcohol, and alcohol alone, was the cause.

Thus locomotor ataxia is not unfrequently ascribed to the action of alcohol, yet nine-tenths of cases so ascribed would be found, if carefully examined, to have had sexual excess, syphilis, exposure to cold and damp, and the numerous other causes to which, when there is no possibility of bringing in alcohol, the disease is ascribed.

Hammond says, "in the majority of cases no cause can be assigned" "which is very probably the case with the greater part of the remainder, only as the individual happens to be intemperate or indulgent to excess in sexual pleasures, and his friends happen to have a holy horror of these two vices, they endeavor (and do not hesitate at a little ingenious twisting) to prove that the alcohol, or the sexual excess, was the cause of what otherwise would be registered" "etiology unknown."

There have been certain cases where alcohol has undoubtedly caused locomotor ataxia, but these are far from typical, and have a very favorable prognosis.

As with locomotor ataxia, so with other nervous diseases, these are all increasing with great rapidity although compared with the period of the reigns of the four Georges, this is a very temperate age.

The Irish in their native land have always been held as a very intemperate, yet certain forms of disease are infrequent among them there as compared with themselves in England and the United States.

Causation, to speak frankly, is hastily jumped at,more especially where certain benevolent desires point the way, as is the case with alcohol. Alcohol certainly does cause disease, both psychic and physical, and with the forms of the former it is more especially the object of this paper to deal. The psycho-pathological action of alcohol on which all authorities are agreed, is the production of a peculiar delirium which, whether it result from alcohol alone, or from an abstention after a long continued debauch, is acknowledged by all as a consequence of alcoholic poisoning. This, like the delirium produced by other toxic agents, as opium, belladonna, and sometimes quinine, as also that of certain fevers, is characterized by great dread, hallucinations of disagreeable objects, etc. There are also said to be at times pleasant and agreeable visions. These are exceptional, and in the few cases that occurred of those which came under observation, they were certainly attributable to modifying circumstances and were really a psychic disturbance complicating the existing delirium. One case a broker had been engaged in selling and buying gold to a considerable amount, failed, and while laboring under the excitement produced by his arduous business labors went on a prolonged spree which resulted in delirium tremens, in which the idea of enormous quantities of gold being on the ground before him were mingled with the ordinary hallucinations of delirium tremens. The delirium varies.

In certain cases, as in the delirium of fevers, the patient, while retaining a recollection of the hallucination, can be recalled to ordinary life by a question from a by-stander regarding his hallucination, the unreality of which, under the temporary stimulus produced by the contact with a normal mind, will then be recognized, although the patient five minutes before believed firmly in the truth of his delusion.

Thus in one case cited by Prof. Hammond, the patient displayed much emotional disturbance under the influence of the hallucinations; when asked if he really believed the halluci-

nation, indignantly demanded if they thought he was a lunatic. Sometimes, however, the hallucinations have a stronger hold, and have doubtless an origin in disturbed cerebral circulation. Thus one patient who came under observation insisted that some one was lowering pavement blocks on the top of his head. The hallucinations generally affect sight and hearing, but in this case there were only disorders of touch limited to the vertex. Sometimes there is, strictly speaking, no delirium, but only a general dread and much emotional disturbance. These are all functional and changes in the circulation rather than changes in the nerve cells themselves, and in their nature they resemble the cases where the disturbance is due to a poisoned blood.

The forms of insanity, where every other etiological factor can be safely excluded and which can only be assigned to the influence of alcohol, have all one general characteristic, that of depression, and are closely allied in nature to those produced by other poisonous agencies. The insanity arising during a violent secondary fever of syphilis and the insanity of alcoholism, as also the insanity produced by the fungi infesting the bread-yielding plants, have all so much in common that we can safely assert that alcohol *per se* produces only the toxic class of psychoses, and that the other forms usually ascribed to alcohol are due to it but in a very subordinate degree, other causes complicating alcohol to such an extent that its action may be regarded as of small extent, perhaps rather salutary than otherwise, inasmuch as the other causes, did no alcoholism exist, would be considered sufficient to cause the insanity.

The psychological action of alcohol, which is said to very much change during intoxication the character of the individual, is a point on which there is very little dispute, yet a careful examination will, I am sure, convince one that the tendency of alcohol is to only bring out the natural character of the intoxicated individual of which more or less slight evidences are to be found during the sober condition; but which the stimulant action of alcohol brings out into very strong prominence.

The great reason for the general belief on this subject seems to me to be the lack of observation of the intoxicated individ-

nal's previous character and the great tendency of all weak-
willed individuals such as these are to throw the blame on
something which will render them less responsible. There has
been an attempt to establish a form of disease known as
inebriety. The exact basis for this disease can scarcely be de-
termined from the writings of those who advocate its establish-
ment; there is so much sentiment about it, as there is about
everything connected with this subject ; but to quote from the
lately established *Journal of Inebriety,* " a weak will-power,
coupled with a sensitive nervous system, seems to be inherited
in most cases of inebriety." This by the inebriate advocates
is construed into an irresistible tendency to drunkeness, regard-
less of everything, and it is asserted that this tendency is a
physical disease, and therefore to be treated as such. We
know of many cases of morbid impulse other than this with a
less satisfactory explanatory motive, but for that reason we do
not consider them as fit subjects for treatment in an asylum.

There are many weak-willed criminals who are nevertheless
punished for their crimes, although their parents have been
criminals and their training bad. We may put them in a re-
formatory and it is quite a curious fact, that the most success-
ful inebriate asylums have been those conducted on a reformatory
plan, as the Washingtonian at Boston and the Fort Hamilton
Home.

Prof. Hammond, at the close of last meeting of he society,
well defined this class of beings as " Drunkards," and the true
difference between the inebriate and the drunkard is only a
matter of cash.

The language of a report quoted in the *Journal of Inebriety*
is " The institution is no place for loafers, the money qualifica-
tion is enforced in most cases." Here is the great distinction.
The inebriate having money is sent to the asylum to be cured
of his disease, while the equally weak-willed drunkard being
moneyless is sent to the work-house to be punished for crime.

Any impartial observer will say that the establishment of
these asylums by the State is only a fraud on the public, and
the attempt to twist a sensible protest of the American As-
sociation of Medical Superintendents of the Insane Asylums
against the incarceration of sane criminals with victims of

mental disease, into an approval of inebriate asylums, is worthy of the cause of the inebriate curers.

The pathology, diagnosis, and treatment of inebriety appears, to a candid observer, like "a tale told by an idiot, full of sound and fury signifying nothing."

Chronic alcoholism has many of the characteristics of other nervous diseases, and so many other influences mingle in the case that, as in the acute form, it is difficult to say how much is due to alcoholism, how much to overwork, or sexual excess, or exposure, or syphilis. In all probability, did there not exist in the cerebral cortex some defect due to another cause, there would not be many cases of chronic alcoholism. That the brain of the chronic alcoholist shows very marked changes is undoubted, but who can say, but like an over-worked engine, it breaks down under an extra strain. The chronic alcoholist, if alcohol be taken away absolutely, will have a very hard struggle if he does not break down physically or mentally; a little alcohol will enable him to get along very comfortably. And the over-worked Paris literary hack, who has to keep up a constant drain on an exhausted brain, can only do this by the use of alcohol, and if he did not drink, would soon become an inmate of the asylum. In the excessive bustle and hurry is the probable cause of much that is claimed as due to alcohol.

The changes in acute alcoholism seem most extensive along the fissure of Sylvius and in the occipital lobe, as they also seem to be in chronic alcoholism due to successive attacks of acute alcoholism.

In dealing with the subject, I have given conclusions rather than extended reasoning from the facts given; these seem to me to follow so naturally that they form the only apology for this attempt to protest against the general desire to substitute wishes for exactly ascertained facts, and the great tendency to consider everything as due to irresistible impulse, thus furnishing a specious excuse for the meanest crimes.

DISCUSSION OF DR. KIERNAN'S PAPER.

Dr. E. C. Spitzka, while agreeing with Dr. K. in many important points, would ask for more definite information regarding the pathology of alcoholism. He had understood that

Dr. K. claimed the existence of two separate conditions of the cerebral cortex as the result of intemperance. In no author, ancient or modern, could a similar statement be found, and in his own autopsies, when he found any pathological condition it, was diffuse induration, due to proliferation of the connective tissue. Our late distinguished Dr. Peters had fifteen years ago called attention to the preternatural fineness of the brain in drunkards. He also had expected to hear the symptomatology more thoroughly discussed than had been the case, particularly with regard to the hallucinations and delusions of drunkards. He thought that the hallucinations could be divided into two great groups; 1st, true hallucinations, due to cortical hemispheric conditions ; and 2d, delusive interpretations of scotomic dots, due to retinal hyperæmia. The resulting figures are characterized by being small black objects, cockroaches, beetles, bats, snakes, small devils, mice and rats; while the first group represented visions of faces, large figures, buildings, etc.

He endorsed heartily Dr. Kiernan's objections to the cant indulged in by the superintendents of certain inebriate asylums —a cant which is but an echo of the teachings of some of our eastern lunatic asylums, as well as some English institutions, regarding which one of the old English alienists said that they do not deserve the name of scientific hospitals, but of boarding-houses whose boarding housekeeper was the possessor of a medical diploma.

The following case was also presented by Dr. Spitzka, which called out considerable discussion.

A case was exhibited of a peculiar reflex spasm, limited to the muscles of forced inspiration and the platysma myoides. It was rare that the action of the latter muscle could be so well studied as in the present case. These spasms were very violent in character, and their exciting cause was over-exertion in walking or speaking. Dr. Spitzka, who exhibited this patient, was of the opinion that the primary cause was an irritation of the anterior pulmonary plexus of nerves, produced by the pressure of a post-sternal tumor, which had been diagnosticated four years previously by Dr. Alonzo Clark ; he himself believed the tumor to be of a scrofulous nature, as the

dullness had become less marked, and a superficial varicosity of the thoracic veins had disappeared after a thorough course of iodide of potassium. He had not had an opportunity of examining the larynx, but the peculiar character of the voice led him to believe that the recurrent laryngeal nerve was implicated. From the fact that the platysma; diaphragm, trapezius and scaleni were involved and not the sterno-cleido mastoid, he did not believe the spinal accessory to be the motor nerve involved, but rather the cervical plexus,—even the trapezius is supplied by branches from this plexus as Claude Bernard had proven experimentally.

The irritation passed from the anterior pulmonary plexus, up the vagus to the medulla, and there was transmitted in some way to the centres for respiration. He had been informed that other practitioners who had seen this case considered the spasms as choreiform, a view in which he could not participate[1].

1. Paralysis of the left vocal cord has been since found in this case by Dr. Clinton Wagner, to whom I am indebted for the laryngoscopic examination. The patient is now suffering from hæmoptysis, probably bronchial in character.

Reviews and Bibliographical Notices.

I.—THE FUNCTIONS OF THE BRAIN.

FUNCTIONS OF THE BRAIN. By David Ferrier, M. D., etc., with numerous illustrations. G. P. Putnam's Sons, New York. 323 Pages.

(CONCLUDED FROM LAST NO.)

We now come to the most important part of Dr. Ferrier's work,—that which relates to the "functions of the cerebrum."

After pointing out briefly the nature and value of the researches of Flourens, concerning the cerebral functions, and the fresh impulse given to the study of cerebral physiology and pathology, by the publication of the views of J. Hughlings Jackson, whose influence on this book is clearly traceable, Dr. Ferrier passes at once to the subject in hand. He discusses the dictum, as to the non-irritability of the gray cortical substance of the hemispheres. In the course of this discussion, he sharply criticises Nothnagel's declaration (*Virchow's Archiv.* LVIII. P. 420), that the latter had been able to irritate mechanically, by means of a fine needle, the posterior part of the cerebral hemispheres in a rabbit. Dr. Ferrier makes it highly probable, that the phenomena witnessed by Nothnagel were really due to wounding of the corpora quadrigemina, instead of the cortical layer which in the rabbit covers those bodies.

The doctrine of the non-excitability of the cortex, as regards chemical and electrical irritation, is next briefly discussed and rejected, at least, in so far as electrical excitation goes. He admits that to Fritsch and Hitzig are due the discovery of the fact, that the cortex of the cerebrum is, or seems to be, excitable by the electrical current. He takes no further notice of the caustic strictures of Hitzig, (*Untersuchungen ueber das Gehirn,* &c.) than to express regret, that he should have been misunderstood as implying priority of discovery, or as neglectful of the work of the German experimenters. His mode of treating Dr. Hitzig, is in striking contrast to that he himself had received at the hands of the latter. The book indeed, is eminently free from the customary unpleasant features of con-

troversial writings, and the spirit more than ordinarily candid and judicial.

His own method is next contrasted to that of Fritsch and Hitzig, the latter having chiefly used the galvanic current, Dr. Ferrier, the induced or faradic. Dr. Ferrier replies briefly and reasonably to the criticisms, (many of them bitter) as to his use of the induction current, and shows that the conditions of his experiments have been partially misapprehended. Various conditions affecting the success of the experiments, are named, and among them one is mentioned, the neglect of which he declares, has misled Fritsch and Hitzig. They employed a galvanic current mostly, of a certain strength, at various times, and to various parts of the cortex. But Dr. Ferrier says: "various regions of the brain differ in regard to their degree of excitability. A current sufficient to cause decided contraction of the orbicularis oculi, will frequently fail to produce any movement of the limbs. By arbitrarily fixing a standard of stimulation which was thought sufficient, Fritsch and Hitzig, failed to elicit most important positive results of deep significance, in regions of the brain which they choose to call non-excitable.

"There is no reason to suppose one part of the brain excitable and another not. The question is, how the stimulation manifests itself. Though it is obviously advisable to use no stronger current than is sufficient to produce a definite result, *the measure of the intensity of the stimulus to be employed in each case, is the degree of definite and decided localization of effects uniformly attainable.*" (P. 130.)

He defends his mode of using a faradic current, of varying intensity, and states the mean strength of current to have been derived from 8 c. m., of the secondary coil of DuBois Reymond's instrument, and an electro-motive force equalling that of a single Daniel's cell. He compares the action of the currents and gives quite strong reasons for choosing as he did, the induced, instead of the galvanic current; but the reasons referred to are now so well known as to not require re-statement.

He replies to the objection urged by Dupuy, Carville and Duret and others as to extra-polar conduction of the current, so as possibly to excite parts to which the electrodes are not applied and thus to complicate results. But while he admits the fact of extra-polar conduction, he denies the deductions made from it. Dupuy and others have held that instead of excitation of the cortex, the current is conducted by the moist brain substance downward to the basal ganglia, especially the corpus-striatum which being thus excited, the movements follow. Dr. Ferrier says, that "incitation of the corpus striatum causes a general contraction of the muscles of the opposite side of the body, and it is impossible, by applying the electrodes directly to the surface of this ganglion, to produce localized contraction of any one muscle, or group of muscles." (P. 133.) But, while this may be a sufficient answer to the objection urged by M.

Dupuy and others, neither view represents the probable truth, as we have learned to regard it. We have, for some time past, been convinced that it is not necessary that the gray matter of the cortex, or any other part of the nervous system, should be held as excitable by any artificial means whatever. It has seemed more probable to us, that, instead of the gray matter, (cells) the *fibres* of the cortex, which pass downwards towards the basal ganglia, or which pass from one convolution or area of the cortex to another, are excited by the current, as it is well known nerve fibres can be in other parts of the nervous system, and in this way, either the gray matter of the cortex or of the motor basal ganglia is excited, not by the electrical stimulus directly, but in the natural way, by the nerve fibres, which terminate in it.

This view is, of course, perfectly agreeable to the doctrine of the localization of function in the cortex, and avoids the real objections which lie against a form of the doctrine, which makes direct artificial excitability of the gray matter one of its essential factors.

It also will enable us to explain the occurrence of the phenomena following the excitation, without supposing them to be due to extra-polar conduction, since there does not seem to be any actual proof that such is the fact. Dr. Ferrier opposes the following arguments to the hypothesis of conduction. He says; " A conclusive overthrow of the theory of conduction is the fact that stimulation of the Island of Reil, which immediately overlies the corpus striatum, causes no movements, while the more distant parietal regions at the same time react, actively and definitely to the same stimulus." (P. 134.) Also the relations of chloroform narcosis to the electrical excitation of the hemisphere and basal ganglia, are mentioned as being seriously opposed to the conduction theory. They may be also held at first sight, as opposed to the view of the case we have enunciated above, but a little attention to the case will show such is not the fact. But while Dr. Ferrier denies that the phenomena made manifest in his experiments were due to direct excitation of the corpus striatum, he admits that the results of the experiments may be, and in fact, are, complicated by "lateral diffusion and irritation of neighboring centers and tissues. This constant source of error can only be eliminated by carefully repeated experiments, and with the aid of the complementary method of localized destruction of the centers in question."

The view which we have offered above, but which is not mentioned as if novel, is also confirmed by the experiments of Burdon Sanderson, well known already to our readers. He removed the cortical motor centers in animals, so as to expose the underlying white matter, but found that the same movement could be caused by exciting the exposed white substance of the hemisphere, as followed previous excitation of the gray matter which had formerly overlaid it. Says Dr. Ferrier, " to deduce

from Sanderson's experiments, that the cortical centers are not motor, and that the movements in reality depend on the corpus striatum, is no more reasonable than it would be to argue, that, because the same muscular contractions which result from irritation of the corpus striatum, can also be excited by direct excitation of the crura cerebri, or the motor columns of the spinal cord, therefore, the corpus striatum has no motor function. Such conclusions only indicate a grave misconception of the constitution and evolution of the nervous centers. Essentially the same movements are differently represented in different centers. Many of the muscles of respiration, are reflexly co-ordinated in the medulla oblongata, are also under the control of the will, and centrally represented in the cerebral hemispheres. The motor combinations which are integrated in the corpus striatum, are again differentiated in the cerebral hemispheres, both with essentially different significations. This re-representation of the same movement in different centers, is an important guide to the true interpretation of the facts of the physiology, as well as the pathology of the cerebro-spinal system.

" The highest nerve centers cannot, however, act independently of the lower, nor can the functions of the one be understood, out of relation to the other." (P. 137.)

We have given so much space as we have in relation to the method of investigation adopted by Dr. Ferrier, and the results of the same, because we feel them to have a higher importance than has been accorded to them by many who have given them their attention.

Dr. Ferrier now passes to a description of the cortex of the brain of the monkey, more especially the convolutions and sulci of the same. We cannot follow him in this portion of his work, nor into the detailed results of his experiments, interesting and suggestive as they are. Besides, on the monkey, many experiments were made on the cortex of the brains of dogs, jackals, cats, rabbits, Guinea-pigs, rats, pigeons, frogs and fishes. The results are stated with some fulness, and also comparisons of the same are made, which it would be interesting to discuss, but we have no space in this notice, in which to allude to them. We must refer to the work itself for details and illustrative sketches. Brief remarks are added, as to the results of experiments on the basal ganglia, but we must omit any farther notice of these subjects for the present. We pass to Dr. Ferrier's discussion of his results.

He recognizes that " the mere fact of motion following stimulation of a given area does not necessarily signify a motor region. The movements may be the result of some conscious modification, incapable of being expressed in physiological terms, or they may be reflex, or they may be truly motor, in the sense of being caused by excitation of a region in direct connection with the motor parts of the crus cerebri." The method of stimulation alone, requires to be supplemented by

others, especially by that of the destruction of localized areas. In the chapters which follow is a critical discussion of the question at issue. It was the fortune of Dr. Ferrier that he was able to conduct the most of his experiments on the brains of the higher monkeys, which most nearly resemble the human brain.

Excitation of the angular gyrus, (a part of the parietal lobe), is followed, as a rule, by movements of the eyeballs, and often with movements of the head to the opposite side, etc. But Dr. Ferrier ascribes these phenomena not to excitation of a motor cortical center, but to excitation of the cortical center of visual sensations, from which the movements are supposed to be excited in a reflex way. Destruction of the angular gyrus causes, according to Dr. Ferrier. total temporary loss of vision in the opposite eye, and, if the lesion is accurately circumscribed, loss of vision is the only result observed. No motor paralysis follows. Various proofs of these statements are offered, which seem to render them quite probable.

Excitation of the " superior temporo-sphenoidal convolution," in the monkey, is followed by sudden retraction or pricking up of the opposite ear, wide opening of the eyes, dilatation of the pupils, and turning of the head and eyes to the opposite side." Upon the whole. Dr. Ferrier concludes that the movements observed are not excited by acting directly on a cortical motor center. but by exciting the cortical perceptive center for auditory impressions, and in this way rousing the animal to such action as the hearing of sounds might produce. Very interesting confirmation of this view was derived from experiments conducted on certain animals which depend much on their hearing. Bilateral destruction of the convolution in question, or of a definite part of it, appeared, so far as could be told, to produce deafness. Destruction of the hippocampal region (hippocampus major and uncinate convolution). abolishes tactile sensibility on the opposite side of the body. Dr. Ferrier appears to be fully aware of the gravity to the animal of this lesion, and has taken measures accordingly to exclude complications. To avoid wounding other parts, a peculiar method of operating was adopted, which is given by the author. To our mind. these experiments leave but little doubt as to the correctness of the inferences drawn from them by Dr. Ferrier, especially if they are taken in connection with the observations of Veyssière (*Recherches Cliniques et Experimentales sur l'Hemianesthesie de Cause Cerebrale*, Paris, 1874, etc.). Charcot, Raymond, Rendu and others.

But the facts contributed by these observers just mentioned, do not so much refer to the cortical centers of general sensibility as to the locality of the peduncular expansion of fibres, which appears to constitute the path of general sense impressions on their way to the cortex, but they nevertheless seem to support the views of Dr. Ferrier.

The *subiculum cornu ammonis*, is next referred to. " Irri-

tation of the subiculum in the monkey, cat, dog, and rabbit was attended by similar phenomena in all; viz., a peculiar torsion of the lip, and a partial closing of the nostril on the same side. This is evidently the outward expression or reflex indication of the excitation of subjective olfactory sensations of an intense character. Similar reaction is produced by the direct application to the nostril of a powerful or disagreeable odor. As a rule, the reaction was limited to the nostril on the same side, though in the rabbit both nostrils usually reacted conjointly."

This fact of the occurrence of the movements on the same side as that to which the irritated hemisphere belongs, is quite agreeable to the anatomical fact, as it seems to be, that there is no decussation of the olfactory nerves.

" Unilateral lesion of the subiculum does not abolish smell on both sides, but causes diminution of smell on the one side; viz., the side of lesion,—a fact which disproves the decussation of the olfactory paths in the anterior commissure." The general result of the experiments was the " localization of the centers of smell and taste in close relation to each other, in the lower portion of the temporo-sphenoidal lobe." This localization by experiment Dr. Ferrier thinks, receives strong confirmation from certain clinical phenomena which have been observed, especially in cases, where, in consequence of severe blows on the vertex, there has been loss of the senses of smell and taste, and in which there was reason to believe there had been lesion of the parts in question. Under this head, the discussion is a very interesting one, but we have no space in which to examine it.

Next in order, the results of an experimental investigation into the functions of the occipital lobes, are given. No positive results of value are arrived at. But Dr. Ferrier says, "*negatively*, however, this method of experimentation establishes facts of the utmost importance."

" The removal of the occipital lobes is without effect, on the faculties of special sense, or the powers of voluntary motion." " The functions of circulation and respiration are likewise unaffected." But " after removal or disorganization of the occipital lobes, the appetite for food is abolished, the animals refusing that which formerly they exhibited a great liking for. This I have tested in various animals and various ways" One case, however, occurred in the course of his experiments, which tended, apparently, to overthrow his conclusions, as to the occipital lobes being the cerebral seat of hunger. But, says our author, " notwithstanding the recovery of the one animal, I am clearly of opinion, from an extensive observation of the effects of localized destruction of various regions of the cerebral hemispheres, that there is a causal relation between the removal of the occipital lobes and the annihilation of the appetite for food."

But, he continues, on another page, " I am far from regarding the evidence adduced in support of the relation of the occipital lobes to the visceral sensations as of the same weight as that

relating to localization of region of special sense. Farther investigations by new methods, aided by careful clinical and pathological observations, may serve to throw more light on a very obscure subject." (P. 197.) Under this head, some very interesting and suggestive remarks are made in regard to the clinical relations known to exist between abdominal disease, and various states of the abdominal viscera, and the condition of the brain and the emotions, and *vice-versa*. We do not know of a more fruitful and practically interesting inquiry than the one now indicated, but we dismiss it for the present, promising to return to it specially at some other time. Incidentally, Dr. Ferrier was able to observe, that injury to the posterior cerebral lobes did not abolish the sexual appetite.

But, conjecturally, he locates the sexual appetite in " the occipito-temporal convolutions, or those connecting the lower and inner part of "the temporo-sphenoidal, with the occipital lobe." * * * * "The value of this hypothesis I leave" says he, " to be tested by further physiological and pathological research."

Next in order is a consideration of the comprehensive subject of cortical " motor centers." Most of the decisive experiments were conducted on the higher monkeys, whose brains most nearly resemble the brain of man. The " motor zone," as it has been called, lies in the cortex partly of the front and partly of the middle portion of the brain about the fissure of Rolando. This part of the cortex appears to be clearly connected by means of descending fibres with the motor masses in the basal system, comprised in the corpus striatum and nucleus lenticularis, or extra ventricular portion of the corpus striatum. But this subject has been so extensively noticed and discussed in all its phases, in the pages of this JOURNAL and others, as to render a detailed review unnecessary at this time.

Dr. Ferrier endeavors to meet, in this work, many of the objections which have been made, not only to the general doctrine of the localization of function in the cerebral cortex, but also those made to his own researches, whether as regards his methods, or his deductions, and upon the whole, he succeeds very well in every way, in attaining his object. But to adequately notice these at length would require a long article. It seems to us that something would be gained and nothing lost by Dr. Ferrier, if he would abandon his position in regard to the direct excitability of the gray matter by artificial means, in favor of that of the excitability of the fibres in and beneath the cortex, the case would be simplified, especially in avoiding a troublesome though not a fatal objection.

In this part of the work it is, that Dr. Ferrier endeavors to answer the various objections made to his experiments, and his conclusions from them.

To the objections drawn from experiments made on animals lower in the scale than monkeys, he replies that the constitution of the cortex is so much more complex in the latter as com-

pared with the rabbit, cat or dog, as to make comparisons difficult, and, critically speaking, sometimes impossible. Homologous parts must be selected in different animals, to render the experiments truly comparable, but such parts cannot always be pointed out in the animals most often experimented on. Adequately to criticise, therefore, experiments on the monkey should be principally used as the subject, in the one case as in the other. But this has never been done by any one as it has been done by Dr. Ferrier. No one, so far as we know, can lay claim to having conducted such extensive researches on these highest of all mammals next to man.

In general, it is contended, that the lower down the animal is in the scale, as regards the perfection and complexity of the nervous system, the less marked are the distinctions of cortical areas, and the more capable are the subordinate centers, of performing the offices apparently devolving on the cortex in the higher animals. Various circumstances, it is said, must be taken into the account, and duly considered, to make the researches conducted on one species comparable with those conducted on another species.

Then the facts of carefully observed cases of paralysis, including *post-mortem* results are invoked to support the doctrine of motor cortical centers. The objection that destruction of the supposed motor centers in rabbits, cats, dogs, etc., does not lead to permanent paralysis of the related groups of muscles, is met by the statement that paralysis, or at least, impairment of motion, *is* practically permanent in the monkey. Then, in cases where only one center is destroyed, and the other in the opposite hemisphere is left intact, the movements may yet occur, on the principle enunciated by Dr. Broadbent, that is, that of bilateral actions of the members or trunk, excited from either hemisphere, on account of the association of the two halves of the cord, medulla, etc., by means of commissural fibres.

He rejects, and we think, wisely, the law of *functional substitution* of Carville and Duret (*loi de la substitution functionelle*), or the *loi de suppléance* of Flourens, Longet and Vulpian, which assume if one part of the cortex is destroyed, that some other part may, and, in point of fact, often does, vicariously assume its functions. "Such a mode of interpretation," says he, "seems to me not a whit more justifiable than the supposition that the organ of sight might take up the functions of the organ of hearing, or that a *nerve* might at one time be a motor nerve, and at another a sensory nerve, or perform both functions at once."

We entirely agree with the principle embodied in these remarks, and yet must object to the illustration offered of it. Dr. Ferrier clearly implies, in the latter part of the quotation just made, that a *motor* nerve could not convey impulses as a *sensory* nerve, or might not convey both kind of impressions indifferently. But though the subject is still in a state of uncertainty,

scientifically speaking, yet it seems to us that the evidence, such as it is, all looks in one direction, that is to show that motor and sensory nerve fibres are structurally the same, and might possibly be substituted the one for the other. The differences, it is probable, do not lie in the nerves themselves, but in their peripheral and central apparatuses of termination. This view would seem to be supported in some measure, by certain experiments of MM. Philippeaux and Vulpian and later of Bert. But to pass on: The *real motor* apparatus, in the lower animals, more particularly, lies below the cortex, in the basal system, and subordinate portions of the cerebro-spinal axis.

The region of automatic movements, even of the highest character, whether acquired or not, lies in the main, below the cortex. In the opinion of Dr. Ferrier, " the corpus striatum is the center in which movements primarily dependent on volition proper, tend to become organized" and hence automatic.

The opinion of Nothnagel, that the affection of motility which results from injury to the so-called motor centers, is due to paralysis of the muscular sense, is rather carefully examined. Also the opinion of Hitzig, who held that the disorder of motility in question is due, not to destruction of a cortical motor center, or of the path from mind to muscle. but on the contrary, destruction of the path from the muscle to the mind. This opinion does not differ essentially from that of Nothnagel.

He also canvasses the opinion of Schiff (*Archiv f. Exp. Pathologie und Pharmacologie, Bd. III., 1874, p. 171*), who has held that "the disorder of motility is essentially an ataxy dependent on loss of tactile sensibility." In reply, Dr. Ferrier says that "the facts of localized convulsions in man from irritation of the cortex completely dispose of this supposition. For it is found that in those cases in which limited convulsions occur, the movements are not preceded by or associated with any sensation further than that which accompanies the violent muscular contractions. But the most conclusive proof of the untenability of Schiff's hypothesis, is the fact that sensibility to touch, pain, &c., is absolutely unimpaired after destruction of those centers. * * * *   *   *   Schiff argues that because animals in which the cortical centers are destroyed, *resemble* in their gait the movements of animals in which the posterior columns of the cord are divided, and because in the latter case the ataxia is due to diminution or abolition of the sense of contact, therefore the cerebral ataxy is due to a similar cause. Mere resemblance, however, is not identity, even though we were to admit the fact of resemblance to the fullest extent. The resemblance utterly fails in the case of a monkey, where there is no ataxy. but complete paralysis, and the phenomena in the dog, which Schiff relies on in support of his view, are clearly due to a paresis of movement not amounting to complete paralysis, a condition which has already been accounted for." To the views of Nothnagel and Schiff, Dr. Ferrier opposes a direct denial of the fact, as to the

loss either of the "muscular sense" or "muscle consciousness"
(Hitzig). Dr. Ferrier, indeed, seems to doubt the existence of a
muscular sense. But we cannot share his doubts. He does not
seem to have noticed the anatomical researches of Sachs, which
clearly show the existence of sensory nerves as belonging to the
muscles. But this question is not without its difficulties, and
we cannot in this place enter on its discussion.

Dr. Ferrier next turns to the "antero-frontal regions of the
brain." electrical irritation of this portion of the brain was at-
tended as a rule by negative results. " Removal, or destruction
by the cautery, of the antero-frontal lobes, is not followed by
any definite physiological results." But to this statement the
following exception is made: Animals thus operated on, "while
not actually deprived of intelligence, *had lost, to all appearance,
the faculty of attentive and intelligent observation.*"

In another chapter the discussion of this subject is resumed,
and we will not further notice it now any more than to say, that
no positive conclusions as to the functions of the parts in ques-
tion, was reached by experimental means.

In a note to Chapter IX., Dr. Ferrier gives attention to the
singular views which have been enunciated the past few years,
by Dr. Brown-Séquard, in regard to the physiology of the brain,
and which have been fully noticed in the pages of this
JOURNAL. Dr. Ferrier very properly rejects, as we have done
from the first, Dr. Brown-Séquard's "cases," as not satisfying
"the requirements of scientific evidence in a question of this
kind," also the views of Goltz (*Pflueger's Archiv.*), which we
have formerly noticed at length, he rejects as not having been
adopted, in view of our author's later and fuller experiments on
monkeys.

His experiments seem to have been thoroughly conclusive as
to the effects of the complete destruction of the corpus striatum
on one side, that is, there was complete loss of motility on the
opposite side of the body. His experiments in regard to the de-
struction of the optic thalami were numerous, and of such char-
acter as to lead him to declare, that "to assert in the face of
these facts that sensation can still continue, notwithstanding
the total destruction of the optic thalami, both cells and medul-
lary fibres, is to assert nothing less than a physical impossibility.
* * * * * * The recorded instances, there-
fore, of so-called destruction of the optic thalamus without loss
of sensation, must be put down as only partial lesions, and un-
less more satisfactory evidence is brought than the mere naked
eye appearances, of the optic thalamus in such cases, they can-
not for a moment be placed against the positive cases, now nu-
merous, in which loss of sensation has been demonstrated to re-
sult from lesions situated in this ganglion."

He disputes outright, on experimental grounds, the declaration
by Nothnagel, that rabbits, in which the optic thalami had been
destroyed did not differ very noticeably from the normal. We

have no doubt that Dr. Ferrier is substantially correct in his general conclusion that the optic thalami are in the path of sense impressions, on their way toward the cerebral cortex. Indeed, we deem any statement wholly aside from the truth of the case which denies that the total destruction of the optic thalami, is not attended by derangements of function. Is it possible that such a complex and central mechanism as the optic thalamus, is devoid of functions, as it must be to make its destruction productive of no symptoms ! It is simply impossible. And yet some such conclusion would seem to have been deduced from Nothnagel's experiments. This chapter is concluded by a remarkably clear and judicious general summary of the functions and relations of the corpora striata, and optic thalami, and which we would be glad to quote in full if our space would permit. But we can do no greater service to our readers, who are seeking information on this subject, than to refer them to the work itself.

They are the seats, according to him, of the higher acquired automatic movements, and they lie below the domain of consciousness, and are, hence, "outside of the sphere of psychical activity proper."

We will close our notice by a brief glance at the chapter entitled, " The Hemispheres psychologically considered." He clearly admits two phases as belonging to this subject—*objective* and *subjective.*

He says that "in their subjective aspect, the functions of the brain are synonymous with mental operations, the consideration of which belongs to the science of psychology, and the subjective method of investigation. *No purely physiological investigation can explain the phenomena of consciousness..*" With this declaration we most heartily agree. He adopts the opinion of Bain, who says when "we speak of a mental cause, a mental agency, we have *always a two-sided cause;* the effect produced is not the effect of mind alone, but of mind in company with body."

But it will not be possible for us to notice adequately the chapter now under consideration. It is filled with suggestive remarks on the organic conditions of mental states and actions, with most of which we can coincide. But the discussion of many points in regard to the physiological psychology of the brain, are waived until the appearance of another work by the author, which it is his purposes to consecrate to "Diseases of the Brain."

The remaining two chapters are devoted to a "diagrammatic summary" of his views in relation to the mechanism of the central nervous system, and to "Cerebral Topography." They are both highly interesting, but cannot be noticed in this place. We had intended to have given some space to a statement of our own views in regard to the functions of the brain, which do not very materially differ from those held by Dr. Ferrier, but will reserve such a statement until another occasion.

In closing our notice of Dr. Ferrier's book, we cannot do so without expressing our high sense of its value. Its spirit is admirable. The style is simple and clear. The thought is healthy and unusually correct. One of the best features about the book is its *suggestiveness.* On the whole we can safely pronounce it the most valuable contribution by any one individual to the physiology of the brain, made since the publication of the researches of Flourens. And this judgment is passed after a critical comparison of this work with that of Dr. Hitzig's, which was reviewed at length in an earlier volume of this JOURNAL. In its production, Dr. Ferrier has taken a high rank among living physiologists, and is secure of being assigned an honorable place in the history of physiology. We shall look with much interest for his promised work on the "Diseases of the Brain."

II.—PUTNAM-JACOBI: REST FOR WOMEN.

THE QUESTION OF REST FOR WOMEN DURING MENSTRUATION. The Boylston Prize Essay of Harvard University for 1876. Illustrated. By Mary Putnam-Jacobi, M.D. New York. G. P. Putnam's Sons. 1877. 202 pages.

In a prize essay one naturally looks for extraordinary merit, and in the essay before us we find it. Its two hundred and thirty-two pages are replete with prize thought, in reply to the inquiry, "Do women require mental and bodily rest during menstruation?"

The first of the six sections is introductory and historical, the first subject considered being that of sex as a limitation. The development of sex, as is well known, is a mark of progress from a lower to a higher order of life, whether vegetable or animal. But the author shows that this law has hitherto been applied, not to sex as such, but to the *kind* of sex, and that kind is the masculine. "Not the accidents of the sex or the abnormal exercise of its functions, but the *sex itself* is regarded as a pathological fact." The medical literature on the subject abounds in such expressions as "morbid condition," "natural infirmity," etc. "The morbid effects of menstruation," according to the authorities, are manifold and grave, even to the extent of "temporary insanity," as Storer would say.

Under this head of "morbid effects," the author shows that the modern doctrine of King, viz., that menstruation is a disease of civilization, had its origin with Roussell, as early as 1805. It is, as the author of the essay states, quite in contrast with

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the "plethoric theory," held from the most ancient until comparatively modern times. The first advocates of the plethoric theory considered menstruation analogous to the other spontaneous evacuations, and that the body was purified thereby—the uterus being the sewer of the system, according to Kreiger. Hippocrates explains the plethora by the loose texture of the flesh in women, so much fluid being absorbed by these loose tissues as to make an evacuation necessary, whereas, in men the excess of fluid is dissipated by muscular exercise. Boerhaave and Haller hold that at the time of puberty more blood is manufactured than is needed for individual growth. Burdach makes it depend upon an excess of formative power. We dwell upon this plethoric theory, because it is a modification of this theory that is adopted by the author.

We consider this whole history of great importance. In the first place, we seldom find one so complete and so well stated, and then it is important, as showing the great difference of opinion of great authorities concerning a question of such vital importance, involving, as it does, the whole question of reproduction. As to the source of the menstrual blood, Wagner considers it the formative material destined for the development of the embryo, and that its weight during ten months is exactly the weight of the fœtus at term. This is not exactly true, and yet, in a general way, according to the argument of the essay, pregnancy does represent the integral sum of the suppressed menstruations. To go back of this again and ask the source of this excess of formative material, Astruc answers that it is that material, which, previous to puberty, has been used for growth.

In short, from Hippocrates to Burdach, this more or less modified theory of plethora prevailed, and the excess of nutritive force was accounted for by the cessation of growth and the deficiency of muscular force. Not till 1845, the writer affirms, the time of the discovery of ovulation, were these conditions of nutrition dropped out of the explanation of the phenomena. And now, for the first time, the *periodicity* of menstruation is considered a morbid process, unlike any other physiological process of the body. Under the head of dangers attending this physiological process, we find a formidable list. To use the author's language, "One of the most essential apparent peculiarities of the menstrual process, its periodicity, that formerly was supposed to indicate a periodical increase in the vital forces of the female organism, has come to be considered as a mark of constantly recurring debility, a means of constantly recurring exhaustion, demanding rest as decidedly as a fracture or a paralysis."

Naturally enough, the next question considered is women in industry. For, if it be true that menstruation is an infirmity requiring rest, then justice to women demands that the industries of women should be arranged with reference to this necessity. But what do we find? Women working throughout the

world, without any attempt to secure rest. It seems that the
ideal society of the poet, " where men must work and women
must weep," has never been realized, but that both must work,
has been realized. According to Beaulieu, quoted by the au-
thor, the man has never been able to provide for the family and
leave the woman to look after the house and the education of the
children. The workshop of Europe is a most ancient institu-
tion, frequently managed by women. It seems, too, that in
" the trades corporations, contrary to public opinion, were ar-
ranged to include women." In Great Britain, three-fourths of
the unmarried and one-seventh of the married are engaged in
independent or isolated labor, to say nothing of those who assist
in various directions. In the United States, one-sixth of the
entire female population are laboring in the paid industries of
the country.

This does not include women who are classed as married or
housekeeping. The most eminent philanthropists and hygien-
ists, such as Simon and Hirt, declare it "impossible and un-
necessary to attempt to frame regulations in accordance with
the supposed exigencies of this physiological process." "A his-
tory of female labor would be a history of industry itself," says
our author ; yet it may be the habit of the world is all wrong
in this regard, as it has been in many another. Be that as it
may, the fact exists that modern society can not "yield to na-
ture her inexorable demand for rest during one week out of
every four, in the adult life of women."

Under the head of importance of rest, the author says truly,
it is difficult to ask the question with precision. Granting rest
is necessary, we might ask, for what purpose is it necessary?
The preservation of life? Certainly not : for everywhere and
in all ages women have continued to work and to live. But
perhaps it is " necessary for the attainment of a higher stand-
ard of health, or the avoidance of certain disease." If so, how
necessary? Is it as necessary as sleep? If so, persons should
suffer as much for the want of one as for the other. It should
be shown that lack of this rest, as lack of sleep, is sufficient to
deteriorate the health in the absence of all other complications,
and the degree of health attained ought to correspond with the
amount of rest obtained.

Then comes the question of duration. How long is it neces-
sary to rest? Here the author finds a singular discrepancy be-
tween the claims of theories and the habit of practice. It is
not found that the degree of rest required increases with the
severity of the occupation. The author suggests that it may
be possible that the alleged inferiority of woman's work is due
to this infirmity of the sex, and that complete rest would raise
the quality of woman's work to the level of man's, as suggested
by Dr. Clarke. This question of rest, then, is one of profound
import, and well deserves the serious, careful investigation the
author has made.

The second section is devoted to statistics. First of all, there is shown to be a great difference in statistics. It is easy to answer the question pain or no pain, as Brierre de Boismont has answered it, but to compare the absence or presence of pain with family history, occupation, age, etc., is quite a difficult task ; one which the author has accomplished most creditably.

A thousand tables were prepared, comprising ten questions bearing upon hereditary. studies, occupation, exercise, duration of school-hours, etc. Two hundred and sixty-eight answers were received, and the tables subjected to close analysis and comparison, after which the summary of the results is given as follows:

1. Thirty-five per cent. of the whole number of cases are completely free from even discomfort during menstruation ; while fifty-nine per cent. are not troubled sufficiently to interrupt their daily avocation. As regards the forty per cent. who do suffer, rest is as desirable as during any other kind of pain.

2. Of the painful cases, fifty-three per cent. were such from the beginning, while forty-six per cent. were acquired. The author points out the importance of this distinction with reference to occupation. No occupation begun after the establishment of the disorder, could possibly be the cause of the disorder; hence, study at school is the only occupation that can be considered a cause.

3. Eighteen per cent. of the class (pain) received very little education ; while of the class (painless) none are so classified. The average for beginning school is younger in the class (pain). No reliable conclusion can be drawn from the average age of leaving school, as it is about the same for both classes. But the proportion of those who pursue advanced studies beyond the age of twenty-two, is sixteen per cent. in the first class (painless), and only eight and a half per cent. in the second class (pain). Hence, the important deduction, the highest education is most favorable to menstrual health: while the ornamental education is most favorable to menstrual disease. (The ornamental excludes Latin and the higher mathematics.)

4. It is demonstrated that the majority of all the cases have too little exercise during childhood : but the class who never had pain exercised a great deal more than the other.

5. The contrast in family history is remarkable, being good in $63\frac{2}{3}$ per cent. of those who have healthy menstruation, while it is good in only 38 per cent. of those who have diseased menstruation, whether congenital or acquired. Thus is family history made accountable for what otherwise might be charged to occupation.

6. Capacity for exercise was nearly always in inverse proportion to the habit of pain.

7. Persons without occupation suffer at menstruation in a much larger proportion than those who are occupied. " Social life," ornamental education and celibacy seem to accompany each other.

8. Marriage is much more opposed than celibacy to menstrual pain.

As to the question of rest, the author finds it impossible to decide its influence in preventing suffering, because, according to the tables, the habit of resting was only acquired after the pain was established.

From these statistics it is proved that freedom from menstrual suffering is in proportion to

1. The vigor of childhood and family health. 2. Degree of exercise during school-life. 3. To the thoroughness and extension of mental education. 4. To the general health and capacity for exercise maintained after school-life. 5. To the steadiness of occupation. 6. To marriage at a suitable time.

The most important question—the question of the influence of rest—is unanswered, except that in a large majority of cases rest was superfluous.

These deductions are valuable according to the estimate which is placed upon statistics in general. Some great authorities, such as Trousseau, have little faith in conclusions derived from such data. They look upon statistics as empiricism formulated. We can only say that the writer of the essay has used the statistic line of argument in the fairest possible way, and a study of the tables will well repay one who is in search of models in that line. The sifting process of analysis and comparison is most thoroughly accomplished.

This summary, as the author states, suggests several questions which are answered after discussing the theories of menstruation and deciding upon the one most in accord with the facts which have been deduced. The principal question, which is also the principal question of the essay, is whether there is any thing in the nature of menstruation that would make rest necessary, even without pain. Of course the answer depends upon the theory of menstruation.

Of these theories there are three; viz., the theory of plethora, the ovulation theory and the third, yet nameless, which makes the uterus, not the ovary, the seat of the changes—the cause being the preparation for pregnancy.

The first point discussed is the origin of the ovary. The author seems to consider it unquestionable that the ovary arises from the hypoblast. But Balfour's papers on the development of Elasmobranch Fishes in the English *Journ. of Anat. and Phys.* '76–'77, seem to prove unquestionably that the entire urino-genital system of vertebrates arises from the middle layer, and that the existence of the mesoblast is a demonstrated fact, though its ultimate derivation is one of the "burning questions of modern embryology." Prof. Allen Thompson, in his inaugural address before the British Association, '77, gives a very plain statement of the question as it now stands, and seems to entirely agree with Balfour. Had we space, we should like to quote Prof. Thompson's words on this point, they are so lucid.

The second question that presents itself in searching for a theory of menstruation relates to the origin of the ovum. Is it one of the endothelial cells of the Graafian follicle, or is it an epithelial cell which has migrated hither from the epiblast? This is an important question, inasmuch as it is found that ultimate derivation has to do with function. We should say the origin of the ovum is still an open question, and so far as the "philosophical necessity" urged by, and quoted by our author, is concerned, viz., that the reproductive cell must be an epithelium because of its capacity for growth, it seems to us the same philosophy better applies to the formative cells of the middle layer. The possibilities of these cells are almost limitless, in formative power they rank the highest. To quote from Prof. Thomson's address: the exact seat of the origin of the reproductive cells is still a matter of doubt; they appear in the parental body at a very early period of its development, and clearly derive their origin from a deeply seated part of the formative cells which are undergoing transformation into the primitive organs. If this means anything, it certainly means that the ova are derived from the middle layer.

It is well established, as the author shows, that not only the follicles, but the ova also are matured before puberty, so that the function of the ovary is not a function of adult life only, but of childhood as well. The point to this fact is the absence in childhood of the great sexual disturbance which the theorists attach to the functional activity of the ovary.

The ten laws of Pouchet are next reviewed, and objections are made to the following; viz., that the shedding of the ovule is marked by periodical surexcitation of the genital organs; that the menstruation of women corresponds to the phenomena of excitement manifested at the mating season in certain animals, and that fecundation is in constant relation with menstruation.

Pflueger's theory, viz., that the gradual accumulation of sexual irritation in the ovary finally determines by reflex transmission the afflux of blood to the uterus and ovaries, which constitutes the flow, is essentially the theory of Prof. Jewell. *Jour. Nerv. and Mental Disease*, April, 1875. The explanation of the nervous mechanism by which the process is effected is, we believe, original with Prof. Jewell.

Our author does not dispute the periodical hyperæmia, but she does dispute the sexual element in ovulation. It, ovulation, is essentially a process of nutrition which does not call into action any special nerve centers any more than does any other nutritive process of the body. The sexual part of reproduction is inconstant, it may or may not take place, but the nutritive part is constant, dependent upon the internal nutritive powers, and not upon external circumstance.

While admitting the hyperæmia, the author affirms that the rupture of the vesicle must be considered the "*consequence*, not the cause of the rise in vascular tension."

Rouget's theory is next reviewed. Condensed it is the theory of erection, demonstrated principally by experiment on the cadaver. The objections made by the author are well nigh unanswerable. We have not space to quote them. The fifth statement, viz., that "erectile tissues, belonging to the sphere of animal life, are developed from the animal layer of the blastoderm, or epiblast, while the generative intestine of the women is derived from the nutritive layer or hypoblast," is a questionable statement. We believe that all the parts that go to make up the erectile tissues, such as blood vessels, connective tissue, etc., come from the middle layer, as does the entire generative intestine of both man and woman. We would say, in passing, that the main reason why the independent existence of the mezoblast is questioned, is because it is not found in the lower animals. It is not found simply for the reason it is not needed— the highly differentiated tissues are not found in these animals. All of this class of tissues spring from the middle plate.

· The author next asks the very pertinent questions why the blood flows *to* the utero-rovarian plexus, and why it flows *from* the uterine mucous membrane. She reviews the theories of Pouchet (lax tissues), Kundrat and Williams (desquamation), and discards them all after answering them by good objections. The author's answer is that there is a necessity in the female economy for the periodical evacuation of a few ounces of blood— necessity so profound that if the ordinary mode of exit be closed the evacuation will, nevertheless, be effected elsewhere. There is in woman an excess of nutritive force and material, which, when not utilized in reproduction, is expended in menstruation.

While this is all very clear, we do not see that it answers the question why the blood flows to these parts in preference to other parts. We suggest that this general blood pressure might be relieved through the medium of the splanchnic, the same as in increased tension from other causes. The heart and great vessels are relieved by the vascularization of the abdominal and pelvic viscera, through the splanchnic division of the vasomotor system. Possibly we may have overlooked the author's explanation of why the blood is determined to the genital organs.

We would say, in passing, one of the greatest faults of the book is the lack of a complete index. There is frequently so much space between the asking and the answering of the questions, owing to the complicated and very exact investigations made by the author, it is difficult to bring the questions and answers together. A good index would be a great assistance.

In answer to the question why the blood flows *from* the uterine mucous membrane, the statement is made a good way farther along that the tension becoming excessive from the accumulation of blood the closed system of the circulation gives way at its weakest point, viz., the fatty degenerated uterine decidua.

With the hypothesis that the menstrual period represents the climax of nutritive force and material, the author proceeds to trace the rhythmic wave by the excretion of urea, the tension of the arterial system, the rise of temperature, etc. From the tables given the deductions are that for a few days preceding menstruation the amount of urea excreted is increased above the average of the inter-menstrual period ; this excess diminishes during and markedly decreases after the flow.

The next measurements are of the pulse and temperature. The rise and fall of the temperature corresponds pretty well with the amount of excretion of urea. The pulse is not constant, but the assertion that the pulse is lowered is not justified. The third measurement is that of muscular force, both by the hand dynamometer and by the lifting of weights. The measurements taken justify the conclusion that the week preceding menstruation is one of increased muscular strength.

The next measurement is that of arterial tension—sphygmographic traces of the radial artery. The conclusion is that there is a rhythmic wave of plenitude that reaches its maximum from seven to eight days before menstruation, and has its minimum from one to four days after menstruation.

The cause of this rhythmic wave is attributed not to the heart nor to any obstruction, but to an increase in the mass of blood, and good reasons are assigned. It is characteristic of the essay that no theories are discarded and none are advanced without reasons, not merely one or two but a very host.

We would call especial attention to the distinction the author clearly makes between " irritative rise of tension," a pathological condition, and the physiological type in which the tonicity of the arterial wall is increased along with the increase of pressure—hence no general nervous disturbance as in the other type. In regard to these experimental investigations too much can not be said in praise, they manifest the thorough painstaking scientific spirit. We can but regret with the author that the measurement of the blood corpuscles and of the carbonic acid could not be added to the list, but we are glad to know that they are promised.

Whence is this nutritive material obtained ? The author's answer is, from the motor system, whether nerve, bone or muscle. The argument is at puberty the system begins to refuse a certain amount of nutrition, which constitutes a margin for reproduction. The ground being taken that the cost of reproduction is greater in woman than in man, and the excess in this direction means a deficiency in some other, viz., the motor apparatus. Hermann's theory that the cost of reproduction is the same for both sexes, is merely mentioned ; but we think it worthy of much consideration. If, as our author affirms, and as is well known by all physiologists, the generative organs correspond in number and function, why should there not be the same correspondence in generative force ? The author makes

the statement, also, that women eat less than men. From our own personal observation, we should say this is not true. We believe one of the effects of a normal pregnancy, is to increase the appetite ; and our observation has been that, ordinarily, women eat as much as, if not more, than men. At all events, we think this point well worthy of investigation. Special attention is called to the statement, that while the quantity of motor force, nervous or otherwise, generated by women is less than in men, the tone of the motor system must be just as perfect, the elaboration of the structure of the motor apparatus is in no way inferior, the anatomical conditions of sensibility and thought remain the same. Here is an apparent, if not a real contradiction. How can we reconcile this complete condition of one part with the deficiency of the other? We find further along that the author follows out and applies the logical conclusions of her reasoning, based upon this motor deficiency: "As the characteristic bodily deficiency is lack of muscular strength, so the characteristic mental deficiency should be lack of power of attention. The mental act of attention, requires processes in nerve centres closely analogous to those which precede muscular action, if indeed the motor centres themselves be not always called into activity." This agrees with another statement ; viz., that *all* intellectual actions require muscular actions for their expression ; and both statements agree with the argument of the essay; viz., that the motor system of women is inferior to that of men ; but the whole argument contradicts the statement of the author that the organs of sensibility and thought are not encroached upon. If thought be in any way dependent upon motion, and motion be lacking, how can thought or its organs be complete ? It seems to us that the application of the argument can but prove the mental inferiority of women. It is a fine theory to say that muscle force and mental force correspond ; and yet what is really the history of mentality as compared with muscularity? We are not prepared to say. We merely make the suggestion. Yet we do know that there are notable cases among men and women of great intellectual power with great muscular weakness.

Because of this lack of power of attention, and back of that again lack of motor power, the *difficulty* of woman's work bears no kind of proportion to its effect upon the health. The remedy suggested is not to decrease the amount of work, either mental or physical, but to interrupt the work frequently. The statement that eight hours work would cause less fatigue in two sessions than in one is as true of men as of women. It is not so much the amount of work that fatigues as it is the number of consecutive hours one works. The "breaking down" so common among both sexes, is largely due to long uninterrupted hours of work. The author found the same thing to hold true in school-life ; not the prolonged but the uninterrupted hours of study told upon the health.

Another contradiction to this sound mind in sound body question, is the peasant woman. More muscular even than her husband, why should she have imperfect nerve centres?

The author explains it by saying, the nutritive balance is disturbed. We suppose by this is meant that there is a physiological type for the motor system of woman. If that is over-developed it is done at the expense of the nerve centres, which centres can not bear the strain of the rise in vascular tension at such menstrual period. The peasant woman's muscular strength then is really pathological and not physiological. It is as much beyond as that of most women is below the normal standard.

The writer makes the distinction between individual and supplemental nutrition very clear—the one is for the use of the individual, and the other for reproduction, and that in general the supplemental will be maintained even at the expense of the individual, hence the most delicate women continue to bear children. The function of menstruation belongs to vegetative rather than animal life, and is analogous to the provision made at the nodes of plants for the development of branches.

Under the head of muscular functions the author states, " It has been demonstrated that the contraction of muscles during exertion is attended by an increased excretion of carbonic acid, while the excretion of urea remains the same. This is positive proof, and is accepted as such by all physiologists, that the heat required for the production of motor force is not derived from combustion of the albuminous parenchyma of the muscle, but of the substances, principally hydrocarbonaceous, contained in its juices." We must beg leave to differ with the statement that this is accepted by all physiologists. Prof. Austin Flint, Jr., has certainly earned the title of physiologist in its best sense, and his valuable experiments on this question, so well known to all physiologists, at least suggest that the question is still an open one. Still another contribution from Prof. Flint on this same point is promised (in the English *Journal of Anatomy and Physiology*, if it has not already appeared in the last issue not yet seen by us). The effects of muscular exertion are not positively proved, Pavy to the contrary notwithstanding. The theory of Liebig. viz., that the tissue evolving force was itself oxidized, has been disputed ever since it was propounded, but continued to be accepted till overthrown by Fick and Wislicenus, in their famous ascent of the Faulhorn. In 1871, the theory was reinstated again by Flint, in his observations on Weston. Pavy, in his observations on the same pedestrian, is attempting, since 1876, to overthrow this theory of Liebig and Flint, which the latter is attempting, as earnestly and scientifically, to maintain. Thus the question stands.

With regard to the nutrition of the nerve centres, the author adopts the hypothesis of Wundt,—dividing the process into "negative or internal work" and "positive or external work."

We agree with the statement that there is a great deal of

loose thought about the "waste of nerve substance," "defective nutrition of the centres,' etc., and we are almost forced to believe that there is such a thing as "functional" disease of the nervous system without organic disease; but we are hardly prepared to conclude there is no parallelism between the activity of the nutrition or function of nerve centres, and of muscles. The law of the correlation of motions which is maintained throughout the essay, is violated in this statement.

We cannot see that the chemical process of muscle nutrition is analysis, while that of nerve nutrition is synthesis. Is it true that muscular tissue is simpler in chemical structure than the albuminoid substance that feeds it? We supposed that any tissue was not only physiologically and histologically, but also chemically, higher, more complex, than its elements. The "negative" and "positive" work of Wundt, are only other names for potential and active energy, and are as true of muscle as of nerve. Is the process of muscle repair a simple oxidation? We know this is true of muscle waste, but we did not know that the two processes were identical. Indeed we did not know that the great problem of assimilation had been solved at all.

Having thus considered the arguments, had we space, we should like to restate the questions as asked and answered by the author; but we must refer the reader to the essay itself for this. We can state but one—Is there anything in the nature of menstruation that should lead us to expect a necessity for physical and mental rest, even when no pain is experienced? the capital question of the essay. "There is nothing in the nature of menstruation to imply the necessity or even the desirability of rest for women where nutrition is really normal."

According to the author's own showing, the equilibrium between the supplemental and the individual waves of nutrition is such an uncertain affair, that Pallen's "neuric causes," or as the author terms it, "an epiphenomenon" may as well be admitted in fact if not in theory. If it cannot be shown that there is an equally delicate equilibrium in the other sex, whose disturbance is attended by consequences just as disastrous, then is it true that woman is unequally burthened, and the extra provision which nature makes for the extra cost of reproduction, is made null and void by the conditions imposed—conditions almost impossible to fulfill. Admitting all that the author claims, the very fact that nature has made such a fine point in balancing these reproductive and individual forces, makes woman an exceptional being. Yet, we consider the author has made a valuable contribution to science on this question of menstruation. Not only that, but it should prove a great boon to the race. The minds of women should be disabused of the hereditary sentimentalism that often surrounds this physiological function, converting it into a disease. The writer has clearly shown that much of it is morbid, as in the class that are disappointed in marriage and have no occupation.

We wish that a clear statement of the argument and the conclusions might be placed in the hands of every woman. It might inspire each one to at least try to find and keep that almost impossible equilibrium, so beautifully demonstrated by the author. The book as it is can never be understood by the unprofessional. Indeed its one great fault as a scientific work is its cumbersome style.   S. H. S.

III.—THE WEST RIDING LUNATIC ASYLUM MEDICAL REPORTS.

THE WEST RIDING LUNATIC ASYLUM MEDICAL REPORTS. Edited by J. Crichton Browne, M. D., F. R. S. E., and Herbert C. Major, M. D. Volume VI. London: Smith, Elder & Co. 1876. 309 pages.

The sixth volume of this very valuable medical annual, contains thirteen papers, nearly all of decided value, and some of special interest. It would not be practicable to give here abstracts of them all, and we will only endeavor to notice the more important.

The first article in the volume is that by Dr. Herbert C. Major, on the Histology of the Island of Reil. The position, morphological history, and the pathological evidence of its possessing certain special functions, give to this portion of the cerebrum a peculiar interest and render the solution of the questions which the author sets himself to solve particularly desirable. Dr. Major sought to find whether (1) there was any difference between the arrangement of the layers of the cortex, the size of the cells, the vessels of the insula and those of other portions of the brain; (2) whether there was any difference in structure of the various gyri of the insula between themselves: (3) whether there was any difference between the corresponding parts in the right and left hemispheres, and (4) whether there was any difference in the union of the white and gray matter of this and other portions of the brain. His results were that there was generally none of these differences, only in the third layer of the insular cortex the cells were a little smaller than those of the vertex. These results negative as they are, are of value, the only peculiarity, that of the cells in the third layer, being suggestive, as it is in this layer that the degenerative changes described by the author and others are most frequently observed.

The article by Dr. Rabagliati, on the classification and nomenclature of nervous disorders, seems to contain some good ideas, but we can hardly agree with it as a whole. The classification he recommends is chiefly etiological and it has the disadvantages

of all our ignorance in regard to that particular aspect of disease.
It appears to us to be better to allow the nomenclature of disease
to work out its own evolution rather than with our present
knowledge, or rather ignorance, of pathology, to give ourselves
any great amount of trouble in trying on general taxonomic
systems. As Dr. Rabagliati says, names come in time to lose
their primary significations and to stand as mere symbols, and
thus do less harm than might be anticipated. Thus we still use
the term " hysteria " without regard to its original signification;
simply meaning by it a general complex of symptoms for which,
as a whole, we have not thought it worth while to choose an-
other designation. On the other hand, the phrase "softening of
the brain," which years ago was a sort of recognized medical
term, has almost dropped out of scientific language, though still
retaining its old vague and extensive meaning in the usage of
the laity. It seems to us better, therefore, to go the old way for
a while and let nomenclature regulate itself with the gradual
increase of our scientific knowledge of disease, rather than to
anticipate its development by elaborate general systems.

Dr. Robert Lawson continues in this volume the publication
of the observations upon the properties and action of hyoscya-
mine as a therapeutic agent, which was commenced in the
preceding volume, and continued in the Practitioner. In the
present article he gives special attention to its action in various
forms of insanity and recounts cases of its useful employment
in the epileptic status, in mania with incessant loquacity, and in
various destructive insane cases. In two forms only of mental
disease did the drug seem to be unavailing; these were acute
melancholia and mania with delusions of suspicion in the early
or acute form. In these, however, other means had also failed,
and the negative result in these cases is therefore not conclusive.
The drug used was the amorphous alkaloid, prepared by Merck,
which is comparatively cheap, but not altogether durable in
solution. Hence the author advises that only a small quantity
be prepared at a time for use. The doses varied from a sixteenth
of a grain to one grain, the former acting powerfully in various
conditions, characterized by frequent interrupted nervous dis-
charges and generally considered to be associated with sclerosis,
the latter often quieting and at once reducing to reason violent
and wilfully destructive cases. It produces a calm, according to
Dr. Lawson, such as follows the administration of no other drug
in common usage, and sometimes seems absolutely curative at
once. In cases of retention of urine, occurring in the progress
of nervous diseases, and due evidently to spasm of the sphincter
of the bladder, it produces free and voluntary diuresis. The
principal cautions indicated in its use are that great care be
taken in its administration in the case of the aged or persons
showing marked symptoms of arterial disease; that it be freely
diluted; and that its use be avoided in cases of furious mania,
where great excitement exists, and where artificial feeding will
probably have to be resorted to for some time.

The amount of the doses of hyoscyamine is noteworthy when we remember that it has been considered as identical with atropia by some authors, and when we remember that by some former authors the medicinal dose had been considered to range only between one-thirtieth and one-fifteenth of a grain. We have seen notices in journals recently of as much as a grain and a half having been administered at a dose, exceeding even the amounts here given. The comparative innocuity of this substance seems to be another distinction between it and the alkaloids of belladonna and stramonium, with which it possesses several qualities in common.

Dr. Henry Sutherland's paper, "Cases on the Borderlands of Insanity," is only noteworthy on account of the author's *ex cathedra* manner of disposing of the question of moral insanity. He says, using the editorial *we*, "we do not believe that such a condition ever existed except upon paper.   *   *   * Cases of moral insanity are either cases of moral depravity or of imbecility, or of the two conditions combined." And yet he narrates his cases of "moral depravity" as on the borderland of insanity. There seems to be a great deal more of this kind of positive opinion on this subject than is fairly justified by any facts.

The clinical notes on conditions incidental to insanity, by Drs. Lawson and Lewis, form a very interesting practical article. The first observation relates to the use of tannin suppositories in cases where palsy of the rectal sphincter exists, leading to the dirty habits so common among certain of the insane. The experience of the authors with this means of relief was extremely satisfactory; it seemed to increase the local tone of the sphincters of both rectum and bladder, and to continue in its good effects during the day as well as during the night. Among the other advantages that could be traced either wholly or in part to this treatment was the disappearance of bed-sores, which became almost unknown in the hospital. This method of meeting the difficulty of dirtiness in the insane may not be altogether new, but its success as here related makes it appear like a very important practical discovery. Among other matters of interest in this article, are the notes on the occurrence of voracious appetite as a symptom of brain tumors, two cases of recovery from mental disease accompanied by othæmatoma, and some rather extended remarks upon the state of the pupil as an indication of certain physical phenomena in the insane.

By far the longest paper in the volume is that by Dr. Crichton Browne, on the pathology of general paralysis of the insane. The fundamental lesion in this disease, according to Dr. Browne's opinion, is to be found in the adhesions, which are met with in at least eighty per cent. of the cases, between the pia mater and the surfaces of certain convolutions, and which are to be distinguished from those met with in ordinary chronic meningitis by the absence of the purulent products observed in the latter dis-

ease. There are other characteristic appearances such as thickening and softening of the skull, adhesions of the dura mater, staining of its surface, etc., but these are the most constant, though, as stated, even they are not present, or at least prominent, in a few instances. In these exceptional cases the appearances of the disease most resembled those of chronic atrophy of the brain; they were among the less typical and acute cases. Hence, from all these facts, Dr. Browne concludes that the pathological process in general paralysis is an insidious inflammatory one, arising from excessive functional irritation, leading to disintegration of the cerebral gray matter, and he makes some suggestive remarks in regard to these morbid changes observed in the brain representing the course and distribution of the morbid processes observed in the course of the disease. Thus we might expect to find the adhesions over the motor or sensory regions of the cortex controlling the functions, implications of which were marked symptoms during life. If this is the case, close clinical observations of cases of general paralysis, followed by the most careful and accurate autopsies after death, would, better than most other diseases, furnish valuable physiological facts in regard to the cerebral functions and supplement the results of physiological experimentation upon the lower animals. Some six cases are reported, and five colored plates of brains of paralytics, showing the distribution and extent of the adhesions are given. They appear to be most common over the anterior portion of the brain, the occipital lobes and the island of Reil being alone uninvolved.

The latter part of the paper is a general discussion of the symptoms of the disease, with reference to the theories of its pathology adopted in the preceding portion.

Another paper of considerable length, by Dr. Hughlings Jackson, is the concluding one of the volume on Epilepsies and on the after Effects of Epileptic Discharges. Dr. Jackson's views on epilepsy are too well known to require explanation here, and they have been previously noticed to some extent in this JOURNAL. We will only say that his ideas of epilepsy are much more comprehensive than those usually held in the profession. He uses the word to signify every excessive nervous discharge from the cortex, i. e., every super-normal excitation of functions from one or many of the cortical centres; and includes under this head, not only the convulsive attacks known as true epilepsy and epileptiform convulsions, and the *petit mal*, but also such disorders as migraine, and, as we understand him, even very limited convulsions of single members, without any other sensory or motor trouble. Following out his idea, even neuralgia, which we think must have always a central seat, must be classed among the epilepsies. All that is needful is that there be some super-excitation and excessive discharge of some cortical centre for the part involved in the pain or convulsion. It is only when the highest centres are implicated that

we have any trouble of consciousness. This is well enough, as expressing a pathological relationship of all these affections, but it extends the use of a name commonly applied only to a very formidable affection, to a very large number of minor disorders, a usage that can hardly become a general one, and which may possibly lead to misapprehensions. There may not be much in this point, but it has often seemed to us that if exactness of terms is desirable, it would be well to use different designations for the general pathological class and its individual species.

The after effects of epileptic discharges, here considered by Dr. Jackson, include such cases as those of transitory hemiplegia, aphasia, and other losses of function, following epileptic attacks. These are accounted for by the general principle that " parts of the central nervous system are temporarily exhausted by epileptic (that is, excessive) nervous discharges." We have in these cases only an exaggerated fatigue, reacting to a such degree as to produce actual temporary paralysis, sometimes closely simulating that due to organic gross disease.

It is not practicable to give a thorough abstract of this article ; it is a suggestive one, that will well repay reading. It is not, however, a finished paper, the remainder is promised to be published for private circulation.

There are several other articles in the volume that are worthy of notice, but which we cannot do justice to here. As a whole, it is worthy of its predecessors in the series, and a valuable addition to medical literature.

IV. FOERSTER: DISEASES OF THE EYE IN NERVOUS AFFECTIONS.

BEZIEHUNGEN DER ALLGEMEIN-LEIDEN UND ORGANERKRANKUNGEN ZU VERÆNDERUNGEN UND KRANKHEITEN, des Sehorgans. Von Prof. Foerster. Handbuch der gesammten Augenheilkunde, red. von A. Graefe und Th. Sæmisch. Bd. VII., 1ste Hælfte. (*The relations of constitutional diseases and local affections to changes and diseases of the eye, etc.*

The plan of this rather novel undertaking is to describe all symptoms occurring in the organ of vision from affections of other organs, and to study the etiology of diseases of the eye, as far as it is in relation to external disease. As the subject both of the greatest importance and of chief interest to the readers of the JOURNAL, we will limit the review to the article on the nervous system (pp. 104–145).

To no other organ is the eye as nearly related as to the brain, since it is not only in close proximity, but the retina and optic

nerve are to be considered as parts of the brain. Besides, the eye and its appendages are supplied by five pairs of nerves, the 3d, 4th, 5th, 6th, and 7th ; its vessels also communicate with those of the cranial cavity, as well as its lymph spaces connect with the spaces between the dura mater and the pia mater.

Nevertheless *cerebral hyperæmia* cannot be positively diagnosed by the vascular state of the conjunctiva or even retina. Apoplexy, as a rule, causes no alteration in the retina ; frequent ecchymoses, however, in the conjunctiva or retina are of a prognostic value in indicating impending apoplexy. *Miliary aneurisms* of the brain could probably be diagnosed by an ophthalmoscopic examination, since they have been found by Liouville in the dead retina.

Suppuration in the interior of the eye in *cerebro-spinal meningitis* is the result of the spreading of the process along the lymph-spaces of the optic nerve ; there may be also, however, a continued inflammation of the optic nerve and retina without implication of the choroid. Paralysis of the lids from meningitis may lead to exposure of the cornea and subsequent ulcerative keratitis. The implication of the motor nerves of the eye can produce irritative (such as pupillary contraction) or paralytic symptoms. Amaurosis occurring from meningitis is sometimes caused by the pressure of an exudation on the optic nerve, and hence the possibility of resolution. In some cases the inflammatory process extends from the meninges directly to the orbital cellular tissue without involving any integral part of the eye or injuring the vision. Leyden considers the chemosis of the conjunctiva thus produced as an aid in the diagnosis of acute purulent meningitis.

Increased intra-cranial pressure from whatever cause is often manifested by an optic neuritis, neuro-retinitis or choked disc. The occurrence of the latter is now correctly explained by Manz by a distension with fluid of the subvaginal lymph-space of the optic nerve—*hydrops vaginæ optici*—compressing the vessels of the nerve. Injection of fluid into the subdural space of rabbits produces the phenomena of choked disc. Vision, however, may not suffer in this state. The most frequent causes of choked disc are tumors of the brain.

The seat of an intra-cranial morbid process is often revealed by a study of the field of vision. In conformity with the theory of semi-decussation of the fibres in the optic chiasm the following two varieties—and only these—of hemiopia are observed:

1. In the visual field of both eyes, the *external* half is defective; only objects situated on the internal side of the point of fixation are seen, (corresponding to loss of function of the *internal* half of the retina)—*nasal hemiopia.*

2. In the visual field of one eye the *external* half, of the other eye, the *internal* half is defective—*homonymous hemiopia.*

Nasal hemiopia is a slow, progressive affection, caused by

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some morbid process invading the centre of the chiasm. Homonymous hemiopia, on the other hand, a more frequent symptom, is mostly of sudden origin, frequently occurring with apoplexy and remaining stationary. It points therefore to a localized process involving the tractus opticus somewhere between the central origin and the chiasm. Foerster has also seen some cases in which there is no complete hemiopia, but only a small defect in identical spots of the visual field of both eyes.

An allied condition seems to be the so-called *Flimmerskotom.* (Amaurosis partialis fugax. Foerster.) This trouble, evidently of central origin, consists of a suddenly occurring defect in identical spots of the visual field of both eyes, gradually increasing in extent and surrounded by a zone of flickering. This condition lasts usually but 15 to 25 minutes.

Many brain diseases give rise to symptoms on the part of the ocular muscles, paralysis or more rarely to spasm and nystagmus. An involvement of several of the motor nerves of the eye points to some process localized in their nuclei in the floor of the fourth ventricle.

As regards the co-ordination of ocular movements, the anterior tubercula quadrigemina seem of importance In Adamiuk's experiments, irritation of one of the anterior tubercula caused the eyes to be directed towards the opposite side, and if strong enough, the head was turned in the same direction. A deep incision separating the tubercula in the median line, limeited the effects to the eye of the same side. Irritation of the median line produced an upward movement of the eyes with dilatation of the pupils, while a downward and inward movement and pupillary contraction is the result of irritation on the posterior part of the tubercula (similar movements have also been obtained by Ferrier, from irritation of the cerebellum). In confirmation of these experiments are the statements of Prevost, that:

"In apoplexy, turning of the eyes and head towards one side is observed.

1. If the process is located in the hemispheres the eyes and head are turned towards the side affected, though sometimes only for a few days.

2. If the process occurs in the isthmus encephali, the eyes may also be directed towards the opposite side, and remain thus for some time."

Partial cerebral sclerosis gives rise occasionally to atrophy of the optic nerve. However, of more importance in the diagnosis is the occurrence of paresis of the ocular muscles (and diplopia) and especially a state resembling nystagmus, a tremulousness in the movements, not existing during rest. While multiple incomplete paralyses of ocular muscles point to some diffuse process in the brain, the successive complete destruction of function of ocular motor nerve is a rather positive evidence of a localized process such as a tumor, etc., at the base of the brain.

Mental diseases give rise frequently to differences in the width of the two pupils; in fact, this symptom occurring in apparent health has some prognostic importance. Inflammatory or atrophic changes of the papilla are also no rare symptom in insanity; especially in *general paresis* is atrophy a frequent occurrence.

Tabes dorsalis is often accompanied by atrophy of the optic nerves. There is even evidence that this symptom may precede all other manifestations of the disease by several years. This atrophy is characterized by a gradual limitation of the visual field commencing at the periphery, while sight may remain tolerably fair in the vicinity of the macula lutea. A subjective symptom of the ataxic atrophy is a want of sensibility for red and green rays. There is an aversion to too bright a light; the patients claim to see better at dusk than with sunlight. The trouble is always progressive, although the patients are often remarkably hopeful.

Other symptoms of *tabes dorsalis* are slight, transient and recurring pareses of ocular muscles. Pupillary changes are also frequent, especially contraction with immobility to light, while efforts at accommodation render the pupil still narrower. The same occurs also in injury of the spinal cord.

Paralysis of the cervical sympathetic nerve, by pressure of tumors, etc., causes pupillary contraction; the slight *ptosis* mostly coincident is evidently due to the inactivity of the unstriated muscular fibres discovered by H. Mueller in the lids. A diminished tension of the eye and retraction into the orbit are also observed in long continued cases. The pupillary changes in hemicrania have been repeatedly discussed in this JOURNAL.

As regards the so-called neuro-paralytic keratitis, consequent to section of the trigeminus, Foerster maintains its traumatic origin, since the eye not feeling any injuries does not seek to protect itself against them. (Recent experiments, however, by Ferrer, *Wiener Sitzungsberichte*, have shown conclusively that the process is caused merely by the drying of the cornea, in consequence of the maintained immobility of the lids, following paralysis of the trigeminus.)

Epileptic seizures produce occasionally ecchymosis of the conjunctiva, referable to venous stasis. In the beginning of the attack, pupillary dilatation is often noticed. Anæmia of the retina has been seen by Hughlings Jackson during the attack : in the interval venous congestion, and sometimes choked disc has been found.

In *hysteria* a few cases of more or less transient amaurosis have been described. A different affection, however, is the so-called *anæsthesia retinæ*, with concentric limitation of the visual field. This disease, as described by v. Graefe, does not seem to depend on hysteria.

In the chapter on relations of diseases of the sexual organs to the eye (p. 88), Foerster describes a peculiar form of asthenopia,

which he calls *kopiopia hysterica.* The patients, almost exclu-
sively females, complain of irregular attacks of pain, and un-
pleasant sensations in or around both eyes, which are aggravated
by reading, mental or physical exertion. There is likewise pho-
tophobia, especially to artificial light. Local treatment, by
means of spectacles, etc., produces no improvement. According
to Freund (of Breslau), the affection is always coincident with a
peculiar form of chronic parametritis, leading to atrophy of the
pelvic cellular tissue. The above abstract suffices to show that
the subject is essentially a new one, and how it has been
treated.

The happy blending of personal observations of the author
with the previously-known scattered facts, the precise and
critical style, and the absence of diluting verbosity, make the
entire work profitable and readable.   H. G.

V.—BUCKNILL : AMERICAN INSANE ASYLUMS.

NOTES ON ASYLUMS FOR THE INSANE IN AMERICA.   By John
    Charles Bucknill, M. D., etc.   London: J. & A. Churchill,
    New Burlington Street, 1876.

There is perhaps no higher English authority on insanity, in
both its medical and legal relations, than the author of these
notes of a visit, in the summer of 1875, to some of the insane
asylums of the United States and Canada.   Nor is it alone as a
writer that Dr. Bucknill is distinguished in his own country.
He has served for many years with success as the medical super-
intendent of a public asylum, and more recently in the import-
ant office of Lord Chancellor's visitor in lunacy.   The "Notes"
appeared first in the *London Lancet,* where they excited con-
siderable interest and no small controversy.   As we should
expect, at least an equal interest has been felt by those in our
own country who are the subjects of Dr. Bucknill's free and
confident criticism.   This did not fail to appear at the last meet-
ing of Superintendents of American Insane Asylums, in St.
Louis, although from motives of policy a full discussion of the
vexed questions raised by the book was not encouraged.   We
shall endeavor to write of Dr. Bucknill's facts and comments
with that freedom and plainness of speech of which he has set
the example.

In his preface, and throughout many subsequent pages, the
writer strongly condemns the free use of mechanical restraints
in American asylums, and urges their superintendents to adopt
the so-called "system" of non-restraint.   We are compelled to

say, that his language upon this subject is in the style too generally adopted by British writers when addressing their brethren of foreign and colonial parts. It has the same tone of impatient and rather contemptuous criticism which appears in the comments of the *Times* and the *Saturday Review* upon American politics and society. Dr. Bucknill has before him the report of a discussion of the non-restraint system by the Association of Superintendents of American Asylums, at its meeting in 1874, which he is tempted to discuss. "But," he says, "the subject is so threadbare in the pages of the *Lancet*, wherein the great battle was fought in ancient times, that I shall only venture upon a few brief extracts and remarks. The termination of the discussion is so astounding and instructive that I must really entreat the *Lancet* to find space for it."

In the extracts quoted, Dr. Walker, of Massachusetts, who had lately visited several English asylums, states that their medical officers, "almost without exception, expressed a preference for mechanical restraints, as against the dogma of their total abolition." Upon this and other equally inoffensive statements, Dr. Bucknill comments as follows:

"I must resist the strong temptation to treat the above in the manner it invites, but how to treat it seriously I scarcely know. Yet it is a most serious matter, and reveals the true foundation of the American prejudice; namely, profound ignorance of what has really been done, and is yet doing, in this country."

Now whatever may be the merits of the non-restraint system, Dr. Bucknill has no warrant whatever for charging American superintendents with "profound ignorance" of what has been done and is doing in British asylums. At least a dozen of these gentlemen visit the principal asylums of England and Scotland, for every British alienist who visits our asylums. This is as well known to the medical profession as the general fact is known to every one, that the number of American travellers in Great Britain is more than ten times that of British travellers in America. Of this general fact, indeed, Dr. Bucknill does not seem to be ignorant. In another chapter, predicting a popular revolt in this country against the use of restraints for the insane, he says:

"The Americans, who are about the best informed, most inquisitive, and widely-travelled people in the world, are not likely to be ignorant of the treatment of the insane in other countries."

We have never heard that American superintendents are deprived of the privileges enjoyed by the mass of their countrymen, and confined to their own asylum grounds. Dr. Bucknill is not less than impertinent in asking "why the leaders of opinion in America do not come to England and really study this most important question fully and conscientiously." For ourselves, we have no doubt of the truth of the facts stated by Dr. Walker and others. In visiting some of the best English asylums, it has been frankly conceded to us by the medical officers that the

dogma of non-restraint is as practically contrary to the best interests of the insane as it is theoretically false and even absurd. They were willing to laugh at the claim that everything done in the present century to improve the condition of the insane is due to the non-restraint system, nor did they deny that in certain cases of insanity mechanical restraint is absolutely necessary to proper treatment. But it must be said that the abolition of restraints, forced upon the English asylums by popular distrust and prejudice nearly half a century ago, has become almost a part of the British Constitution. Of course, then, Dr. Bucknill thinks "the claim of Dr. Ray that the question of non-restraint should be argued upon scientific grounds alone, not altogether admissible." The latter believes that locks and window guards are as much mechanical restraints as the muff and belt, and that they are far less objectionable than drugs, the shower bath, and the hands of attendants, which have been substituted for them. He has also fairly urged that a dogma which forbids the use of any agent in medical treatment is clearly unscientific. We confess ourselves unable to understand how Dr. Bucknill can pronounce "the unqualified opinion that mechanical restraint is an evil and abuse," while he would set down as a quack any one who should say the same of opium or alcohol. That its excessive use in the asylums of this country is really an abuse which calls loudly for reform, our own knowledge and information do not permit us to doubt. But that this abuse will ever be removed through a crusade for the total abolition of restraints from asylums, we no mere believe than that dram-drinking will be done away with by preaching the doctrines of total abstinence. We believe, also, that the liking for rational theory, as the basis of practical reforms, is as strong with the American public as the contempt for it is with that of England; and the theory of non-restraint in the care of lunatics confined in asylums, is clearly a contradiction in terms.

It is certain, however, that the refusal of our asylum superintendents to discuss the subject of restraints cannot long stifle what Dr. Bucknill terms "a blazing question," and hide the necessity of reform. Such facts as are stated in a late essay on "The Management of the Insane," by Dr. Wilbur, a distinguished physician and philanthropist of New York, describes an untenable and even dangerous position. In his visit to the State Asylum at Utica. Dr. Bucknill found "not a single patient in restraint, and was told by the superintendent, Dr. Gray, that he did not use restraints." Dr. Wilbur refers to Dr. Gray's own reports to prove that he favors this use, and that even the most objectionable kinds of restraint are freely used in his asylum. In short, it is substantially charged by Dr. Wilbur that before conducting his distinguished visitor through his wards, Dr. Gray had ordered the doors of more than thirty rooms containing crib-beds to be locked, and all other restraining apparatuses temporarily removed from his patients. Whatever may be the

facts in the case, there is no doubt that the policy of reserve and, we were about to say, concealment in the case of the insane has too many advocates among American superintendents. This policy should be distinctly denounced by every friend of asylums as dangerous to them as well as to the insane and the public. Our own opinion, based upon a careful study of English asylums, is, that their comparative freedom from serious abuses is due far less to the system of non-restraint than to their thorough supervision by public officials, and the absence of all secrecy from their administration. Except, perhaps, in one or two of the New England states, the management and supervision of American asylums exists only in name. In most of the States the managers are merely local politicians, whose office it is to secure the largest possible appropriations from the State treasury. For this purpose they combine readily with canal, prison, and other officials to form a "ring," against which State boards of charity and other supervising agencies are powerless. In England asylum-managers are chosen from the principal tax-payers in the district charged with the support of the asylum. They are usually gentlemen of wealth and comparative leisure, and capable of appreciating the importance of the duties entrusted to them. Moreover, the government of the asylums is such as to devolve real duties upon them, which they cannot escape even if they would. No one of the officers of the asylum has supreme authority, but the rule of each is confined to his own department, leaving the board of managers as the true administrative head. It is plain that the absolute authority given to the asylum superintendent in this country leaves no room for any but a formal management. In fact, there is in our system no check whatever upon these officers. Dr. Bucknill points out the dangers of such a system, and expresses his astonishment at the boldness of the Association of asylum superintendents in formally resolving that no further supervision of asylums is necessary. The argument of their resolution runs, that "when the legislature shall establish a board of officers to supervise the medical practice of the State, with power to enter every sick man's chamber, to inquire respecting the medicines and diet prescribed, and any other matter connected with his welfare, then it may be proper to consider the propriety of extending the same kind of paternal visitation to the hospitals for the insane." To this reasoning Dr. Bucknill is content to reply that its author, Dr. Ray, " forgets the difference between a sane and free man suffering from sickness in his own household, and a lunatic incarcerated in an asylum."

On the subject of the building and maintenance accounts of asylums, Dr. Bucknill is less full and satisfactory in his comments than might have been expected. He has been a firm opponent of the policy of building palaces for the insane poor, and a few words from him showing its deplorable effects upon the lunacy system of Great Britain, would have been of great value.

The States of Massachusetts and New York, to whose asylums
he gave especial attention, are both engaged in building new
asylums after the most extensive and elaborate designs, and at
an immense cost.   At Danvers, Mass., a million and a half
dollars have been expended upon buildings for the accommoda-
tion of 450 patients.   This is at the rate of more than $3,000
per patient.   The asylums at Buffalo and Poughkeepsie, in New
York, upon which nearly three millions have already been ex-
pended, it is thought possible to complete at a cost of $5,000
per patient, by making considerable changes in their plans.
When we consider that the best modern asylums in England
cost less than $1,000 per patient, it is strange indeed that so
acute and outspoken a critic as Dr. Bucknill should have failed
to condemn our enormous extravagance.   The most of this ex-
cessive costliness of design is, that, after ten years in building,
these asylums are not yet more than one-half completed, while
barely one-fourth of the insane of New York are provided for
by the State, leaving the other three-fourths crowded together
in county asylums and poor-houses.   Moreover, the State in-
stitutions when completed will provide for less than half the
public insane.   Most of those now in the county asylums must
remain in them, and they will, doubtless, continue to suffer
from the diversion of a large share of the public bounty to the
maintenance account of the palatial asylums, as they have
so long suffered from their slow and costly construction.

For nothing is more certain than the fact which we remem-
ber to have seen tersely expressed by Governor Tilden, in one
of his reform messages, that " these magnificent homes lead to
magnificent current expenditures for their support."   The rela-
tive cost of the Buffalo and Poughkeepsie asylums will amount
to five times that of the asylum at Utica ; and the cost of sup-
port in them must necessarily be much greater, on account of
their greater size, and more complex system of internal arrange-
ment.   But it chances that Dr. Bucknill's attention was called
to the cost of support at the Utica institution, and it is inter-
esting to follow him in his search for an explanation of it.   " I
found," he says, " that the average weekly cost in the State
asylums was not under four dollars per patient."   In fact, the
average cost in fifty-three of these institutions, for the years
1875 and 1876, was a small fraction less than four dollars.   This
he compares with the average cost in British asylums, which is
not quite $2.50 per week ; and suggests as a reason for the dif-
ference, that " the high price of clothing and comforts, and the
high rate of wages, counterbalances the low price of food in
the United States."   But only a small part of the difference can
really be accounted for in this way.   While Dr. Bucknill prop-
erly includes the cost of clothing under the head of mainte-
nance, several of our asylums, in order to make their returns
more favorable, have omitted this item from the account.   This
is the custom of the Utica institution, of which Dr. Bucknill

states the weekly cost at $4.50. It is much more than this, as will appear further on. But what most astonishes him, is the difference between this cost and that of the New York City asylum. " It may well be," he says, " that some explanation can be given of the difference in the weekly cost of maintenance between four dollars and fifty cents at the asylum for the State of New York, and of one dollar and thirty cents at the asylum for the city of New York ; for I cannot believe that, however accurate my general impression as to the condition of the latter institution might have been, the whole of this difference could be so accounted for."

His general impression received from a visit to the New York City asylum, was, "the patients were badly clad and insufficiently fed." But he was right in supposing that so great a difference in the cost could not be wholly due to this fact. How it may be accounted for, is what we have been at some pains to learn, and an accurate statement of the excess in the maintenance account of the Utica asylum may not perhaps be without its importance. The facts and figures are taken from Dr. Gray's reports to the Managers and to the New York State Board of Charities.

As no fair estimate of expenditures can be made from the accounts of a single year, we have taken those of the Utica asylum for the last eight years—1869–76. During this period, Dr. Gray has received for the support of a daily average of 600 patients, an average sum of $173,260 each year ; or at the rate of $5.55 per week per patient. As the average weekly cost in American asylums of the highest class is $4.00 per week, we find an excess of $48,360 per year, for eight years, in Dr. Gray's maintenance account. In addition to the above, he has received, during the same period, an average sum of $37,850 each year in appropriations from the State treasury, for repairs and improvements. Now the capacity of the asylum has not been increased since 1869; and it appears that this yearly sum is at least ten times larger than other asylums of the same rank and size have expended for similar purposes. Dr. Gray's total excess, then, has been more than $80,000 yearly for eight years past, amounting to a round sum of $650,000.

The great importance which the cost and maintenance of asylums has lately assumed in this country, will justify us in giving a brief space to this part of the subject under review, taking the Utica asylum as a text.

As year follows year, Dr. Gray's extraordinary expenditures become so ordinary in occurrence, and his ordinary expenditures continue so extraordinary in amount, that it would seem difficult for him to decide under which of these two heads to place his large yearly excess. In his report for 1871, the items for ordinary repairs ($19,705.74), and for farm expenses ($11,380.47), are classed as extraordinary expenditures. This is, of course, contrary to universal rule ; but with the omission also of the item for clothing

($14,143.42) from ordinary expenditures, the latter are so reduced in amount, as to give a weekly cost per patient of $4 96, which is only twenty-five per cent. greater than the average of State asylums. But in 1874 he properly reports the sums for ordinary repairs ($30,420.87) and for farm expenses ($10.250 68) as ordinary expenditures. In this way, the sum of his extraordinary expenditures is reduced to a less immoderate amount ($16,743.13), and making a few thousands more or less in the gross sum of his ordinary expenditures ($212,790.08). But when this sum comes to be used as a basis for calculating the weekly cost per patient, it is found to yield the extravagant rate of $7.18. Here Dr. Gray proceeds to add a third division, which he calls "current expenses." This term has hitherto been used to denote the monthly or quarterly sums which, added together at the end of the year, form the ordinary expenditures. In the accounts of all public and private institutions these terms cover exactly the same items, and are in fact the same. What then does Dr. Gray include in current expenses? Ostensibly, it is the ordinary expenditures of the year, less the amount paid for clothing. But instead of deducting the item for clothing alone ($9,059.87), Dr. Gray also deducts those for officers' salaries ($14,610.58), for ordinary repairs ($30,429.81), and two other items ($1,501.69), making a total of $55,602.01. By rejecting from extraordinary expenditures on the one hand, and dropping out from ordinary expenditures on the other this large sum, the weekly rate per patient is reduced to $5.42. This cost is not much larger than that which Dr. Gray obtains in 1871 by omitting from the account a different set of items, with a total of $49,042.69. But it is still more than one-third larger than the average of American asylums ; and the true cost ($7.18) calculated upon the ordinary expenditures, is eighty per cent. larger than this average, and only a fraction less than three times the average cost in English asylums, as given by Dr. Bucknill.

These figures we give partly to illustrate the real cost of maintenance of one of our best asylums, and to show how far from being satisfactory and exhaustive the examination of Dr. Bucknill's was, at least in regard to one of the American asylums to which special reference is made in his memoir. But to proceed.

We have now to state that for the extreme theories of insanity as disease, upon which all this extravagance of asylum construction and maintenance has been based, Dr. Bucknill is in no small degree responsible. In his writings upon the pathology of insanity, which have been a standard authority in Great Britain and this country for many years, he insists, again and again, upon the principle that the only true and scientific conception of insanity is that of disease. Nor is he satisfied to conceive it as functional disorder only, but declares, positively, its dependence upon morbid nutrition-changes in the brain. It

is true that while repeating and enforcing this theory through many pages, he admits the impossibility of proving it. The fact that we really know nothing of the connection between mind and brain in health or disease, and that such knowledge is impossible to us in the nature of things, is fully admitted by him. And yet he does not hesitate to deduce from his fundamental principles the positive doctrine that " any pathological state of the organ of mind will produce a greater or less amount of disease of the mind, that is, of insanity." Even when first written, nearly a quarter of a century ago, this doctrine was contradicted by well-known facts, and it has since been abundantly disproved. Instead of being dropped, however, by his disciples in this country, it has been developed into numerous logical absurdities, and made responsible for the most serious practical errors. Insanity is a disease, they say, whence it follows that all lunatics are sick persons demanding medical treatment. Again, insanity being one disease, and it being undisputed that for some of the insane the special care and treatment of hospital-asylums is required, therefore all chronic insane and demented persons require such care. And still further, since of a certain small class of the insane a large proportion will recover, equally with or without treatment, therefore the same proportion of all the insane will recover if treated in hospital asylums. The practical conclusion to which the public of New York and many other States have been led by such false reasoning from an hypothesis confessedly impossible of proof, is seen in the enormous and costly asylums which we have already described.

In regard to the curability of insanity, however, it is only fair to Dr. Bucknill to state that nowhere in his treatise does he give any support to the absurd claims of our American specialists. He does, indeed, quote upon this point from Dr. Thurnam, a well known English writer, but the conclusions of the latter have been greatly misrepresented. This has been done not by one alone but by many writers in this country, and it is only as being the most convenient that we quote the following from the report of the Utica asylum for 1869:

" Dr. Thurnam, a distinguished writer on the subject [insanity] states that if cases were treated within three months of the first attack, four-fifths would recover; but if twelve months elapsed, four-fifths were incurable; and so in proportion as the term was longer or shorter."

Now, it appears on examination of the book referred to, that every fact stated, and every inference suggested in this paragraph is incorrect. Dr. Thurnam does not say what is attributed to him, nor anything that is equivalent to it. All cases of insanity can no more be placed in an asylum within three months of the first symptoms, than all cases of crime can be committed to prison within three months of the first appearance of vicious conduct. Four-fifths of all the insane will not recover under

any circumstances, and it has been stated without denial before the British Medico-Psychological Association, that an appreciably larger number do not recover under asylum treatment than without it.

We may add, in further illustration of these facts, that lunatics committed to asylums fall naturally into three classes:

1. Cases of simple acute insanity. These are mainly persons of an insane temperament which has been inherited or is congenital. Paroxysms of mental disorder are excited in them by some trifling accident, mental or physical, and continue through a period of weeks or months, when the patient generally recovers nearly his former mental condition. There is nothing in science on which to base a theory of medical treatment in these cases, and experiments upon them with medicines are so irrational as justly to be considered abusive. About one-fourth the yearly admissions to asylums are of this class, and according to Dr. Thurnam, four-fifths of them—not four-fifths of the whole—may be expected to recover.

2. Cases of chronic insanity, forming more than one-half the yearly admissions to asylums. In these the mental disorder has been slowly developed through many months or even years before they could be pronounced insane. For this reason their early treatment in asylums is impossible, and only a very small proportion of this class recover, whether with treatment or without it. They are simply cases in which mental viability reaches its term in advance of bodily decay and death.

3. Cases of bodily disease, usually of the brain, in which mental disorder has supervened. These form less than one-fourth of the admissions to asylums, and are proper subjects of medical treatment. It is well known, however, that the insanity of brain disease almost invariably belongs to its last and incurable stages. Cases of this kind have usually been given up as hopeless before they are sent to an asylum.

Now, to provide proper asylums for all these classes of the insane poor is certainly one of the most imperative duties of government; but the scheme of stamping out insanity by curing four-fifths of the whole number of those who become insane each year, can only have been born of ignorance or of imposture. As the authority of Dr. Bucknill has been quoted in behalf of the doctrines upon which this scheme is founded, we must again regret that he has lost a fit opportunity of disclaiming all responsibility for them.

We had marked several other points of interest in medical psychology, touched upon in these "notes;" but the space already filled forbids further comment, at present. If we mistake not, the time is favorable for a free and thorough discussion, in this country, of many questions relating to insanity and the insane, and it seems not unlikely that Dr. Bucknill's little book may be the means of opening it in earnest. With this hope we are the less unwilling to close our imperfect review.

VI.—THE PHYSIOLOGY OF THE RETINA.

F. Boll. Zur Anatomie und Phys. der Retina. *Berliner acad. Monatsber.* 12th Nov., 1876, (abstr. in *Centralblatt f. d. med Wiss.* 31st March, 1877).—1. Zur Phys. des Sehens, etc. *Berliner acad. Monatsberichte*, 11th Jan. 1877 ; and (2) Nachträgliche Zusätze zu d. Mittheil. 15th Feb. 1877 (both in *Centralblatt.* 9th June, 1877).—Zur Anat. und Phys. der Retina. *Arch. f. Anat. und Phys. von Du Bois Reymond.* 1877, I. Heft. p. 4.

W. Kühne. Zur Photochemie d. Netzhaut. *Verhandl. d. naturhist. Vereins zu Heidelberg*, Jan. 1877 ; and *Revue Scientifique.* March 3d, 1877.—Vorl. Mittheilung über optographische Versuche. *Centralblatt.* 20th Jan., 1877.— Zweite Mitth. über Optogramme. *Centralblatt.* 27th Jan., 1877.—Ueber der Sehpurpur. *Centralblatt.* 17th May, 1877.—Ueber das Vorkommen von Sehpurpur. *Centralblatt.* 4th April, 1877.

Schenk und Zuckerkandl. Sehpurpur in Auge eines gehenkten Menschen. *Allgem. Wiener med. Zeitung.* 13th March, 1877.

Fuchs. Zur Farbe der Netzhaut. *Wiener med. Wochenschrift.* 10th March, 1877.

Helfreich. Purpur der Retina. *Centralblatt.* 17th Feb., 1877.

Adler. *Centralblatt.* 7th April, 1877.

Dietl und Plenk. *Centralblatt.* 21st April, 1877.

Schmidt-Rimpler. *Centralblatt.* 9th June, 1877.

Michel. *Centralblatt.* 16th June, 1877.

Stef. Capranica. Phys-chem. Unters. über die farbigen Subst. der Retina. *Arch. f. Anat. und Phys.* 1877 : II. and III. Heft. p. 282. (To be continued.)

Repeatedly statements have appeared by various authors— Leydig and others,—that the fresh retina of the frog and some other animals possesses a reddish tinge ; but no further attention had ever been paid to this. It caused therefore great sur-

prise when Boll announced that the living retina is always *red*, and that this color is at once destroyed by exposure to light. This coloration, as Boll found, is limited to the external part of the retinal rods. A small number of rods, at least in the frog, are also of a bluish-green tint. This coloration of retinal rods seems to exist in all animals. Of invertebrates, Boll found it in the cephalopoda and anthropoda. Amongst vertebrates, he found it in all fishes and amphibia examined. In reptiles and birds, the colored oil globules of the retina interfere with its detection. In the retina of the lizard and snake (Coluber natrix), there are no rods, only cones ; and, hence, the red color is not found (Kühne.) In the pigeon, Boll noticed a slight fading of the retina, when removed from the eye. In the falcon, Kühne found the red color absent behind the spots containing oil globules. The long rods of the owl (Athene noctua), however, show the greatest intensity of color, it being almost bluish or violet, and fading but very slowly on exposure to light. Of all mammals examined, Kühne failed in finding the red color only in the bat. The embryo of the calf has the external part of the rods colored as early as they exist. In man the *retinal red* was first seen by Schenk and Zuckerkandl in an executed criminal ; soon afterward by Fuchs in the human fœtus. It has since been confirmed by Adler, Schmidt-Rimpler, and Kühne. Only Michel failed to find it in a normal eye, extirpated for orbital cancer, although he mentions all precautions as to the exclusion of light. The macula lutea was found by Schmidt-Rimpler to differ from the rest of the retina, it being of an orange tinge, in an extirpated eye, and dark brownish-red in fresh cadaver eyes. Kühne found the cones of the macula colorless in man and monkey, and the zone around it feebly colored, on account of the scarcity of rods. The zone bordering on the ora serrata was also but feebly colored ; the ora itself colorless. Some queer statements have been made by Adler. In a blind eye he failed to detect the *retinal red*, though he used the same precautions, which enabled him to find it in other cases. In a case of retinal tumor, the red color was found in that part of the retina which had still perception of light, while the blind part was colorless. A retina, partially detached and prolapsed by an injury, was found uniformly red on examination with the light of a lamp ; exposed to sunlight, the detached part lost its red color.

The color of the retina persists after death, if the eye is kept in darkness; it disappears after twelve hours in the rabbit, after about twenty-four hours in the frog. Its persistence in the extirpated eye on exposure to light is an index of vitality. In warm blooded animals light bleaches the retina left in situ in a few minutes; in extirpated eyes of the frog on the other hand the persistence of vitality is indicated by the persistence of the retinal red for many hours, although exposed to light. On removing the frog's retina it is at once bleached, hence the color-

ation of the retina *in situ* must be maintained by a *regeneration* of the color.

Boll describes four stages of the frog's retina after removal from the eye in bright daylight. In the first ten to twenty seconds it is of an intense red (not *purple* as was originally stated), but fading rapidly while assuming a yellow tinge. In the second stage it is almost colorless, while the rods show a strong satin-lustre during thirty to sixty seconds. This lustre he found also in the retina of the pigeon where the red color was masked by the color of the oil globules. Hereupon the rods lose their lustre, swell and acquire the same index of refraction as the other layers, so that the retina becomes perfectly transparent. At the end of fifteen minutes the beginning post-mortem changes destroy this transparency.

The rapidity with which the retina is bleached depends on the intensity and color of the light. According to Kühne the rays of the spectrum between yellow and green possess the greatest influence, while the bleaching becomes slower as we proceed towards the violet end of the spectrum. Yellow rays have scarcely any action, red and ultra-violet (chemical) ones none at all. Hence experiments on the retinal red can be made most readily in the light of a monochromatic sodium flame, which destroys the color only in about two hours.

Exposure of the *living* frog to *sunlight* also bleaches the retina. When removed, at the end of five minutes, exposure in the living animal, it has become quite pale; in fifteen minutes the red color is destroyed, the lustre alone remaining (Boll). Mere daylight will bleach the living retina, only after a much longer time. On returning the animal into a dark box, the retinal red reappears in about one hour, and gains its greatest intensity in about two hours. Kühne found the color restored by darkness even in the exstirpated eye.

Of much interest is the action of the separate rays of the solar spectrum on the *living* retina. *Red* light renders the retinal tint darker, almost brown. In fading, when exposed again to ordinary light, it passes through a brownish-yellow stage. The color of the bluish-green rods is also intensified by red light. *Yellow* rays have no further action than rendering the color somewhat clearer. *Green* light, when of moderate intensity, or if intense—of short duration, changes the color to a purple-red, which when bleached by ordinary light, passes through a rosy tinge. A longer exposure to intense green light gives the retina a cloudy appearance, and if prolonged it finally bleaches it. The color of the bluish-green rods is, however, intensified and not as rapidly destroyed. The number of the bluish-green rods seems to be considerably *increased* by the action of green light, although Boll admits the uncertainty of such an observation. *Blue and violet* rays have about the same effect as green light. *Ultra-violet* rays are of no effect at all.

The anatomical separation of the retina from the choroid coat

succeeds most readily when the color has its maximum of intensity. Decolorized retinæ usually tear during this manipulation, and patches of pigment cells remain attached to them in the latter case. The following cause of this was discovered by Boll in eyes hardened with alcohol:

In the decolorized retina fine processes of the pigmented epithelium extend between the rods and cones up to the membrana limitans externa; in the colored retina no such processes are found.

Is the retinal red due to an optical arrangement, or to a chemical pigment?

On compressing the retina, there appears momentarily an intense green lustre, followed at once by decolorization. The retinal red is not affected by freezing, nor by the action of distilled water, glycerine, solutions of chloride of sodium, carbonate of sodium, acetate of lead or alum. All these experiments were of course made with sodium light. It is rapidly destroyed by boiling, by the action of alcohol, and caustic alkalies; less rapidly by ether and chloroform. Ammonia, by rendering the retina transparent, increases the brightness (and persistence) of the color. Alum (five per cent. solution), on the other hand, renders the retina hard and white; the posterior surface alone is found red. Acetic acid changes the retinal red to an intense golden-yellow, which fades but slowly. The dried retina also fades very slowly, until it has assumed an orange tint, when it is no longer sensitive to light.

The only *solvent* of the retinal red was found by Kühne to be bile, or a purified cholate. The filtered solution appears carmine, changing to buff on exposure to light. The red solution absorbs all rays of the spectrum between yellowish-green and violet; hence, the fresh retina appears gray or black, when illuminated by light of this color.

What is the source of the retinal red? The retina removed from an extirpated eye of a frog, is bleached at once; left *in situ*, it retains its color for hours. If partially detached from the choroid, Kühne found the detached portion of the retina soon bleached, while the other part remained red. On reapplying the detached retina to the choroid, its color was restored. The choroid is, hence, the source of the retinal red. If, on removing the retina, patches of the pigment-epithelium adhere to it, the corresponding spots are not bleached as rapidly. This is not due to the black color, since any other black background does not retard the bleaching. No red pigment, however, can be obtained by rubbing over the choroid with a piece of paper. The vital nature of the process may be shown besides by the action of heat. A temperature of 43° C. does not destroy the retinal red; if, however, the red has been previously bleached by light, a bath of that temperature prevents its regeneration, though the retina be left *in situ*. More light is shed on this obscure question by the observation of Boll, that the retinal

red, when rendered golden-yellow by the action of acetic acid, is identical in color with the oil-globules found in the retinal pigment-epithelium of the frog. In frogs kept in darkness, the epithelium contains only the ordinary golden-yellow oil-globules and a few smaller colorless oil-drops. If, however, the animal has previously been exposed to sunlight, and is examined during the period of regeneration of the retinal red, there is found constantly a large number of pale, yellowish oil-globules, the pigment of which has evidently been extracted. The—as yet unfinished—investigation of this pigment, made by Capranica, in Boll's laboratory, promises further interesting results.

The old question as to the possibility of obtaining persistent images on the dead retina, has been settled by these investigations. The readiest method, Kühne found, was to expose the freshly decapitated head of a rabbit to the light of one window (or to the light issuing from a window of ground-glass, in a dark box). On hardening the retina for twenty-four hours in a five per cent. solution of alum in the dark, a distinct white image of the window was found on the otherwise red posterior surface of the retina. By drying the retina, the specimen could be preserved. In bullock's eyes, Kühne could succeed in a similar manner, until one hour after death. In the latter case the image can be seen at once on spreading out the retina in a solution of salt. Optograms can also be thus obtained in curarized frogs.

Finally, the question arises, is the retinal red seen with the opthalmoscope? The color of the fundus of the eye is, no doubt, due to several factors: the color of the retina itself, and the vascularity of the choroid, covered, as it is, by the pigment-epithelium. At the moment of the death of a mammal, especially when from hemorrhage the fundus becomes paler. If now one eye is exposed to light, while the other is protected, the latter appears more of a buff, and quite different from the former, if examined with the opthalmoscope after the lapse of some minutes. (Helfreich, Boll.) On the other hand, Dietl and Plenk found that the fundus of a rabbit, which was still red, and seen with the ophthalmoscope, after death by hemorrhage, was whitened by injecting milk into the carotid artery. Still the retina, when removed, showed its red tint. The fundus of the frog appears always bluish-gray, whether the retinal red exists or is bleached. Boll thinks that this is due to the processes of the pigment-epithelium, which give the retina the optical character of a cloudy medium. If, however, the anterior half of the frog's eye is removed, the retinal red is seen, on looking obliquely at the retina. In mammals the retinal red is probably not the only source of the color of the fundus, inasmuch as we should expect the color of the retina to be but faint in an eye long exposed to day-light. In fact, Boll states, that the human fundus shows the deepest red color in the morning, before the eye has been exposed to day-light. H. GRADLE.

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VII.—FAT AND BLOOD, AND HOW TO MAKE THEM.

By S. Weir Mitchell, M. D.   Philadelphia: J. B. Lippincott &
    Co.   Chicago: Jansen, McClurg & Co. Pages, 101. 1877.

This plain and well written little book has a practical value
wholly disproportional to its size. It sets forth nothing new,
in fact, and not much that is new in a practical way. The mat-
ter of the work is hence not, strictly speaking, a fresh contribu-
tion to medical knowledge, but its value consists in the prac-
tical and common sense stress which is laid on certain important
facts in the treatment of that vast multitude of cases, in which
there comes to be from various causes, an impaired nutrition,
often comprising anæmia, in various degrees, and always neu-
rasthenia, and all that these terms may imply, and which con-
tinue for years often, among the opprobria of medical practice.

This little work comes to us with a *plan*, sensible and practi-
cable, for the management of such cases, a plan which commends
itself to the common sense of every physician of experience. It
comprises two main features, which correspond to the two great
phases of nutrition: *waste and repair.*

In the cases referred to by Dr. Mitchell, waste has overtaken
the process of repair, so that a thorough loss of balance as be-
tween these two factors has occurred. The dictates of a sound
experience would be to stop, or reduce to its minimum, the proc-
ess of waste, and to increase the materials for, and quicken the
process of repair. And these are the main teachings of the book.
It is essentially an expansion of a lecture by the author on " Rest
in the Treatment of Nervous Diseases," published in the " Amer-
ican Clinical Series," by Dr. Seguin. That lecture was noticed
in our pages at the time of its appearance.

The first brief chapter is on " Fat in its Clinical Relations."
Dr. Mitchell lays, very properly, much stress on the clinical
inquiry as to whether the patient " is losing or has lost flesh, is
by habit thin or fat?" He says " this question is one of the
utmost moment in every point of view, and deserves a larger
share of attention than it receives." To this we heartily agree.
In his few remarks on the intimate relations of fat to the nour-
ishment of the tissues and blood, Dr. Mitchell utters practical
truths, without troubling himself apparently as to their real order
of sequence, or what seem to be their true causal relations. We
have often noticed in our author's writings a want of a delicate
adjustment of facts in view of their relations, which bespeaks
want of care, or of practice, in the finer procedures of analytic

reflection. As instances under this head, we would cite the following passages: He says, "We too rarely reflect that the blood thins with the decrease of the tissues and enriches as they increase."

Now we would certainly reverse this statement. It is true that, roughly stated, impoverishment of the blood goes with a decrease of the tissues. But while this statement will do for practical purposes, it is certainly not true so far as the relations of the facts are concerned. The tissues do not nourish the blood of course, but just the contrary. But the statement of Dr. Mitchell strictly interpreted would seem to imply that the impoverishment of the blood *follows* the decrease of the tissues, and as a consequence of the same. Happily, however, no bad practical consequences arise from such dialectical blemishes.

Climate, in the opinion of Dr. Mitchell, " has a good deal to do with a tendency to take on fat." Alcoholism, inertness, especially such as we see in some cases of hysteria, in which there is a belief on the part of patients of incapacity to move, the use of morphia, etc. Quite interesting is the account given of certain " fat, anæmic people, usually women," in which there is often an unusually fine physical appearance, associated with great physical weakness. But no mere abstract of this chapter would do it justice. We must refer the reader to the work itself, for the suggestive, practical hints it contains.

One of the more important points to be secured in the management of the class of cases under consideration is their seclusion from disturbing influences, and especially from their customary relations, those which have surrounded them in their invalid period. Says Dr. Mitchell, " I have often made the effort to treat them in their own homes, and to isolate them there, but I have rarely done so without promising myself that I would not again complicate my treatment by any such embarrassment. Once separate such a patient from the moral and physical surroundings which have become part of her life of sickness, and you will have made a change which will be in itself beneficial, and will enormously aid in the treatment which is to follow." We have not the slightest doubt of the necessity for pursuing this course in a most firm and enterprising manner.

The next chapter is one of the most valuable, and relates to the function of *rest*, in the treatment of these worn, anaemic, neurasthenic cases. We have no expectation of impressing any member of the profession, in advance of a real personal experience, with the value of this simple matter in the management of such cases. But simple as it is, there are but few plain, practical subjects which require more detailed thought, more in the way of specific direction, and personal inquiry and supervision, in order to insure success.

But if such patients are actually unable to take exercise voluntarily, how shall we avoid on the one hand the evils of pro-

tracted inactivity and secure the advantages of exercise? "By *massage*," says Dr. Mitchell; viz., by *passive exercise.* And here again, about one physician in five hundred will probably be led to feel the real value of this phase of the treatment of the cases being discussed. It is really quite a study to know *how*, *when*, and how forcibly, or how long such treatment should be used; how to adapt it to various cases, and to what extent it may profitably supplant *active* exercise. Nothing less than a volume would suffice in which to do the subject justice.

The next chapter is given to the subject of electricity. Dr. Mitchell uses the induced current, and moist sponge electrodes to the back of the neck and to the feet,—the spinal current,—while the patient reclines, and he also employs a sort of electrical massage with the hand. But nearly everything will depend on the studied tact, the intelligence of the physician or operator, for if not tactfully used, the patient if not pained, may be thoroughly and needlessly disgusted by the blundering attempts made in the use of the electricity. We agree with Dr. Mitchell's estimate of its value.

The longest chapter in the work is on "Dietetics and Therapeutics." Dr. Mitchell finds it necessary to pay great personal attention to this matter. Every one will admit at first sight the importance of good feeding in such cases, but only the few will ever give that attention to the subject which will enable them to declare in a positive and thoughtful way what good feeding is, and just what will be adapted to different cases. Only the few will give those minute directions as to diet, or will see to it from day to day that they are carried out, which are so necessary to success. But here lies one of the chief difficulties of the case. Physicians content themselves as a rule with the delivery of generalities in regard to rest, diet, exercise, etc., which it is vainly expected patients will apply to their courses of life. Any plan, however good, must as a rule fail under such circumstances.

Dr. Mitchell lays much stress on a "skimmed milk" diet, in the outset, in such cases as are referred to, reminding one of the so called "milk cure." Then follows in due time beef tea, beefsteak and other highly nutritious articles of diet, given in as large quantities as the patients can bear, with the view of *fattening* them. To this is added large doses of iron, at meals, either the sub-carbonate or the dialysed iron, and also extract of malt. But this latter does not seem to be what we ordinarily meet with, such as Liebig's, Loeflund's, and other extracts, but a species of strong beer or ale, "Hoff's Extract of Malt," and which is to be taken at meals.

By means of seclusion so as to avoid unnecessary friction or excitement, and in this way to diminish nervous waste, by rest or freedom from fatigue, and by the substitution of passive for active exercise, by the stimulating and soothing influence of electricity, by the best of food, and suitable restorative tonics, all

managed down to the finest details under the direction of the physician or a trusted attendant, by such means would Dr. Mitchell finally rescue such patients from the slough of despond into which they have fallen. Our own experience has been for years confirmatory in the main of Dr. Mitchell's, but we have found out long since that success can only come about by the most thorough and absolute attention to the plan of action, by taking from three months to a year in which to accomplish a cure, and above all, by not simply recognizing such a course as practicable and useful, but by faithfully *doing* the things recommended.

We can cordially recommend this little work to our readers.

SHORTER NOTICES.

I. A SERIES OF AMERICAN CLINICAL LECTURES. Edited by E. C. Seguin, M. D. Vol. 1. New York. G. P. Putnam's Sons. 340 pages.

II. TRANSACTIONS OF THE AMERICAN MEDICAL ASSOCIATION. PRIZE ESSAY. SUPPLEMENT TO VOL. 27, 1876. EXCISION OF THE LARGER JOINTS OF THE EXTREMITIES. By H. Culbertson, M. D. Philadelphia 1876. 692 pages.

III. ALCOHOL AS A FOOD AND MEDICINE. A paper from the Transactions of the International Medical Congress at Philadelphia, Sept., 1876. By Ezra M. Hunt, A. M. M. D. New York, 1877, National Temperance Soc. and Publication House. 137 pages.

IV. THE MORTALITY OF SURGICAL OPERATIONS IN THE UPPER LAKE STATES, compared with that of other regions. By Edmund Andrews, A. M. M. D. assisted by Thomas B. Lacey, M. D. Chicago 1877. 123 pages.

V. THE CURABILITY OF INSANITY. By Pliny Earle, A. M., M. D. Read before the New England Psychological Society on retiring from office as its President, December 14, 1876, and published by that society. Utica. 1877. 52 pages.

VI. SONS OR DAUGHTERS? CHOOSE! By George B. Starkweather. Hartford, 1877. 411 pages.

I. Glancing over the twelve lectures constituting the series of 1876, one may be in doubt as to the class of readers for whom they are intended. None of them are—strictly speaking—original. Many of them, however, are not

only suggestive, but really profitable to the general practitioner, combining as they do, personal observation and reflection of the author's with the previous knowledge on the subject. Some others, however, are so elementary in their nature and so entirely devoid of originality, that they are scarcely in place in the volume before us.

The first paper by Dr. Bartholow on "Principles of Physiological Antagonism" treats mainly of the action of anti-pyretics. It is an instructive and really suggestive article. In his experiments, however, on the reduction of temperature by drugs, he overlooks the fact, that *fettering* alone suffices to reduce the temperature of rabbits by several degrees.

In Dr. Jewell's paper on morbid sensibility, we find essentially his theory of mal-nutrition of the nervous system as the cause of neuralgia and hyperæsthesia. His views on the subject have been previously stated in this Journal.

In his article on the treatment of melancholia at home. Dr. Seguin gives an admirably clear description of the disease, while his remarks on the treatment—the result of the author's observations—are also very satisfactory.

Dr. Delafield's lecture on Dyspepsia is an instance of preciseness, without losing thereby in interest.

The following two lectures by Noyes and Lefferts need no farther criticism than our initial remarks.

While the succeeding articles by Sands and Wier are also beyond the scope of this JOURNAL, treating as they do purely surgical subjects. they deserve mention. however, as an interesting and profitable *resume.*

Dr. Hammond's paper on spinal irritation is written mainly in defense of his hypothesis on hyperæmia of the posterior spinal columns as the cause of that class of symptoms. We agree with him in the utility of *cautious* hypothesis as a spur to further investigations. We also admit that the objections which may be and have been raised against the admissibility of his hypothesis are more theoretical speculations, than arguments on solid basis, but the same must also be said of the hypothesis itself.

The last lecture by Dr. Miles on "Peripheral Paralysis." is a judicious summing up of our knowledge on the subject, without, however, adding thereto   G.

II. This work appears like a very complete monograph of the subject of excisions. and the fact that it received the prize of the American Medical Association, and the honor of special publication by that body, is the best confirmation of the impression produced at first sight. The author tabulates and discusses nearly four thousand cases of excisions of the larger articulations of the body. the hip, shoulder, knee. elbow. ankle and wrist joints, analyzes them according to all the conditions of operations, age. climate, country, etc., and gives rather

briefly the conclusions that seem to him to be warranted. The work gives evidence of great care and industry in its compilation and will be considered as a very valuable contribution to the surgical literature of the day.

III. At the meeting of the International Medical Congress at Philadelphia last year, memorials were received from the National Temperance Society, and other similar associations requesting an authoritative medical statement of the ill effects of the use of alcohol as a beverage. In the medical Section to which these memorials were referred, the paper of Dr. Hunt, republished in the present form, was read and elicited a lively discussion, and after various propositions had been offered, its conclusions were adopted as expressing the sentiments of the Section on this question and as such were transmitted to the memorialists. These conclusions are as follows:

1. Alcohol is not shown to have a definite food value by any of the usual methods of chemical analysis or physiological investigation.

2. Its use as a medicine is chiefly that of a cardiac stimulant and often admits of substitution.

3. As a medicine it is not well-fitted for self-prescription by the laity, and the medical profession is not accountable for such administration or for the enormous evils arising therefrom.

4. The purity of alcoholic liquors is in general not as well-assured as that of articles used for medicine should be. The various mixtures when used as medicine, should have definite and known composition, and should not be interchanged promiscuously.

We presume that there are very few, if any, who will disagree with these two last conclusions, and the others seem to us equally safe to adopt. We can honestly recommend this *brochure*, as a rational, and we trust an effective, medical temperance tract.

IV. Dr. Andrews has collected in this memoir, all the available and reliable statistics in regard to the mortality of a number of the principal surgical operations in the Upper Lake States, and compared them with those collected elsewhere, and makes generally a very favorable showing for the former. No geographical boundaries are given of the region here called the Upper Lake States, but the cases were collected mostly from the vicinity of Chicago and Milwaukee, and some, perhaps, as far south as Cairo, Ill. The discussions of the cases is well done, and the work is, for its compass, a very excellent one of its kind.

V. This paper touches a matter that is well worth attention, the influence of the personal error in the statistics of insanity. If the statistics of the cures of insanity are vitiated, either by pardonable errors of judgment, or by inexcusable misrepresentations induced by a desire to show the most favorable results from

the treatment employed, the public suffers, not merely on account of the deception, but in other respects. The State builds costly asylums for the insane, in which, on the strength of these statistics, a certain per centage of cures to admissions is annually expected. They are not intended as receptacles for the chronic pauper lunatics, who are turned out on the general public or suffer from insufficient accommodation, and often even from maltreatment in county almshouses and jails. We believe, also, that many cases that, for the protection of society, should be kept in restraint, are all the while turned loose in lucid intervals as cured, or as incurable but harmless. A recent murder trial in this city (Chicago) is evidence of this fact.

Dr. Earle goes over the whole question historically, reviewing the statistics of asylums in this country, and what he demonstrates can best be stated in his own words. His conclusions are:

1. " That the reported recoveries from insanity are increased to an important extent by repeated recoveries from the periodical or recurrent form of the disease in the same person, and consequently, that

2. " The recoveries of *persons* are much less numerous than the recoveries of *patients* or *cases;* and, consequently—

3. " From the number of reported recoveries of *cases*, or *patients*, it is generally impossible to ascertain the number of persons who recovered.

4. " The number of reported recoveries is influenced, sometimes largely, by the temperament of the reporter; each man having his own standard, or criterion, of insanity.

5. " The large proportion of recoveries formerly reported, were *often* based upon the number of patients *discharged*, instead of the number admitted, and, generally, upon the results in a number of cases too small to entitle the deduction therefrom of a general formula of scientific truth, and those proportions were evidently increased by that zeal and (for want of a better word) rivalry which frequently characterize the earlier periods of a great philanthropic enterprise.

6. " The assumed curability of insanity, as represented by these proportions, has not only not been sustained, but has been practically disproved by subsequent and more extensive experience.

7. " The reported proportion of recoveries of all cases received at the institutions for the insane, has been constantly diminishing during a period of from twenty to fifty years."

Dr. Earle's statements and the figures by which he supports them, confirm us in the belief that the duty of the State to the insane is not fulfilled by the present practice. One great or fundamental evil in the system now in vogue is the expense, not only in the management, but in the original cost of the asylums, which confines the benefits of the appropriation to a portion only of the unfortunate insane, while others equally deserving the

bounty of the public are thus needlessly subjected to hardships, and often to actual abuse. It is time that medical men should attempt to model public opinion in this regard, and it is the intention of the editors of this JOURNAL, as far as it is in their power, not to leave the field neglected.

VI. This work discusses in a popular way, but, on the whole, with perfect regard to propriety, the rather peculiar subject of the production of the sexes. The author advances the theory, that the stronger of the two parents impresses the sex of the weaker upon the offspring, and supports it with over three hundred pages of entertainingly written matter, showing certainly a very considerable degree of industry and literary research. It does not seem to us necessary to here examine his arguments or to discuss his views, we will only say that the former do not appear to us any more conclusive in favor of the latter than are first appearances. Still, while we cannot say that the book is a very important contribution to scientific literature, it is well gotten up, and will doubtless obtain many readers.

Editorial Department.

IN this number of the Journal we publish a review, by an Eastern contributor, of Dr. Bucknill's pamphlet, on " American Asylums," which seems to call for comment. It deals, among other things, with the spirit and general accuracy of Dr. Bucknill's paper, and as we think, justly animadverts on the same.

It seems hardly possible as yet, for an American to represent English institutions, and customs, to the satisfaction of Englishmen, and certainly it is true, that Englishmen seldom see and represent American institutions and customs so as to satisfy a well-informed American. It is not often that we find flagrant cases on their respective sides, such as Dickens and Hawthorne, but such as they are, they are far from rare.

But whether the mutual ignorance and prejudices, concerning each other, of England and the United States, are disappearing or not—and we think they are disappearing—there can be no doubt they continue to exist, and the pamphlet of Dr. Bucknill's is a proof of it. He seems to have laid himself open not only to the strictures of our contributor, but also to those much more severe, but not less just of Dr. Joseph Parrish, well known to the profession in this country, who has published an open letter to Dr. Bucknill, in reply to some of his misstatements in regard to the workings of Inebriate Asylums in this country.

In the course of his letter, Dr. Parrish is led to say, " It is very evident, my dear doctor, that you did not visit our institutions with the purpose of learning much about them. You did not see them in any earnest spirit of inquiry and ex-

amination. You display a very discreditable degree of ignorance of their polity, and of the laws by which they were created, and under which they exist," etc. (*Journal of Mental Science*, October, 1877. P. 455.) And we are sorry to say, from the facts of the case, that the complaints and criticisms of Dr. Parrish appear to be justified in no small degree. We hope to see in the future the two great English speaking peoples more intelligent in respect to each other, and more tolerant of mutual advice and criticism, that they may finally see, more nearly eye to eye, on all great questions involving their welfare, and their relations to the rest of the world.

The other point in the review of Dr. Bucknill's paper, to which we wish to call attention, is that of the expense of American asylums, and more particularly those in the great State of New York, to which particular allusion is made in the pamphlet of Dr. Bucknill. It is contended by our contributor that Dr. Bucknill was not correctly informed in relation to this subject, and the New York State Asylum at Utica, under the able management of Dr. J. P. Gray, is specially mentioned, and the figures of the asylum for the past few years are discussed in an interesting and suggestive manner. So far as we can see, the statements are reliable, and seem to show, to say the least, that a fresh consideration of the question of the expenditure of the moneys contributed for the support of our public charities, is needed. For it seems to us that it can be made clear, that not only is the average cost of maintenance of pauper patients often far above the liberal necessities of their cases, but that the cost of the buildings erected are often on such a scale of extravagance as to amount to a waste—absolute waste, in view of the requirements of the case—of one-half of the means contributed toward their erection. We now refer to such institutions, for example, as those being erected at Buffalo, and Poughkeepsie, in New York State, and at Danvers, Massachusetts. In these brief remarks, we do not forget that the asylum at Utica belongs to a class called " mixed," where both public and private patients are treated. This latter class are more expensive, as a rule, than the public or pauper patients. And when, as appears to be true at Utica, the published reports give us no means for

knowing how many of the whole number are private and how many pauper patients, it is of course impossible to compare such institutions with those which are purely charitable. But we are to return to a discussion of this whole subject in a future number.

When the cost of providing quarters for each pauper or indigent patient exceeds seven hundred to one thousand dollars, we feel that money is being needlessly expended, and when the expense of providing quarters for each patient rises from $3,000 to $5,000, we feel there is almost, if not quite, a criminal sacrifice of means, to the carelessness of Boards of Trustees, and to enable ambitious architects to immortalize themselves at the expense of funds wrung from the taxpayers in the name of the indigent insane.

But we cannot give adequate attention to this subject at present. It is our intention, however, to consider it to an extent commensurate with its importance, in the forthcoming volume of our JOURNAL, in relation to which subject we think we can furnish our readers with matter for thought.

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The W. & S. Tuke Prize of one hundred guineas, for "the best series of original cases and commentary, illustrative of the somatic etiology of various forms of insanity, accompanied, when possible, in fatal cases, by reports of postmortem examinations and microscopical preparations, their bearing on the symptoms being pointed out," has been awarded to our correspondent, Dr. E. C. Spitzka, of New York. According to the conditions of the competition, as stipulated by the British Medico-Psychological Association, it was open to all the world, but the right to withhold the award was reserved if there was no essay of sufficient merit. While of course there is no nationality to science, we must still admit feeling a considerable degree of satisfaction in having this prize awarded to one of our American neurologists.

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The Index to the present volume will appear with our January issue of the coming year.

𝔓eriscope.

a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

THE NERVES OF THE LABYRINTH.—Horbaczewski, in a communication made to the Academy of Vienna, gives the results of his investigations on the nerves of the internal ear in man and animals. He finds that in the sheep the nervus vestibuli and the nervus cochleæ are completely separated from each other at their origin. The primitive fibres of the cochlea remain always much finer than those of the vestibular nerve. It is remarkable that the size of the vestibular nerve increases with the size of the animal in a much more rapid ratio than the size of the cochlear nerve, so that, for example, the nervus cochleæ of the horse appears much more slender, as compared with the nervus vestibuli, than that of the rabbit or of man. In the sheep the nervus cochleæ is distributed exclusively to the cochlea, and the nervus vestibuli to the remaining portion of the internal ear. But this distinction is by no means so sharply marked either in man or in the horse. In the horse there is constantly an exchange of fibres, though a comparatively slight one, between the two nerves. In man the two roots form a common trunk, which subsequently divides into the two nerves. The nervus cochleæ is not, however, exclusively distributed to the cochlea, but gives off a fine branch, which runs in the recessus cochlearis to the vestibular extremity of the ductus cochlearis, and through the macula cribrosa to the septum of the two sacculi contained in the vestibule. He thus demonstrates the correctness of the statement made by Flourens, that the vestibular nerve is a completely distinct pair from the auditory nerve, or nervus cochleæ. *Lancet* (Am. Rep.) July.

THE SEGMENTATION OF THE HEAD.—By slow degrees an approach is being made to a true understanding of this most difficult and interesting question. The old explanations by archetypes and by the structure of the highest developed skulls, have fallen into disfavor. Attempts to settle the cranial segments by considering the distribution of nerves in the adult have been shown to be unsafe, because nerves are necessarily adaptational in their character, and liable to the greatest modification on changes taking place in the organs they supply. The development of nerves, however, is a much surer guide, showing primitive and funda-

mental characters. The nerves behind the ear are five in fishes, although the number of strands of which the vagus is made up in some cases points to a loss of distinct nerves and segments in the hinder part of the head. The auditory and facial nerves originate as one, so that the auditory appears as a specialized portion of the facial. The trigeminal likewise arises as a single nerve, and in front of this there is no nerve having a similar history to these and the spinal nerves. Thus we have an indication of seven segmental nerves issuing from the brain case. When the visceral clefts are considered, we find in sharks six clefts indicating seven segments, or one more, if the mouth be regarded as a cleft. The head-cavities between the outer wall of the head and the mucous membrane of the throat, discovered by Mr. Balfour in sharks, furnish a similar number. They are eight in all, one premandibular, one mandibular, one hyoid, and five branchial. Thus the examination of three sets of organs leads to the assignment of eight body segments to the head. But the question is far from being settled so long as the brain-case itself and the brain cannot be satisfactorily explained. *Nature*, Aug. 9.

The Mode of Contraction of Muscles.—At the session of the Soc. de Biologie, July 6, (rep. in *Gaz. des Hopitaux*), M. Onimus presented, in the name of M. Trouvé, an electric apparatus designed to show the mode of muscular contraction.

" M. Trouvé, struck by the considerable effects produced on the muscles by a feeble electric current, thought that here ought to be found one of the principal receivers of the electro-motive force. Directing his experiments with this idea, the result was the construction of an apparatus that answered to all the functions of muscle.

" M. Trouvé compared the active molecules of the muscle to little electro-magnets, attracting each other by their opposite poles. It is easy to understand how such a mechanism acts. The action of two electro-magnets, multiplied by the square of the section, gives a fair idea of the work done by the system, and of the amplitude of the movement, but it does not account for the very considerable effects observed in the muscle. Therefore, M. Trouvé, continuing his researches, obtained the proof that it was necessary to totalize each individual effort of the electro-magnets, since this total ought to give mathematically the result of the whole power of the mechanism, and correspondingly a better idea of that of the muscles.

" What mechanism could now sum up thus these effects ? M. Trouvé, remembering the child's toy, consisting of articulated paralellograms on which toy soldiers are arranged and moved, constructed an apparatus composed of a series of electro-magnets attracting each other by their opposite poles, and connected by articulated parallelograms which totalize all their actions.

" Without presuming to reproduce in any way the form of the muscle or pretending to imitate all its movements, this little apparatus never-

theless explains nearly all its properties, and permits us to formulate the following theory.

"The power of the muscle is the resultant of all the molecular attractions. This little apparatus explains in a very satisfactory fashion the total contraction of a muscle by localized electrization (method of Duchenne de Boulogne), without having recourse to reflex actions or the propagation of molecular connection. It permits, also, an explanation of the persistence of muscular contraction in its effects by the persistence of magnetism."

DISSIMILAR PROPERTIES OF TACTILE AND ELECTRIC SENSATIONS.—We take the following from the report in the *Gaz. des Hopitaux* of the proceedings of the Soc. de Biologie, July 28th, 1877.

M. Bloch had, in May, 1875, submitted to the Soc. de Biologie certain experiments which he had performed to test the rapidity of the sensory nerve current in man.

"These experiments," said he, "were based upon the sensations of tact, and the excitation employed consisted in mechanical shocks given by a flexible index fixed in a fly-wheel, and striking at each turn the part upon which he was experimenting.

"I have undertaken these experiments again, but substituted an electric shock for the mechanical one formerly employed.

"My first care was to determine the duration of the persistence of the electrical sensations, and, in seeking this element of the problem, I have met with results altogether unexpected, which form the subject of the present communication.

"The electric shocks were given by two breakings of the current, by means of two induction coils.

"The closing and opening of the current are made upon one of Mareys polygraphs, covered with paper and carrying a smooth, narrow metallic band, over which two rubbers are made to pass successively. These may be separated to a greater or lesser extent at will.

1. In receiving the two shocks at the same point, say on a finger, they will be fused, and there will be a single sensation for the interval of 1-31 second.

"This figure approaches very nearly those given by many physiologists. If it is a little smaller than that of others, if it consequently proves more delicacy and a distinction of the two electric shocks pushed farther, this holds good especially when I receive two shocks only, and not a continued succession of them. In this latter case the sensibility shows itself very quickly, and the fusion of sensations takes place with still greater intervals.

2. "If one of the shocks is received on the index and the other on the median finger, we feel them perfectly distinct at the interval of 1-31 of a second, and the combined sensation appears only when they are 1-43 of a second separated.

3. "With the index and the auricular of the same hand, we distinguish

the first from the second shock very clearly at 1.43 of a second. It is necessary to decrease the interval between them to 1.62 of a second in order to produce the fusion of the two sensations and not to be able to recognize the first from the second.

4. "With the two hands, at 1.62 of a second, the order of the shocks is easily perceived, even without knowing it beforehand. The synchronism of the two sensations appears only at 1.83 of a second. For the two feet the limit is the same, 1.83 of a second. Thus in this latter case the persistence of the first sensation endures no longer than when we try the two hands, the reverse of what we observe with mechanical shocks. This last I demonstrated in my researches in 1875.

5. "In the case of the mechanical shocks, which I have only studied only in parts quite distant from one another, I found that the interval required to obtain synchronism is always the same, 1.45 of a second, whether we use the two hands, or the thumb and the auricular of one hand, or the thumb and index of the same, and that the persistence of the first sensation is rather a constant fact.

"The sensation of tact and the electric sensation have, therefore, quite different peculiarities as regards the question before us.

"Another corroborating fact. While the distinction of the two electric shocks is difficult when they strike the same point, since the fusion takes place at 1.31 of a second, inversely, the same part struck mechanically with two blows, they are dissociated at least as well as when at two neighboring points, and perhaps even with greater delicacy."

THE EFFECTS OF MECHANICAL, CHEMICAL, AND ELECTRIC EXCITATION OF THE PNEUMOGASTRIC.—MM. Dastre and Morat communicated to the Soc. de Biologie Aug. 11 (rep. in *Le Progrès Médical*) a note on the effects of mechanical, chemical, and electric excitations of the pneumogastric in the tortoise. The arrest of the heart from excitation of the pneumogastric, as is well known, does not occur except when this excitation is made by the use of electricity, the current frequently interrupted and of a certain intensity. MM. Dastre and Morat, operating on a suitable animal, the tortoise, have obtained arrest of the heart by the habitual excitants of the nerves; the result is such as to relieve the doubts that have been produced relative to the *role* of the pneumogastric as a nerve of arrest. The following are the conclusions of these authors:

1. The pneumogastric of the tortoise is put in action under the influence of mechanical excitants, (ligature, tearing, crushing, cutting) and under the influence of chemical excitants, (glycerine); 2, the arrest of the heart is also obtained by the use of *very feeble* currents (tetanizing) hardly sensible to the tongue; 3, the arrest of the heart is also obtained by the single discharge of induction, by the opening or closing of a strong current. The effect of the opening is always greater than that of the closing of the current. These results were very neatly observed during the summer season with a temperature of 20° to 25° c. (= 68° to 95° F.) The pneumogastric being usually considered as producing the arrest only under the

influence of a very strong tetanizing electric current, some physiologists have been led to consider it as only an ordinary motor nerve, a strong excitation of which caused the paralysis and stopped the heart, while a feeble one accelerated its pulsations. MM. Dastre and Morat show, on the other hand, that the pneumogastric responds in only one way to excitations, no matter what their nature, their intensity and their rhythm. This action is always *arrest in diastole.*

ELECTRIC VARIATION IN THE CARDIAC MUSCLE.—Muscular contraction it is known, is always accompanied with electric phenomena; the difference of electric potential between two points of a muscle undergoes a diminution, which, according to Bernstein, precedes by about 1-100th of a second the contraction of the muscle. This *electric variation* has been observed on various muscles, and in particular on the heart, (by DuBois Reymond and Kuehne,) and recently M. Marey has represented it graphically by photographing the indications of a Lippmann capillary electrometer. We learn from the *Journal de Physique*, that M. De la Roche has tried the experiment on the heart of a living man. Two points of the epidermis of the chest were connected with the poles of a capillary electrometer, by means of electrodes, formed each of a bar of amalgamated zinc, with a plug of muslin at its lower end saturated with sulphate of zinc. Held with insulating handles, the bars were applied, one with its plug opposite the point of the heart, under the left nipple, and the other to another point of the chest. The mercurial column was then seen to execute a series of very distinct periodical pulsations synchronous with the pulse; each pulsation even marked the double movement of the heart, (of the auricles and ventricles.) The amplitude corresponded to about 1-1000th Daniell. *Nature,* June 14.

THE NERVES OF THE DURA MATER.— At the session of the Soc. de Biologie, Aug. 4, (rep. in *Le Progres Médical*), M. Duret made a communication, the principal conclusions of which are as follows: 1. The excitation of the nerves of the dura mater produces, by reflex action, convulsive movements of the corresponding side of the body, and sometimes, also, of the opposite side, in the face, the trunk, and the members. These facts were already published by M. Bochefontaine M. Duret confirmed the observations of his predecessor; but he noticed, further, that if we inject through the hole in the skull a small quantity of coagulable or irritant substance, incapable of acting by compression, we observe a veritable tetanic contraction, a pleurosthotonos of the same side of the body.

2. The nerves of the dura mater may act, by reflex action upon the pupils, dilating them, especially the one on the corresponding side (a fact already noted by M. Bochefontaine). 3. Their *brusque* excitation may produce a veritable respiratory syncope and death from contraction of the diaphragm. 4. They act also by reflex action on the vaso motor

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nerves of the eye, and of the brain of the same side, and this action is rapid and powerful. 5. By the same mechanism they may augment the tension of the cephalo-rachidian liquid, and that of the general circulation.

6. It is probable, also, that they act on the secretions, since in the animals subjected to experimentation it is not uncommon to see the occurrence of salivation, micturition and involuntary dejections.

In a clinical point of view M. Duret called attention to the importance of the observations of M. Bochefontaine and himself. Inflammation of the dura mater, as he had shown on a dog in which he had injected iodine under the skull, reveals itself by convulsions, and often by contractures of the *corresponding side*, and sometimes on the opposite side. It will be henceforth possible, therefore, for the surgeon to ascertain whether a bony splinter or a foreign substance acts on the dura mater or on the nervous substance of the hemispheres; for if the dura mater is injured or irritated, the convulsions and the contracture follow on the same side, while if they were produced by encephalitis they would be on the opposite side. The state of the pupil, of the globe of the eye, of the respiration, of the circulation, etc., will permit us to assure ourselves of the diagnosis. It is not a mere supposition, guided by the existence of these signs a distinguished physician of Bernay, formerly *interne* of the hospitals, Dr. Blain, suspected in a wounded man suffering from fracture of the skull with depression, a splinter irritating the dura mater. He performed the operation of trephining, and saved the patient.

THE FUNCTION OF LANGUAGE.—At the session of the Acad. de Médicine of Paris, July 23, M. Edward Fournié, physician at the Deaf and Dumb Institute, read a communication, which is reported as follows in the *Progrès Médical.*

In the preliminaries of this memoir the author repudiated the introduction of philosophical systems into physiological studies, and he shows that the psychological method, resting simply upon each one's own feelings, is an unreliable and incomplete instrument in the hands of the philosophers, when they pretend to study the physiology of the brain under the name of psychology. Nevertheless, as the determination of the psychic elements is a necessary preliminary in every study of the brain, M. Fournié recognizes the necessity of the use of the psychological method, on condition that it is employed in concert with pathological anatomy, experimentation, and under the direction of a physiological analysis.

After these preliminaries, M. Fournié examines, critically, the prevalent doctrine of the localization of the function of speech in the third left frontal convolution. This, as every one is aware, is the doctrine of MM. Bouillaud and Broca. The author accepts all the facts of pathological anatomy on which this doctrine is supported, but he proposes to prove that these facts are not properly open to the interpretation given them: 1. Physiological analysis of intelligent movements, of which speech is the

highest type, informs us that in the execution of these movements, phenomena of sensibility, of memory, under the control of the judgment and the will, and phenomena of movement directed by the special, senses of sight or hearing, enter as necessary elements of the case. But it is impossible to admit that all these activities, which we find, moreover, in every cerebral functional activity, are localized in the third left frontal convolution. 2. Anatomy informs us that in all points of its extent, the nervous system is subject to laws of organic symmetry, and consequently to laws of functional symmetry.

The loss of speech from a lesion of only one side of the brain does not prove speech is localized on that side; it proves that the two sides are absolutely indispensable for the formation of speech. In support of this opinion, M. Fournié shows that if in the phenomena of sensibility and of memory the two hemispheres can act for each other, it is not so for the excito-motor phenomena, which have an analagous *role* on the two sides, but distinct as to the locality of the result obtained. To complete these demonstrations, M. Fournié submitted the formation of the word to a very delicate analysis; he considered it first as a sensory phenomena, then as a phenomenon of movement, and closed with the following conclusions: 1. The material conditions of speech considered as a sensory phenomena, are found in either hemisphere; 2. The material conditions of speech considered as a motor phenomenon, are indispensably connected with both hemispheres; 3. Contrary to the opinion of MM. Broca and Bouillaud, it is not possible to admit that the material conditions of speech are localized in the third left frontal convolution, notwithstanding the exactness and the authenticity of the facts of pathological anatomy on which this view is supported.

VASCULO-CARDIAC REFLEXES OF SENSORIAL ORIGIN.—At the session of the Soc. de Biologie, June 30, (rep. in *Gaz. des Hopitaux*), M. Charpentier made, in his own name and that of M. Couty, a second communication on the mechanism of the vasculo-cardiac reflexes of sensorial origin.

These reflexes are essentially variable as regards their form and intensity, with certain individual or experimental conditions, and in the same animal, with the duration, the intensity of action, and the repetition, but not with the nature of the external excitant.

The variations of tension are independent of the cardiac troubles; these cardiac disorders are transmitted solely by the pneumogastrics.

The integrity of the brain is indispensable for the production of vasculo-cardiac sensorial reflexes; and *en resumé* it is not the sensation itself that causes these reflexes, but a more remote inconstant cerebral action of an emotional nature, thus acting through the intermediation of the mesencephalon.

CEREBRAL THERMOMETRY.—At the session of the French Association for the Advancement of Science, Aug. 29, (rep. in *Le Progres Médical*) M.

Broca read a remarkable communication on cerebral thermometry and the *role* it might play in the diagnosis of diseases of the brain. To obtain this temperature he used a finely graduated thermometer, one face of the bulb applied against the cranium and the other enclosed in a sack enveloped in layers of wadding. The external temperature, therefore, could not affect the mercury. Generally M. Broca employed six of these appliances arranged in a sort of crown around the head. In this way he obtained the temperature at six different symmetrically disposed points on the circumference of the cranium; the two anterior thermometers placed directly behind the external orbital apophyses, the two middle ones above the ears in the temporal region, and the posterior ones in the occipital region. For the sake of brevity, he gives a name to each of these thermometers; those of the left side he denominates F., (frontal) T. (temporal) and O., (occipital); those of the right side correspondingly, F'., T'. and O'. When we add together the figures given by these thermometers and divide the sum by six, we have the mean temperature of the head; but each thermometer gives only the temperature of the point to which it is applied, and we can compare this figure with that of the others. This comparison is the one thing that gives us important results.

The experiments of M. Broca were begun in 1869, but it is especially since 1873 that he has applied his researches to the diagnosis of cerebral affections. In order to have a basis of observation it is needful to know, first, the physiological temperature of the various regions of the head. For this purpose he experimented upon the *externes* and the attendants of his service who possessed nearly the same intellectual development, and he endeavored as much as possible to have each of his experiments performed under identical conditions. But here, this difficulty presents itself; the thermometer is, as we have said, applied to the cranium, and is separated in consequence from the substance of the brain by media of various thicknesses. F. and F'., situated at the external part of the temporal form, are only separated from the brain by a thin layer of muscle and by the bone itself, which is here not of much thickness. At T. and T'. we do not have the muscle, and the bone is not very thick, but the hair (which, by the way, is removed as much as possible out of the way) is a bad conductor. At O. and O'. we have also the hair, and the thickness of bone is greater than at the temporal fossa. Nevertheless, these sources of error are truly too slight to explain the very noticeable variations of temperature that exist between these different regions.

The averages given were obtained from twelve *externes* and attendants at the Hospital des Cliniques, placed as nearly as possible under the same physiological conditions, the thermometer being left in place in each examination some twenty minutes. The maximum temperature of the brain was found to be 34° 85 c. (= 94° 70 Fahr.), the minimum, 32° 80 (= 91° 04 F.), the mean temperature should, therefore, be 32° 82. But if we compare the left thermometers, F., T. and O., to the right ones, F'., T'. and O'., we notice that, as a regular thing, the left side temperature is higher than that of the right. Thus on the right side the mean temperature was 33° 90 (= 93° 09 F.), while on the left it is a little over 34° (= 93° 20). In

the normal condition, therefore, the temperature of the left side is higher than that of the right, to the amount of about one-tenth of a degree centigrade. But, what is remarkable, this inequality only exists when the brain is in a state of rest When the brain is at work there is a tendency to establish an equilibrium, and for the two hemispheres to show equivalent figures. It is not necessary to suppose with M. Broca that the left hemisphere is better irrigated than the other, that it receives a greater quantity of blood; but that when the brain is in action, as the right hemisphere is less prepared and less mobile, it is compelled to greater efforts, the call for blood is louder on this side, and tends to produce an equilibrium between the two hemispheres.

M. Broca does not rest here, but having compared the two sides of the brain, he tested the different lobes of the same hemisphere and found that the temperature of the occipital lobe was 32° 92, that of the temporal lobe 33° 72, and that of the frontal 35° 28. It will be seen from these figures how the functional activity of the frontal ought to exceed that of the temporal or occipital lobes.

Such are the results obtained by M. Broca in the brain at rest. In action, however, the figures are different. It was difficult to apply to all subjects an equal test not harder for one than for the other. M. Broca tried that of reading, about equally familiar to all, at least to all medical students. After ten minutes of reading aloud, the mean temperature arose from 33° 82, the normal mean, to as high as 34° 23. We have here, therefore, a difference of nearly half a degree cent. in favor of the active brain.

The clinical researches of M. Broca are no less important; he has been able to demonstrate by the thermometer a certain sign of cerebral embolism; he has been able to determine the portion of the brain deprived of sanguine irrigation. For a long time already he had shown that curious phenomena of temperature accompanied embolisms in the members. He had shown what, moreover, might be *a priori* expected, that the general temperature of the limb became lowered, but at the point itself where the stoppage took place there was a rise of temperature. In cases of ligation it has been attempted to explain this apparently paradoxical condition by the constriction of the nerves. But this explanation is not valid in cases of spontaneous obliteration, as in embolism, since the nerves cannot be injured by the lesion. M. Broca explains this local rise of temperature by the collateral circulation that is established; the blood no longer passes through the deeper lying vessels, it penetrates and dilates the superficial vessels, and the peripheral circulation being more active, the temperature will be raised. The maximum of temperature will, therefore, be at the horizon of the embolism.

Nothing of this kind can occur in the brain, since the vessels of the collateral circulation are not sufficiently numerous. When cerebral embolism occurs, seven times out of ten the clot reaches the sylvian artery, and either obliterates it completely or one of its trunks. In this case what should we expect to see? The blood, entering no longer into the territory supplied by the sylvian artery, the temperature will be lowered in the cor-

responding thermometer, but the irrigation will be more active in the frontal and occipital lobes, and the mercury will rise in the corresponding thermometers.

Two cases completely bearing out this theory, as far as they went, were given by M. Broca, and the position may be accepted, therefore, that in the region of the embolism there is decrease of the temperature of the brain.

THE COURSE OF THE FIBRES IN THE NERVE CENTRES.—Flechsig' *Centralblatt*, No. 3, publishes the following as the results of some of his more recent researches.

1. Each pyramid of the medulla (compare my definition given elsewhere), continues, without being interrupted in ganglion cells, through the pons, crus, and inner capsule, into the centrum semiovale of the corresponding hemisphere, and preferably into the tract corresponding to the central convolutions.

2. The continuation of the pyramid fibres forms in the crus and inner capsule, a compact cord, the situation and limits of which are rather satisfactorily established; in the external capsule it has generally an eliptical section, and passes between the lenticular nucleus and the middle third of the thalamus.

3. The tracing of the formation of the medullary sheath furnishes a better topography of the fibres of the inner capsule than was heretofore attainable.

4. In the cerebrum, also, the development history and "secondary degenerations" afford accordant results as to the course of the fibres.

The observations are more fully published in the *Arch. d. Heilkunde*, 1877, 2 flg., under the head, "On 'Systemic Diseases' in the Cord."

THE ANASTOMOSES OF THE HYPOGLOSSUS. M. Holl, *Zeitschr f. Anat. u. Entwickelungsgesch.* II. S. 82 (Abstr. in *Centralbl. f. d. Med. Wissensch.*)

The anterior branch of the first cervical nerve, after reaching the inner margin of rectus capitis muscle and the anterior half of the vertebral column, and giving a twig to the muscle and anastomosing with the sympathetic, divides into two stems, an upper and a lower one, the first of these passes at a right angle to the hypoglossus muscle and disappears in its sheath; and the other descends and serves for connection with the second cervical nerve; forming generally an arched anastomosis which constitutes the first "ansa cervicalis." The second cervical nerve sends a branch up to the twig from the first entering the hypoglossus sheath, it clings closely to it, courses median-wards, also enters the sheath and runs, more or less visible, down on the convex surface of the muscular nerve of the tongue, breaks through the sheath in its passage over the horizontal stem (after giving out a twig which penetrates the hypoglossus toward the median line), and appears as the descending cervical nerve on the anterior surface of the internal jugular vein. It forms in its further course, through

anastomotic connections with the second and third cervical nerves, the so-called ansae cervicales, and together with the said nerves innervates the hyoidal group of muscles. It receives no branch from the hypoglossus, and hence the descending cervical is not formed by a descending branch of the hypoglossal and the cervical nerves (HENLE). Frequently the cervical nerve does not enter the sheath of the hypoglossus, instead of this, it rises from the second cervical and accompanies the branch mentioned from the first cervical as far as to its entry into the sheath into which it sends only a minute filament (which is soon absorbed by the hypoglossus fibres). But it does not itself enter, but separated by a perceptible interval it courses parallel with the hypoglossus, approaching it at its greatest convexity, and is connected with it by nerves which starting from the second or third, or from both, ascend to this descending branch, enter into the horizontal portion of the twelfth cranial nerve, run for a little distance enclosed in its neurilemma and are then peripherally distributed to the muscles. It is seen in such preparations how the descending cervical nerve takes its own course aside from the twelfth cranial nerve, and how this branch, on the one hand only high up, and on the other only at the convex margin is connected with it.

THE MOVEMENTS OF THE BRAIN.—MM. Giacomini and Mosso presented to the Acad. des Sciences in Paris, Jan. 3. (rep. in *Bull. Gen. de Thérapeutique*) the photograph of a woman, thirty-seven years old who, from a syphilitic disease of the cranial bones, had lost a great part of the frontal and the two parietal bones. In order to study the movements of the brain they had fitted the opening in the skull with one of Marey's graphic apparatuses.

They found that the form of each pulsation of the brain varied according to circumstances. It was different from the sphygmographic pulse of one artery, and there was not even a resemblance with traces taken on the fore-arm introduced into a cylinder filled with water and placed conveniently in communication with a Buesson apparatus.

During profound sleep, with snoring, there was a quite pronounced increase in the height of the cerebral pulsations: the respiratory oscillations and undulations became much more pronounced.

Certain causes produce the same change of volume in the brain and in the extremities; others cause variations found simultaneously in opposition in the brain and various parts of the body.

The experiments on the changes of volume of the brain and of the fore-arm of man have furnished the element of a corporative physiology of the blood vessels, and we are enabled to-day to study the modifications produced by identical excitations in the vessels of different parts of the body.

During compressions of the carotids, the cardiac pulsations disappear almost entirely. When the arterial circulation is re-established, the pulsations increase in height, and the brain, after a rapid increase of volume, presents a contraction which continues with the pulsations stronger than before.

By compressing the jugular veins, we cause an increase of volume of the brain. After twenty or thirty seconds of venous congestion of the brain, the volume of this organ begins to decrease. During the nervous congestion the pulsations increase considerably in height, and this increase remains for a considerable time, even after the re-establishment of the normal venous circulation. After the venous congestion we always observe a diminution of the volume of the brain, which is probably produced by a contraction of the vessels. An interruption of the respiratory movements produces the same effect. Very extensive respiratory movements exercise a profound influence upon the form of the cerebral pulsations, and we see in the brain the same phenomena that are produced during compression of the carotids. During the occlusion of the femoral arteries, the cerebral pulsations appear more acute and higher: at the moment when the circulation of the blood is re-established we see a rapid diminution of the height of the pulsations.

Every movement of the body and all intellectual labor, is reflected upon the brain, which undergoes a visible modification in its volume and in the form of its pulsations.

THE MODE OF TERMINATION OF THE NERVES OF TOUCH, IN MAN AND THE VERTEBRATED ANIMALS:

The following is a resume of a lengthy article by Dr. J. G. Ditlevsen, of Copenhagen, published in the *Nordiskt Medicinskt Arkiv*, Vol. VIII., No. 11:

Up to this day, as we know, our experience with regard to the termination of the nerves of touch, has been limited to the knowledge that a portion of these nerves terminate in terminal corpuscles, particularly in the tactile corpuscles of Meissner, and the end-bulbs of Krause. It was admitted that the others terminated either in free extremities, or in closed terminal nets.*

The object of the present treatise is to show that recent researches permit, in the opinion of the author, certain views otherwise entirely clear and much more satisfactory, both to the anatomist and the physiologist.

The author then passes in review all the researches, the result of which is the demonstration of the termination of the nerves of touch in terminal cells. These researches are the following: 1st, the observation of Leydig that the nerves of the *poils tactiles*, or feeling bristles (*Varborster*) terminate in special cells, found in the sheaths of the hair roots, an observation, the correctness of which was confirmed by Sertoli, several years later. 2d, the observation of Merkel on the tactile corpuscles in the tongue and beak of birds, corpuscles that in reality are nothing but groups of terminal nerve cells; 3d, the discovery by Leydig of similar tactile corpuscles in the skin of reptiles and amphibia; 4th, the establishment of the fact that in man, also, the corpuscles of Krause are groups of terminal nerve cells, (Longworth and Waldeyer); 5th, the research of Langerhans on the *Amphioxus lanceolatus*; 6th, that of Merkel on the human skin, and on that of mammals and birds; 7th, that of Ditlevsen

on the skin of frogs. All these researches show that the cutaneous nerves terminate in special terminal cells.

If we add the observations, according to which the tactile corpuscles in man and the terminal corpuscles in birds (ordinarily called Pacinian corpuscles), are probably also groups of terminal cells, which other observations are all enumerated in the original treatise, it is likely that the principle traits of the mode of termination of the nerves of touch in man and the other vertebrates, are the following: 1st, the nerves of touch terminate in cells; 2d, these are situated in the skin, (both in the corium and the epidermis) and adjacent mucous membranes; 3d, they are diffused over the entire body; 4th, they are especially numerous in the active organs of touch, where they often aggregate in groups, frequently having the appearance of small, isolated organs, but do not otherwise present any anatomical peculiarities, distinguishing them from those found in other parts of the body; 5th, the corpuscles of Meissner and Krause are nothing but groups of similar cells.

In the next part, the author submits the communications hitherto made on the free termination of tactile nerves to a critical examination. In succession he enumerates all the results which have been obtained by the aid of the reduction of chloride of gold; he lays open to doubt the possibility of demonstrating the epithelial terminations of nerves on the recent cornea, and he concludes from this that the use of the gold methods does not lead to results worthy of reliance. This is the reason why the author rejects as problematical the observations hitherto made on the free terminations of the nerves of touch.

He arrives at the same results with regard to the closed terminal nets. He states that though reticular plexuses are formed by the division of a nerve trunk into fibrils, and the continual crossing of these, yet no true network is formed where the ultimate fibrils reunite, forming closed loops.

In conclusion, he dwells on some terminations of nerves, the nature of which as nerves of touch is yet undetermined, viz.: 1st, the Pacinian corpuscles in man and the other mammals for which he refers to the publications of Axel Key and Retzius, which were published in this Archive, Vol. IV., No. 25; 2d, the *organs of the sixth sense* of Leydig. (*Ueber Organe eines sechsten Sinnes. Nov. Act. Acad. Cæs. Leop.* XXXIV., 1868, and *Archiv. f. mikr. Anat.* XII., 1875).

These organs are not, according to the author, organs of a special sensation, as Leydig will have it, but they are partly terminations of the gustatory nerves and partly of the nerves of touch. As to the lateral organs of fishes and tadpoles, he is yet in doubt to which of the two above mentioned categories they should belong; but he feels assured that there is not the slightest reason of probability for admitting them as organs of a sixth sense.

THE FUNCTIONS OF THE LIVER.—Many of our readers have doubtless perused the rather remarkable paper by Dr. Lautenbach, published not long since in the *Phil. Med. Times*, on certain new functions of the liver.

He reports the experiments as having been performed in the laboratory of Professor Schiff, at Geneva. The experiments were exceedingly suggestive, and their publication marked their author as one of the most promising and active of our younger physiologists. But it has been with some surprise, to say the least, that we find in No. 36 (1877) of the *Centralblatt f. d. med. Wissenschaften*, the following note, in italics, from Prof. Schiff:

" In No. 32 of the *Centralblatt*, there is reported a series of experiments on ʻThe Functions of the Liver,ʼ with the statement that these experiments were, for the most part, performed in my laboratory. The greater part of these experiments, more than four hundred in number, were not only performed in my laboratory, but were performed by myself, and in fact, for the completion of a work previously undertaken, and I have already communicated an outline of them to the Societé de Physique, and printed it in the March number of the *Archives*. Herr Dr. Lautenbach, of Philadelphia, who was then engaged on other work in my laboratory, made notes on a part of my experiments, as other visiting students and not merely those actually working here were permitted to do. When I had finished the series on nicotine, Herr L. recommended that I should repeat my experiments upon hyoscyamine; and I told him to take them in hand himself, so far as they were performed on frogs, and to follow out thoroughly the method I had used with the nicotine. This, as I have stated in the *Archives*, was the only active participation of Herr Lautenbach in my researches. Later, I permitted him, at his request, to publish in English an outline of these experiments, together with my verbal statements as to similar researches undertaken before his coming to Geneva. I allowed him to use for this purpose a series of notes; but was in the highest degree astonished to see my whole investigation, garnished with some plainly mistaken statements of Lautenbach's (for example, the statements in regard to the portal system of frogs), and with some independent investigations with coniin and cobra poison added, appear under *his* name and as the product of *his* labor. Herr L. has, in previous numbers of Philadelphia medical journals, converted other practical laboratory demonstrations to his own use. If such things are allowed to go unresented, practical laboratory demonstrations and independent investigations by students are threatened with serious peril."

The only comment we feel inclined to make on this case, is, that we hope Dr. Lautenbach may be able to explain the allusions, contained in Dr. Schiff's note, in some satisfactory manner.

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The following are among the articles lately published on the Anatomy and Physiology of the Nervous System.

ROSENBACH, On the Physiology of the Vagus, *Centralblatt*, No. 6; BENEDIKT, The Occipital Lobes in Mammals, *Ibid*, No. 10; TSCHIRIEW, Irritability of Nerves and Muscles, *Ibid*, No. 21; PANSCH, Some Points in Regard to the Cerebral Convolutions, *Ibid*, No. 36; BULGAK, On the Contraction and Innervation of the Spleen, *Virchow's Archiv.*, LXIX., ii.; PFLUEGER, Remarks on the Physiology of the Central Nervous System,

Pflugers Archiv., Bd. xv., iv. and v. Hft.; LANGENDORFF, On Reflex Inhibition, *Archiv. f. Anat. u. Phys*, Phys. Abth., i. and ii., 1877; KLUG, Physiology of the Space-Sense of the Upper Extremity, *Ibid*, ii.; HERMANN, Researches on the Development of the Muscle-Current, *Pfluegers Archiv.*, xv., iv. and v.; BERNSTEIN, On Fatigue and Recovery of Nerves, *Ibid*, xv., vi. and vii.

b.—PATHOLOGY OF THE NERVOUS SYSTEM AND MIND, AND PATHOLOGICAL ANATOMY.

NERVOUS SYMPTOMS WITH EAR DISEASE.—Dr. Hughlings Jackson, *Lancet*, (Am. Rep'r.) June, considers the following nervous affections occurring in connection with ear disease; neuralgic pain, Bell's paralysis, tumor of brain, abscess and meningitis, hemiplegia, epilepsy and epileptiform seizures, and aural vertigo. Some of these, as he says, can only be considered as associated with aural trouble, their relations are not otherwise clear.

Neuralgic pain, he thought, was probably only symptomatic of exacerbation of the tympanic disease, and, *as a rule,* did not precede meningitis or abscess, but sometimes preceding paralysis of the face, it may lead to a false diagnosis of "rheumatic" paralysis of the portio dura nerve.

The following remarks were made on unilateral facial paralysis: (*a*) He had never met with paralysis of the palate in uncomplicated facial palsy. An obliquity of the uvula indicated nothing, since it might occur in healthy individuals. If paralysis of the palate was met with he should suspect not a dependence upon ear disease, but intracranial trouble. There would be two lesions, and if the symptoms came on slowly he should suspect syphilis. (*b*) Uncomplicated facial paralysis with ear disease is not an intracranial or cerebral, and hardly an aural symptom; it is a bone symptom. (*c*) It is occasionally the precursor of serious intracranial trouble, but not often, and their relations are only those of coincidence. (*d*) It is erroneous to infer that because the causal lesion of the palsy is a gross one, that recovery will necessarily not occur. (*e*) The facial palsy does not even indicate serious extension of the ear disease, but only extension in an unfortunate direction. The occurrence of intense pain in the head would be a much more serious symptom.

In some cases of chronic ear disease he had found a mass of tubercle in the cerebrum or cerebellum in the place, so to speak of an abscess from aural disease. Except, perhaps, in chronicity, there are no distinguishing symptoms between the two.

Meningitis and abscess were mentioned together on account of the difficulty in diagnosing one from the other. Abscess was much the most frequent, however, and therefore the safer to predict. Perhaps the very early appearance of optic neuritis, if well marked, would point to abscess. An abscess, as is well known, may give rise to no symptoms, and when then they do come on the case is usually acute, and may simulate meningitis. Some cases of tumor have very similar symptoms, the pres-

ence of ear disease is then useful, but not conclusive, since tubercle may occur from aural trouble. Moreover, the symptoms of brain abscess are, generally, only severe headache and vomiting, which may lead to the mistake of diagnosing stomach or liver derangement. The intensity of the headache, its frequent unusual locality, and the purposelessness of the vomiting, (often with clean tongue) should negative this diagnosis. If the opthalmoscope were used in all cases of severe headache, some diagnostic mistakes might be avoided. There is often optic neuritis, but as this is often unattended with impaired vision, it is not often looked for. Delirium is not a good evidence of acute brain affections from ear disease. A patient may recover for a time from cerebral abscess, and it is clinically worth remembering that a patient with abscess from ear disease may, without being apparently near death, die very suddenly.

Hemiplegia occurs sometimes in children with ear disease, and though the post mortem evidence is wanting, the possibility of venous thrombosis leading to local softening is suggestive.

In the cases in which epilepsy or epileptiform attacks occur with aural trouble, the author does not accept the explanation that the attacks are due to reflex irritation, but suggested that the aural disease has led to trouble in Hitzig's or Ferrier's regions. He thought it possible that the disease is sometimes softening from venous thrombosis, the convulsions depending upon an instability of gray matter around the softened regions, and believed he had some indirect evidence of this.

As regards aural vertigo, or Meniére's disease, the author thought that any kind of ear disease would cause vertigo, and reeling with faintness and vomiting, and it was his opinion that the paroxysm always depends on trouble in the auditory expansion in the labyrinth, whether from actual lesion therein or trouble from without. There are also chronic cases as well as paroxysmal ones, thus there are cases of ear disease in which giddiness appears whenever the patient walks. The paroxysms are usually explained by the laity, and sometimes by medical men, as being due to stomach or liver trouble. The author urged that the mere concurrence with deafness with the paroxysmal or chronic symptoms should not alone settle the diagnosis. He passed in review the most characteristic symptoms of five great varieties of vertigo: (1), stomachic; (2), nervous (often sexual) exhaustion; (3), ocular; (4), epileptic; (5), aural or auditory. Great attention should be paid for diagnosis, to what Knapp has called the limitations of the "field of audition," to the loss of perception of certain notes; if the loss were of some *intermediate* notes it would, the author thinks, point to disease in the cochlea. He tried to show how we should distinguish betwixt a case of chronic unsteadiness from aural disease, that from cerebellar disease, and that from ocular palsy. Finally, he gave his speculations as to the *modus operandi* of the ear disease. There were two sets of symptoms: *a*, "vital," (faintness, perspiration, irregularity of pulse, etc.); *b*, locomotor (vertigo, with or without reeling). He attributed the former to disturbance of, or actual disease in, the cochlear division; the latter to disease or disturbance of the semi-circular canal divisions; the former division was, he suggested, chiefly afferent to the medulla oblongata, the auditory nucleus having (Lockhart Clarke) close connection with the vagal and spinal accessory nuclei; the latter division,

he thought, represented the part going, according to Lockhart Clarke, to the cerebellum. The "vital" and locomotor symptoms were due to disturbance of the medulla and cerebellum respectively. He referred to Goltz's theories; to the experiments by Vulpian and others, on the semicircular canals. He mentioned that Pierret had recently spoken doubt. fully as to the existence of relations betwixt the auditory nerve and the cerebellum. Cyon has recently found that irritation of each of the semicircular canals is followed by a particular ocular movement; a very significant thing towards the interpretation of auditory vertigo.

CHOREA.—A paper read before the British Medical Association Aug. 9 by Dr. Gowers, is thus reported in the *Brit. Med. Journal*, Aug. 25. The object of the paper was to illustrate by cases some points in the common or occasional clinical history of chorea which have hitherto received little attention. 1. *An increase in the irritability to electric stimulation,* to both faradic nad voltaic stimulation. The change can be observed best in cases of hemichorea. Five cases were narrated in which it was observed, and in some of these it was proved to disappear on the subsidence of the chorea. The same fact has been noted by Benedikt and Rosenthal, who have also observed a change in the nerve reaction to voltaism, which Dr. Gowers partially confirms. The significance of the increased irritability is probably an altered state of nutrition of the fibres of the nerves and of the motor nerve-cells of the spinal cord, under an irritation transmitted downwards from the diseased cerebral region, and is another illustration of altered nutrition in functional disturbance. 2. *The relation of the muscular dis. turbance of chorea to voluntary movement.* The inco-ordination of voluntary movement varies independently of the spontaneous spasmodic movements. Instances were mentioned in which the spontaneous movements were slight and the inco-ordination great, in which there was much spontaneous movement and little or no inco-ordination, and in which the relative proportion of the two varied at different periods of the same attack. Reasons were given for regarding the inco-ordination as something distinct from mere inability of the will to still the spontaneous spasm. As far as the observations went, they suggested the conclusion that the spontaneous spasm was in excess late in a case and during relapses. The independent variation of the two elements suggests that they depend on a morbid condition of distinct and perhaps separate regions. 3. *The relation of chorea to other convulsive affections.* If chorea depends on an unstable condition of gray matter, it is not surprising to find it occasionally associated with other diseases which we ascribe to a similar instability, such as hystero epileptic and epileptoid convulsions of various kinds, and even true epilepsy. In illustration of this, a series of cases were narrated exhibiting, 1. Chorea, with remarkable choreo-epileptic seizures during the height of the affection; 2. General chorea succeeding unilateral convulsions of six months duration; 3. Chorea, succeeded immediately by persistent unilateral convulsions, and hystero-epileptic fits; 4. Chorea, more severe on one side, succeeded immediately by convulsions on that side; 5. Chorea, followed after an interval of some years by epileptoid; 6. Chorea, succeeded after many years by true epilepsy.

THE MOVEMENTS OF THE EYES IN COMA.—Mr. Chas. Mercier, assistant demonstrator of anatomy at the London Hospital, calls attention to the independent movements of the eyes in coma, as follows, in the *British Medical Journal*, Mar. 10.

In every case of loss of consciousness, in which the coma has reached a certain depth, or, in other words, at a certain stage in the onset of coma, the eyes lose their normal correspondence and move independently of each other. Usually the optic axes diverge; but divergence is by no means essential, and when it exists is only temporary. One eye may be at rest while the other is in motion, or both may be moving in different directions and at different rates; the positions assumed being often most striking and unnatural, but quite inconstant and uncertain. The movements are never "spasmodic," that is, abrupt, but always slow rolling, gliding motions. They have no relation whatever to the cause of the loss of consciousness, but only to its degree. I have seen them in coma from injury to the head, from cerebral hemorrhage, from alcoholism, from uraemia, from "simple apoplexy" after an epileptic fit, and in profound anæsthesia from chloroform and ether. I have never failed to find them in any case of deep coma that I have examined.

The degree of coma with which this appearance corresponds is most readily observed in the artificial production of loss of consciousness by anæsthetics. After the first purely voluntary struggling, comes usually a period of quiescence and delirium, and then a second struggle from which the patient gradually passes into "surgical anæsthesia," in which the conjunctiva is insensitive. It is usually just at this point, at the end of the second struggle, and just before the conjunctiva ceases to be sensitive, that the independent movements of the eyes begin, and the return of the optic axes to parallelism is a certain indication that anæsthesia is passing away.

So invariable is this sign, that I have been in the habit, when administering chloroform and ether, of taking these movements as the indication of the degree of anæsthesia, instead of the rather barbarous method of rubbing off the corneal epithelium with the finger.

The independent movements have also, I believe, a great diagnostic value. Since it is clearly quite impossible for any one to simulate them by an effort of will, their existence forms a ready and certain means of excluding malingering and hysteria from the diagnosis of a case. The movements are in no degree an active process; but are simply dependent on loss of control, and what Dr. Hughling Jackson calls "reduction in automaticity." In this connection, it is interesting to note that, in some of the lower animals (*e. g.* the chameleon), the eyes normally move independently.

The terms coma and loss of consciousness are used here in a wide sense, to include not only the state of profound stupor, with stertorous breathing, etc., but also, all the stages which precede this graver degree.

Immediately following the above is another short communication from Dr. Francis Warner, medical registrar to the same hospital, on the same subject as follows:

The movements of the eyes have been studied with much care, and present a field for clinical observation. I wish now to direct attention to the loss of association in the movements of the eyes in patients deeply under the influence of chloroform, and in some cases of disease, and congenital defects of the brain. In the healthy subject, the eyes when moving in the horizontal plane are strictly parallel; when they are directed upward they diverge slightly but symmetrically; similarly when they are directed downward they converge symmetrically. In looking at near objects they also converge. Now in each of these physiological departures from paralellism, the inclination of the axis of the eye towards or from the median plane, is equal on the two sides, and those associated movements are supposed to be due to certain "brain centres," which govern such associated movements.

1. Preserving parallelism when they move horizontally.
2. Governing convergence when accommodating for near vision.
3. Governing divergence or convergence as the individual looks at an object above or below the horizontal plane.

When a patient is being placed under chloroform, the eyes first roll upwards as in sleep, and, in so doing, they diverge symmetrically as when they roll up from any other cause. When the patient is fully comatose, and before he begins to breathe stertorously, there is *a loss of association* in the movements of the two eyes. At this stage of temporary brain paralysis, the eyes re usually directed in the horizontal p ane (or what would be the horizontal plane w re the patient in the vertical position). In this condition, *one eye may remain perfectly quiescent, while the other slowly wanders* either inwards or outwards; the same thin may then recur, or the other eye may remain at rest, or t e two eyes may both move, but without any association of m vem nts except th t they both, (enerally at leas) keep at the same level. I h ve myself never seen the same phenomena in the coma produced by eth r; presumably because with ther surgical coma may be brought about by a less degr e of brain paralysis than in the use of chloroform. This is an argument in favor of the greater safety of ether as an anæs hetic.

The following case came under my care at the Children's Hospital, Birmingham. A girl, three years of age, was the subject of permanent paralysis of one arm and leg following convulsions in infancy. She h d never been able to speak, was constantly dribbling and idiotic in manner. On observation there was seen to be an occasional loss of paral elism of the eyes: one would remain at rest, wh le the other wandered inwards or outwards; this was a chronic condition in the child, who was suffering from no acute disease.

I have seen a few similar instances, one in conjunction with two other physicians, who confirmed my observatio s. In a few insta ces, I have seen the same thing in acute cerebr l mening.tis. In considering the physiological significance of this condition. I think the cases quoted indicate that it is due to paralysis of certain portions of the bra n, and that it is not due to any spasmodic action of the recti muscles. I it be granted that a brain centre governs the horizontal parallel movements

of the eyes, this centre mu-t be paralyzed in the condition referred to. Chloroform admi istered to the stage of complete surgical anæsthesia, and just short of producing stertorous breathing, paralyzes this centre; ether may produce surgical anæsthesia without paralyzing it. These observations are incomplete, and the deductions crude; but I have thought the subject worthy of notice, and of clinical interest.

Nervous Affections of the Skin—Hebra u. Kaposi, Lehrb. d. Haut krankh., 2 B., 3 Lief. Nervenkrankheiten der Haut. *Deutsche Med. Wochensch.*, No. 9. According to the authors definition only those pathological conditions of the skin are to be considered as neuroses, " which appear as the expression of their abnormal or disordered innervation, without the possibility of finding in the tissues of the skin itself, together with its nerves, and with the present methods of investigation, any demonstrable pathological alteration." We have, therefore, to exclude a series of processes, that certainly very often stand in connection with certain nervous conditions, but which apparently manifest themselves in definite alterations of the cutaneous tissues, such, for example, as herpes zoster, various pigmentations, psoriasis, etc., and also the central or peripheral nerve lesions connected with anæsthesia and algesia. The authors also exclude the vaso-motor disturbances, the angio- or trophoneuroses of Eulenberg and Landois, from the cutaneous neuroses, since the vaso-motor nerves are more or less implicated in all skin affections, and it is very far from certain whether they are the primary disorders.

There remain, therefore, since the single motility neurosis, the cutis anserina, is to be considered as a physiological phenomenon, only very few skin affections affecting the sphere of sensibility, to be considered. Together with hyperæsthesia and hyperalgesia, anæsthesia and analgesia, which almost exclusively depend on a hysterical condition, we have, as a genuine neurosis of interest to the dermatologist occurring without external irritation, a condition of skin alteration known as pruritus.

" Pruritus is a chronic skin disease which makes itself known alone for months and years by a sensation of itching, while it reveals in the skin no efflorescence or other alteration except such as result from the scratching caused by the itching," and it is distinguished from prurigo, which, according to most authors is not considered as a nervous disorder but rather an exudation process, in which the itching depends upon a nodular eruption and ceases when the eruption goes away.

As etiological moments of pruritus universalis are given, senile marasmus, chronic gastric disorder, the climateric period, menstrual troubles, Bright's disease, diabetes mellitus, tuberculosis, carcinoma of the intestines, icterus, and finally, depressing psychic affections; the prognosis is hence not absolutely unfavorable in all cases. Pruritus senilis is incurable.

Pruritus localis affects almost exclusively the genitals and the anus;

it is rarely met with in the palm of the hand, the sole of the foot, the tongue, and the urethra. Disorders of the sexual organs, uterine cancer, helminthiasis, and hemorrhoid are the usual causes.

The itching of the skin coming on in winter time (pruritus hyemalis) is, according to the author, due to the chilling of the cutis by the cold dry atmosphere, and is therefore, as the result of an external irritation not to be reckoned as a neurosis.

It is evident that the first therapeutic aim must be to attack the cause of the disease, but in all cases the patients first desire the removal of the unendurable itching. Tar is absolutely worthless, on the other hand such agents are to be recommended as will rapidly produce a local cutaneous anæsthesia. Cold douches, washing with acidulated water, alcohol, æther, carbolic acid with subsequent powdering the surface to delay the evaporation of these fluids. The author recommends the following formula :

<blockquote>

R̅

Acid Carbol.,	-	-	-	-	-	1.5 grammes
Sp. Vini Gallici,		-		-	-	300 "
Glycerine,	-	-	-	-	-	5 "

or

Ætheris Petrolei,	-	-	-	-	-	4 grammes
Spirit.,	-	-	-	-	-	-	180 "
Glycerine,	-	-	-	-	-	-	4 "

</blockquote>

Sometimes warm baths, with or without soda or sublimate are of advantage. In pruritus of the genitals, astringents and likewise applications are of benefit. Of internal medicines, arsenic has not proved useful, but on the contrary carbolic acid (0.1 per dose 0.6 or 0.8 per diem) has been of use in some cases.

NEURALGIC HERPES OF THE GENITALS.—During the past year, we have given in the Periscope the title of a series of lectures delivered and published in Paris by M. Mauriac, which have since been brought in a separate *brochure*. We extract the following notice of this work from *La France Médicale*.

This memoir of some 112 pages is the reproduction of the clinical lectures delivered by the author at the Hospital de Midi. M Mauriac, if he makes no pretenses to its discovery, yet wishes to call attention to a pathological subject, still little studied in spite of its frequency. Neuralgic herpes has been almost neglected up to the present time, and that it has been observed only in males, though he does not nevertheless deny its existence in the female. Neuralgic herpes produces perversions of sensibility, hyperæsthesia, analgesia, anæsthesia, etc. The nervous perturbations precede, accompany or follow the appearance of herpes on the genitals, which, moreover, presents in a physiologico-pathological point of view many relations with zona.

The etiological moments of neuralgic herpes of the genitals are most frequently constitutional, they are first to be found in arthritism, herpetism follows next. There may be also, but remotely connected, scrofula, syphilis, diabetes, and nervosism.

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After giving the pathogenetic theory of herpes zoster, M. Mauriac comes to the following conclusions, that it is most probable that neuralgic herpes of the genital organs is connected with a hyperæmic process that invades a greater or lesser extent of the sacral plexus. This irritation may depend upon a general state, a constitutional disposition, on which it is impossible to pronounce with certainty.

After general considerations on the symptoms, the processes, the diagnosis, the duration and the sequelæ of the disorder, M. Mauriac comes to the treatment, which consists in very slight cauterizations, followed by soothing ointments, containing calomel, baths, refreshing tisanes; mineral waters are indispensable, and the most serviceable are the chlorinated sodic and sulphurated waters, such as those of Uriage, etc.

THE ARTHROPATHIES IN LOCOMOTOR ATAXIA.—The following is the notice of a recent memoir on this subject by Dr. Joseph Michel, (Masson, Paris, 1877), in *La France Médicale.*, No. 39, May 16:

The study of these arthropathies is of recent date, it is even astonishing that this complication of ataxia, so remarkable by its progress, its symptoms and the lesions connected with it should have passed unobserved up to the year 1831, when it was noticed, incompletely it is true, by an American physician, J. K. Mitchell. In 1857, Alison took up the subject, but we have to come to Professors B. Ball and Charcot to find an accurate description of this complication, and the proofs of its relations to locomotor ataxy. M. Joseph Michel has collected twenty-three cases, scattered through medical literature or previously unpublished, and after having given an exact description of the appearance and forms of this arthropathy, he attempts to deduce from the study of pathological anatomy and physiology, the exact locality of the nerve lesion which is the primary cause of these articular troubles, and, without attaining any precise result, he shows, nevertheless, that the atrophy of the cells of anterior cornua is far from being constant, as was held by M. Charcot.

In fact, out of seven cases of which an autopsy was made, with histological examination of the cord, in two was atrophy of the anterior cornua found at the points from which arose the nerves going to the diseased joint; in two others there was a dubious and ill defined alteration of the gray substance, so indefinite indeed as to leave some doubt as to whether it really existed; and finally, in the three remaining cases, pathological anatomy was altogether silent and gave no information. " Therefore, in a certain number of cases," says M. Michel, " we observe the coincidence of the arthropathy and a lesion of the large gray cells of the cord, and, on the other hand, there exist cases not less numerous, in which is the arthropathy without our being able to prove the existence of the lesion of the cells." To-day the question is still in the same condition as in 1875, when M. Blum, in his *Thèse d'agregation* wrote as follows: " To establish a connection of cause and effect between the ataxic arthropathy and the lesion of the cells of the anterior horns is an ingenious way of viewing the subject, but one, that in the actual state of the question, cannot be ad-

mitted except with the most extensive reservations." Two observations published since then (Raymond and Bonneret) having both furnished negative results as to the lesions of the anterior horns, allow us to be more affirmative in the doubt than was M. Blum.

We cannot analyze all the chapters of this memoir; we will mention, however, the part that relates to the differential diagnosis of the ataxic arthropathy from dry arthritis, also the chapter on the pathological anatomy. The following are the conclusions of the author, who has been able to give only an hypothesis of the origin of the lesion, which, being based upon physiology, seem to us to approach very near to the truth :

"The arthropathies occurring in the course of locomotor ataxia are actually under dependence to the nervous system. It is, perhaps, the vaso-motor paralysis that is the predisposing cause, while the direct cause is a slight traumatism. Again they may arise from nervous irritation and exaggeration of trophic action; or the nervous system may manifest its action in a reflex way."

THE CAUSES AND PRODROMAL SYMPTOMS OF PSYCHIC AFFECTIONS.— Hecker, *Volkmann, Samml. Klin. Vortraege.* No 108. (Abstract in *St. Petersburger Med. Wochenschr.*) The author addresses himself to physicians not specialists in mental diseas s, to introduce to them their first symptoms and their treatment. Altogether, heredity is proven in about one-fourth of all the cases, and it is to be first of all considered. The first of the eight forms described he designates shortly as hereditary insanity. The hereditary element is shown in asymmetry of the skull, ill-formed ears, defective development of the genitals or of the whole body. With these are observed convulsions or chorea in youth, later disagreeable variable temper, irritability, lack of self-control, over-conceit, and with these often striking ability in certain directions, and talent. Here the physician must look to the bringing up. In the further development we do not meet with simple delusions, partly excepting the ideas of persecu tion, but biased childish judgments, which, with the lack of will power, are readily translated into acts; these acts are generally in violation of morals, and the patients know how to use sophistry to excuse them, hence the narrower designations of this form of disease as moral insanity, *folie raissonante*, or mania sine delirio (typica, Kahlbaum). These cases on the whole, afford but little prospect of a perfect cure, their course is variable and gradual. With the hereditary element which is also present in other forms of insanity, the following causes form one group, tumors of the brain, hemorrhages, meningitis, syphilis, cardiac deficiency, uterine disease, stasis in the portal circulation, typhus, cholera, pneumonia, intermittent fever, and injuries of the head. The last named often develop their action years after their reception, by insidious inflammatory processes, or if cutaneous nerves are compressed by external scars, through the reflex irritation. All the above enumerated causes lead under certain circumstances to another well characterized form that is shown by head-che, violent disposition and inability of the brain to withstand external-

influences, those of spirituous drinks, for example. The mental power is weak, and the violence of the disposition manifests itself in outbreaks of temper and temporary insanity, which speedily pass over and give way to comparative good feeling. Epileptic attacks occur during the course of the disease. Of the outcome it is difficult to say anything special.

As a third form we have the well-known progressive general paralysis. In the beginning there are only slight fibrillary contractions and disturbance of speech, then emotional exaltation, perverted occupation, wasteful use of money, etc., then fits of madness and most extravagant ideas of personal importance, and finally complete dementia and general paresis and death after three or four years. As a fourth form of the disease we may mention the Katatony described by Kahlbaum. It likewise commences with convulsive phenomena like the preceding forms, in about half the cases, especially tonic in their character. It succeeds in a longer or shorter time a melancholic preliminary stage, distinguished by a sort of inflexibility of the entire disposition, and leads to melancholic attonita and flexibilitas cerea. Very frequently there is absolute taciturnity (*mutacismus*) or only the repetition of a few and the same phases, with this we notice the inclination to attach mysterious significations to the words and acts of others; finally there is added a still more pronouncedly negative affection, refusal of food, retention of urine, etc. This condition may pass either temporarily or permanently into an excited state with very decided hallucinations, and then either to cure or comparative health, or final dementia.

To the remarks on katatony, the author adds some general considerations upon melancholia. A melancholic stage, in its broader sense, occurs in the course of various psychoses, but by itself it affords us one phase of insanity which runs its course throughout only as a depressed condition of mind, connected also with pre-cardiac pain, and also with agitation, delusions and hallucinations (ideas of persecution and poisoning, etc.)

This form of disease passes into either recovery or some moderate degree of mental weakness. On account of the frequent attempts at suicide, isolation in an asylum is every way indicated, and rest, improvement of the nutrition, and of medicine, opiates, are the measures to be advised. The designation melancholia should not be retained, because, as said before, melancholic derangement is more frequently only a transition stage of various other psychoses or forms of disease. Kahlbaum has recommended that the genuine melancholia should receive the name dysthymia, in order that we might not give the same designation to different things. As a sixth form of mental disease, the author gives that one called hebephrenia, by Kahlbaum. Beginning almost always between the eighteenth and twenty second year of life with a melancholic stage, the insane basis of which is recognized by the exaggerated complaints, and the simultaneous indifferent emotional condition and laughing countenance, there soon replaces the melancholia a silly excitability.

Seventhly, the author gives the description of mania. This is only a *Symptomcomplex*, a transition stage, and not a form of insanity by itself, and it exhibits like melancholia special characters in different varieties of

the disease. In general the expression is characterized by exaltation, lively disposition and excitation of the conceptive activity to the point of complete loss of control and confusion of ideas. This stage of mania is present to a greater or less degree in all the above described varieties of insanity with the exception of the true melancholia (dysthymia of Kahlbaum). As a seventh species he recognizes that kind of mania periodically alternating with a melancholic stage (*folie circulaire*, cyclothymia of Kahlbaum) and which is lasting and incurable. In the exaltation stage the patients exhibit an apparent increase of activity; they lie, intrigue, and deceive, bluster and destroy with apparently perfect possession of their faculties and are therefore often not considered as at all insane. In women the nymphomaniac condition is common. Then, after awhile, the melancholic stage comes one, sometimes separated from that of mania by a period of apparent health, and so the non-medical observer considers the trouble, as a rule, to be only a very proper *Katzenjammer.* The treatment as a whole must be calmative and isolation in an asylum is essential. In conclusion the author describes an eighth variety of insanity. In this both the maniacal and melancholic phases are absent; the morbid process affects only the reasoning and understanding faculties and does not pass over into dementia. Fixed delusions are the characteristic. It is a genuine craziness (*Verrucktheit paranoia* of Kahlbaum). The delusions are constantly becoming more and more fixed and render the patient absolutely impracticable, while the mental faculties generally are only somewhat weakened.

THE ETIOLOGY OF HERPES LABIALIS AND OF ZONA.—The following are the conclusions of a recent memoir by Dr. Jorissene, of Liege, (*Ann. de la Soc. Méd. Chirurg. de Liege*, 1877,) as reported in the *Bull. Gén. de Thérapeutique*, July 30.

1. "Lactation is a predisposing cause of endocarditis and of herpes facialis, perhaps also of zona:

2. "*Herpes labialis* or *facialis* is a morbid process, pyretic or apyretic, sporadic or epidemic. transmissible (?) under certain circumstances (?) which develops itself exteriorly on the skin, and internally on the lungs, the pleura, occasionally on the stomach, the meninges, or the endocardium.

3. "Herpes zoster or zona is an entirely distinct affection from *herpes facialis.*

4. "Rheumatism is a predisposing cause of zona. Possibly we ought to include in our nosological classification, arthritic zona.

5. "In ophthalmic zona the neuritis precedes the cutaneous eruption, and may cause ocular troubles before the appearance of the exanthem.

6. "Zona is generally unilateral. Zona opthalmicus in the bilateral form has been observed only twice; in the case reported by Lailler when it was complete, and in the one reported by myself, in which it was arrested in its progress."

The following are the titles of some recent articles on the Pathology of the Nervous System and Mind, and on Pathological Anatomy.

Beard, The Nature and Treatment of Neurasthenia (Nervous Exhaustion), Hysteria, Spinal Irritation and Allied Neuroses, *N. Y. Med. Record*, Sept. 15; Raggi, On the Genesis of the Epileptiform or Apoplectiform Attacks in the Insane. *Rivista Clinica di Bologna*, July, 1877; Krafft-Ebing, Insanity in the Climacteric Period, *Allg. Zeitschr. f. Psychiatric*, XXXIV. iv. ; Björnström, Athetosis, *Upsala Lakareforenings Forhandlingar*, XII. vi.; Deecke, The Structure of the Nervous Centres in Health, and their Changes in Disease, *Am. Jour. of Insanity*, July; Kiernan, Katatonia, a Clinical Form of Insanity, *Ibid;* Catlett, The Frequent Association of Diseases of the Ear with Insanity, *Ibid;* Billod, Contribution to the Study of Aphasia, *Ann. Med. Psychologiques*, May; Taguet, Suicide in Hysteria, *Ibid.*

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c.—THERAPEUTICS OF THE NERVOUS SYSTEM AND MIND.

Sophoria.—This is the name given by Dr. H. C. Wood, *Phil. Med. Times*, Aug. 4, to an organic extract or alkaloid obtained from the seeds of Sophora speciosa, a Texan plant sometimes used by the Indians of Southwestern Texas as an intoxicant.

The extract, as obtained, was of a yellowish white color, amorphous, soluble in acidulated water, and very freely in ether, but not in pure water. It was precipitated by alkalies.

The results of physiological experiments with an alcoholic extract of the bean, were as follows:

In *frogs* it produces a rapid loss of reflex activity and power of voluntary movement. The loss of power is not due to any action upon the motor nerve-trunks, as after death these were found to preserve their normal susceptibility. Further, tying the sciatic artery upon one or both sides of the frog did not influence the action of the drug upon either voluntary or reflex movements. This would indicate that the poison is a spinal sedative, and has little or no effect upon either motor or sensory nerves. In all cases the heart continued beating long after the cessation of respiration.

Upon mammals the effect varies somewhat in accordance with the dose. An amount of the extract estimated at two grains (?) produced in a fullgrown tom-cat, in one minute, marked weakness in hind legs, in two minutes, inability to stand, with evident effect upon the respiration ; in three minutes, convulsive movements, with loss of consciousness, continuing with ever increasing embarrassment of the breathing for three

minutes, when all attempts at respiration ceased. The heart kept on beating for one and a half minutes longer. The pupils were unaffected at first, afterwards dilated.

In small quantity the extract produces in the cat vomiting, great muscular weakness, profound quietude, and deep sleep lasting some hours and ending in recovery. In dogs the symptoms were similar to those noted in cats. Death always took place through the respiration. In a single cardiac experiment the drug had no decided effect upon the blood-pressure until towards death, but appeared to accelerate the cardiac beat.

The alkaloid is very active as a poison, a very minute quantity producing almost complete paralysis in a frog within two minutes. One-twentieth of a grain of a very impure specimen produced in a half-grown cat, deep sleep lasting many hours.

BROMIDE OF POTASH IN NEUROSES OF THE HEART.—Dr. Joseph Angrisani, *Rivista Clinica di Bologna*, Mar, 1877. Conclusions.

1. Bromide of potassium has a depressant action on the vaso-motor centres and the cardiac plexus.

2. This effect is produced on the vaso-motor centres by a special mode of action, and not because the bromide exerts an action on the smooth fibres of the capillary vessels. The narrowing of the lumen of the capillaries may rather depend upon the release of the action that the bromide produces on these centres, and vaso-motor nerves in physiological experiments.

3. Bromide of potassium has no action on the muscular fibres of the heart, like digitalis, nor has it any action on the arteries.

4. The most useful and effective therapeutic action of the bromide is in correcting functional anomalies of the heart, such as frequency or intermittence of pulse, arhythmy, etc., whatever may be the condition of the myocardium.

5. It relieves angina pectoris and palpitation quickly and safely when they are pure and simple neuroses. Those cases that depend upon serious anatomo-pathological alterations of the heart, are considerably modified by the bromide, and also sometimes completely cured for a longer or shorter period.

6. The bromide in the cases of the said neuroses is advisably followed always by treatment directed to a radical correction of the cause of their production.

THE ANTAGONISM OF ACONITE AND DIGITALIS.—Dr. J. Milner Fothergill publishes in the *British Medical Journal*, Aug. 4, his first and second reports on the antagonism of aconite and digatalis. The first report gave some conclusions as to the antagonism of aconite and digitalis in the rabbit and the Guinea-pig, as well as some more exact information as to

the minimum lethal doses of these agents. It was found that the minimum lethal dose of aconitine was about 1-400th of a grain for a rabbit of $1\frac{1}{2}$ lbs.; and 1-1200th of a grain, or less, for a Guinea-pig of the same weight.

It was considered advisable in the second series of experiments to limit the inquiry to rabbits, and they were thus instituted with the aid of Dr. Pairman.

The first series was performed on seventeen rabbits, to ascertain the minimum fatal dose of aconitine. It was found to be much less than was supposed, about 1-400th of a grain for a rabbit, weighing $1\frac{1}{2}$ lbs., and 1-300th for one of three pounds.

Experiments with digitaline seemed to indicate that the minimum fatal dose was about a grain to the pound of body weight of rabbits.

The simultaneous administration of the digitaline did little or nothing to modify the effects of aconitine, but if given a long enough time before, (from 4 to 9 hours) it afforded a decided protection. The effect of less than the lethal dose of aconitine upon a fatal dose of digitaline was to hasten the fatal event. This result was in accordance with what was found by Fraser when testing the antagonism of belladonna and Calabar bean. Atropia in moderate doses given a few minutes previous to the administration of aconitine, was found to decidedly modify its effect, and the same was found to be the case when it was subsequently given, while the reverse was found not to be the case, small doses of aconitine not modifying the effects of lethal doses of atropia. In atropia we therefore possess, according to these experiments, an antidote to aconitine, but the relation is not a reciprocal one. .

ELECTRICITY.—Dr. A. S. Myrtle read a paper at the British Medical Association in August last, (rep. in *Brit. Med. Jour.* Aug. 25) in which he stated that he had found the current from five to twenty cells of the Leclanché battery of great therapeutic value in many forms of superficial neuralgia, and especially in those cases where the pain had a decided fixity in its nature, such as migraine, intercostal neuralgia, neuralgia of certain groups of muscles, such as the pharyngeal and laryngeal, accompanied with great difficulty in swallowing and breathing; of the abdominal muscles, accompanied with such intense pain in assuming the erect posture, or in walking, as to render the patient unable to do either for even a very short period without intense suffering. For the relief of neuralgic pain in more deeply seated nerves, such as the lumbar and the sciatic, thirty or forty cells are required. In most cases, the nerves affected are far less sensitive to the faradic current than the healthy nerve is; and as far as Dr. Myrtle's observation went, it matters little how the poles are placed so long as the affected nerve is made a part of the chain through which the electric current passes. In no case ought the current to cause pain, but a mild tingling or pricking; and when the skin is tender or thin, it should not be kept applied to the same spot for more than for a very few minutes (two or three) at a time, else a crop of angry pustules

will put in an appearance. A few applications are of no use, as this agent must be employed daily for from five to fifteen minutes, and regularly, until the pain is entirely subdued. In spasmodic asthma he had met with great success from the use of the constant current. He had seen in ten minutes the respirations reduced from thirty-eight in the minute to eighteen, with complete relief to all painful symptoms. Here he had never found it necessary to make use of more than ten to fifteen cells, and had applied one pole to the par vagum in the neck, and the other over the eighth intercostal space.

NARCEIA AND CODEIA.—The works of M. Claude Bernard have demonstrated that narceia is the most sleep-producing substance in opium. Nevertheless this substance is but little used in therapeutics, and it is for the purpose of bringing to light its properties that M. Barnay has undertaken his comparative researches on the usage of this alkaloid, as well as that of codeia and morphia (*Brochure* 8 vo. 60 pp.). According to M. Barnay, narceia ought to take the first place among the alkaloids of opium; it results. in fact, from his experiments that it acts especially as a hypnotic, that it produces a slumber similar to normal sleep followed by an absolutely normal awakening, that it does not produce nausea, vertigo, or hebetude, like morphia, that it does not cause itching of the skin or convulsions, like codeia. The toxic power of narceia, inferior to that of morphia, is especially less than that of codeia. Finally, beside its hypnotic qualities, narceia possesses, according to M. Debout, a happy influence on chronic bronchitis; it diminishes the cough and modifies the expectoration. M. Behier, on his part, has seen it constantly ameliorate the general condition of consumptives, as well in diminishing the cough and the expectoration, as in arresting the diarrhea. M. Laborde has obtained the same results in phthisis, and by its employment has favorably modified the progress of whooping-cough. The only inconvenience charged to it is that in certain cases it has caused difficulty in the emission of urine. It may be administered like morphia, either hypodermically or by way of the stomach (we would remark, nevertheless, that the authors of the *Traité de Thérapeutique* of Trousseau and Pidoux recognize no advantage in the hypodermic method of administration, on account of the slight solubility of the substance). Only one difficulty in its employment appears; up to the present time the pharmacies are not generally provided with narceia, but whenever physicians decide to give it the place it merits in therapeutics, this difficulty will disappear of itself. As to codeia, its convulsivant properties are such, according to M. Barnay, that it ought to be excluded from therapeutics; one fact interesting to know is that up to a certain dose, rather high, moreover, it appears to be not dangerous, but if that amount is passed in the slightest degree, even one or two milligrammes, = .015 to .030 gr.) the convulsive accidents may appear all at once and cause death. *Journal de Med. et de Chirurgie Pratiques,* August.

ACONITINE IN CARDIAC DISEASE AND NEURALGIA.—M. Gubler says in the *Journal de Thérapeutique* : The cardiac disease was so marked in a young

woman with organic disease of the heart after a small dose of aconitine, in my *clientele*, that she prayed to have the medicine stopped. Liegeois and Hottot have already demonstrated in aconitism paresis of the heart and paralysis, from the action of the alkaloid. Under whatever form we employ it, as the amorphous aconitine, or the crystallized azoʻate of Duquesnel, it is a medicine difficult to manage, and we should use it with care.

It is better to give it in solution than in granules, as the latter are often inactive, and we are tempted to increase the number, owing to the seeming insensibility of the patient to the medicament. By using the solution, owing to its certain absorption we avoid the danger of the accumulation of the poison, and we should begin with half a milligramme, progressively increasing the dose if necessary, as some patients bear even six millegrammes. I have never seen any bad results from its employment if it is given with care and in therapeutical doses.

Its disadvantages are nothing compared with its benefits.

In facial neuralgia its practical importance is very great, and it may be looked upon almost as a specific.

In neuralgia of the fifth pair, and even in tic douloureaux I have never known it fail, and I may mention two severe cases of facial neuralgia which yielded completely to the use of the azotate in progressively increasing doses.

The alkaloid is principally recommended in the congestive form of facial neuralgia; its effects are curative when there is no nervous lesion—palliative when the lesion is established. I am of opinion that all neuroses end by giving place to nervous alterations.

Aconitine, when given in the beginning, will completely cure facial neuralgia, and in those cases where the disease is advanced it will immediately afford relief ; but unfortunately this action does not extend to other forms of neuralgia. *Med. and Surg. Reporter*, Aug. 25.

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STRYCHNIA.—Gorachowzew, *Inaug. Diss.* St. Petersburg, 1877, (abst. in *St. Petersburg Med. Wochenschr*. No. 32.)

Experiments with strychnia on dogs, rabbits, and birds, afforded the author the following results.

1. Strychnia acts, when introduced directly into the stomach or the small intestine, much more slowly than *per os* or rectum.

2. The action of this poison is at least fifteen times slower when injected into a vein of the portal system than when introduced into a vein not opening into the portal circulation.

3. This slower working of strychnia, when introduced directly into the stomach or small intestine, and when injected into the portal circulation, depends upon a property of the liver to retard its poisonous effects.

4. In rabbits and birds, contrary to the case in dogs, the poisonous effects of strychnia were manifested more quickly when it was introduced directly into the stomach, than when given by the mouth.

DITAIN.—E. Harnack, *Arch. f. exp. Path. u. Pharm.* VII. 126, 1877 (abstr. in *Deutsch. Med. Wochenschr.*).

The bark of Alstonia scholaris, a plant of the order Apocynaceae, and indigenous in Java, the Phillipines, East Australia, etc., has been much employed in the countries where it grows by physicians and natives, as a tonic, vermifuge, and antipyretic. Harnack isolated from it a glycoside basic precipitated by acetate of lead and ammonia, which he called ditain.

When injected into the dorsal lymph-sack of the frog, in the dose of five milligrammes, (=.075 gr.) the chlorate of ditain produced spinal paralysis, and (like curare), paralysis of the motor end apparatus of the nerves. The inhibitory fibres of the vagus were also involved. The action of chlorate of ditain on the heart is antagonistic to that of muscarine The inhibitory peripheral terminations of the vagus irritated by muscarine, are paralyzed by ditain. In rabbits a much larger dose is required to produce with certainty the paralysis of the terminal motor apparatus.

Ditain acts in mammals altogether similarly to curare—except in that the paralysis of the vaso-motor nerves is of a much higher grade from ditain than from curare.

PILOCARPINE.—At the session of the Soc. de Thérapeutique, July 11, (rep. in *Bull. Gen. de Thérap.*), M. Dujardin-Beaumetz reported that he had had good results from the use of pilocarpine by hypodermic injection in doses of two centigrammes; employing a solution of 1—50; but he believed this dose ought not to be exceeded; there is reason to fear that in higher doses this alkaloid may give rise to the very severe cardiac disturbances observed by MM. Gallois and Hardy in their experiments upon animals. It is advisable, therefore, not to follow the indications formulated by Adolphe Dumas in his thesis, who advised the administration of pilocarpine in the dose of five centigrammes.

With the dose of two centigrammes the perspiration and salivation are very considerable without the production of the symptoms of nausea always met with in the administration of jaborandi.

M. Constantine Paul agreed with M. Dujardin-Beaumetz, and thought that the diaphoretic effects of pilocarpine might be obtained with very small doses (a few milligrammes). When we exceed one centigramme, we have salivation and the stomachal fatigue.

BROMIDE OF CADMIUM.—At the session of the Soc. de Biologie, Aug. 4, (rep. in *Gaz. des Hopitaux*), M. Galippe reported that, on account of a case in which bromide of cadmium had been given instead of bromide of potassium, and had caused some rather serious symptoms, he had undertaken a series of experiments upon animals with the idea of determining the physiological effects of this body. A very small quantity of bromide of cadmium administered to frogs by sub-cutaneous injection very quickly

produced paralysis and death of the animals. Administered to a dog, by the stomach, bromide of cadmium caused vomiting, bloody diarrhea, and profound depression, but no other serious accidents.

INFLUENCE ON RESPIRATION OF INJECTION OF CHLORAL INTO THE HEART.—At the session of the Soc. de Biologie, July 28, (rep. in *Gaz. des Hopitaux*) M. Francois Franck gave an account of his experiments on the respiratory arrest produced in animals in which a concentrated solution of chloral had been injected into the heart.

These arrests are dependent on the contact of the irritant solution with the endocardium; they are produced, in fact, before the liquid has had time to be diffused in the general circulation, as is proven by the introduction of the substance directly into the right auricle.

They are independent of the arrest of the heart, since they are produced when we have suppressed the cardiac troubles with atropine.

The respiratory arrest follows in virtue of a reflex act, the point of departure of which is in the sensory fibres of the endocardium. These centripetal fibres are contained in the pneumogastric; the section of the depressor and sympathetic nerves, and of the cervical and cranial anastomoses demonstrates this.

M. Franck recalled the fact that respiratory arrests were produced by sudden and intense excitations of all the sensory nerves, and that particularly in the sphere of the pneumogastric, the laryngeal and pulmonary fibres are frequently the point of departure of identical reflex disturbances (exp. of P. Bert, Rosenthal, Jolyet, etc.). These sensory fibres of the endocardium ought not, therefore, to be considered as special nerves; they put the cardiac function in relation with the respiratory function, but they also form part of the great group of sensory nerves, the excitation of which can cause, in a reflex way, respiratory arrests, comparable with these in all particulars.

SALICYLIC ACID.—The following is the substance of the concluding therapeutic summary of an article by Prof. Germain Seé running through several recent numbers of *La France Médicale.*

1. As an external antizymotic medicament salicylic acid has an incontestable action, but does not surpass in this respect phenic acid, and possesses over it no advantage except that of being odorless. As an internal antiseptic it has no appreciable effect, either in purulent affections or in contagious parasitic ones, such as diphtheria or thrush, or in gangrene, or finally, in diabetes.

2. As antipyretic medicines, salicylate of soda and salicylic acid possess transitory and dubious properties, even as against the specific, miasmatic, and virulent fevers, etc., thus it has no marked superiority over the sulphate of quinine; the salicylate of quinine itself cannot yet take any definite rank in the therapeutics of the palustral fevers. Without power

in the cure of variola, the salicylate of soda has not yet given proofs of its efficacy in the treatment of typhoid; its febrifuge power is very limited.

3. It is in acute articular rheumatism that we observe the surest and promptest effects, so indeed that we can almost at once predict a cure of acute febrile or apyretic rheumatism within from two to four days; 51 cases in evidence.

4. In simple chronic rheumatism the trials I have instituted were very satisfactory; also in the acute crises which manifest themselves from time to time, either in the form of simple rheumatism, or even in that of nodular arthritis, the painful attacks cease as soon as in the case of acute rheumatism. More than this, the articular engorgements diminish to a considerable extent, and the movements may become free, even after years of pain, rigidity and immobility, on condition only that the osseous lesions be not too advanced and serious, (eleven observations of chronic rheumatism cured or ameliorated).

5. But it is in acute and chronic gout that the results are most remarkable; in my first trials I was struck with the promptness with which the most painful acute attacks were relieved, in the course of from two to three days the pain, the articular hyperæmia, the redness, the sensitiveness to touch, had all disappeared.

Chronic gout does not take less kindly to the salicylic acid treatment. By its continuous use, even in moderate doses, the patients are absolutely protected from acute attacks.

On the other hand, the chronic peri-articular engorgements readily disappear, the tofaceous deposits of the joints diminish and cease to inflame, in a word, the cure may be complete without producing any metastasis to the heart, the stomach, the respiratory organs or the brain; in no case out of twenty-one which I have been enabled to follow out, was there the least tendency to retrocession of the gout toward the internal organs.

No other inconveniences were experienced than the development of trouble of hearing and sometimes a certain degree of feebleness or of narcotism; these two last disappeared with the decrease of the dose, the disorder of audition was rather more persistent.

Among the affections, often of a gouty nature, we may mention the gravel, which is very favorably modified or rather is very easily eliminated by the aid of salicylate of soda, which has the further advantage of calming the nephritic pains. .

6. The salicylic treatment seems to advantageously modify certain facial neuralgias, but this action is not definitely established; the same is the case with the treatment of sciatica by this agent.

7. In painful affections of the spinal cord, salicylate of soda produces calmative effects, very clearly appreciable; but with the continuation of the treatment a certain degree of weakening may result.

M. Laborde, in response to this assertion of M. Seé, "The cutaneous and general sensibility is not modified under the influence of salicylic acid," showed to the Soc. de Biologie, July 28, (rep. in *Gaz. des Hopitaux*) a dog into the femoral vein of which he had injected, twenty-four hours previously, four grammes of salicylate of soda, and which presented a profound

and absolute anæsthesia of the whole cutaneous surface. In opposition to M. Seé, therefore, M. Laborde believed that he could affirm that salicylic acid possessed analgesic properties.

VERSPERNUM.—M. Bochefontaine, at the session of the Soc. de Biologie, (rep. in *Le Progres Medical*) July 21, read a communication on some physiological experiments upon the action of verspernum, a plant used as a febrifuge in Brazil. Its alkaloid paralyzes the central nervous system and destroys the excito-motor power of the cord.

A NEW THERAPEUTIC AGENT FROM ERGOT. — Podwisozki, *Voienno-Medizinski Journal,* June, 1877, (*Abstr.* in *St. Petersburger Med. Wochenschr,* Aug. 27, Sept. 8,) has been able to isolate a substance from ergot which, from its slightly acid peculiarities, he calls sclerotin acid. It is without taste and odor, and almost colorless, readily soluble in water and dilute alcohol, but is precipitated by 85 per cent. to 90 per cent. alcohol from the watery solution, unites with lime, potash, soda, manganese and silica; after maceration with hydrochloric acid and gradual addition of absolute alcohol we obtain it almost entirely free from unorganic substances.

It forms from three per cent. to four per cent. of ergot. It can be obtained in small masses, like lactucarium, or in the form of powder. Two to four centigrammes, (=.030 to .060 grain) hypodermically administered to frogs, produced complete sensory and motor paralysis, the heart's action continuing, but very weak. After six or seven days, either recovery slowly occurred, or death followed in convulsions. Prof. Holst repeatedly injected subcutaneously four to five centigrammes of this substance in cases in the gynæcological clinic at Dopat, and observed as consequences neither pain nor inflammatory processes. These peculiarities, as well as the ready solubility in water, render this substance preferable to the commonly employed ergotin. Besides sclerotin acid, another similar substance, scleromucin, was obtained from ergot, but thus far it has not been isolated from inorganic matters; and being insoluble in water it is not therapeutically applicable. Two coloring matters and an indifferent substance were also isolated.

The following are a few of the recently published articles on the Therapeutics of the Nervous System and Mind.

LIEBREICH, on the Long-Continued Use of Chloral, *Deutsche Med. Wochenschr.,* No. 27; GOWERS, On the Use of Iron in Epilepsy, *Practitioner,* Oct.; WADE, Hydrobromic Acid in Prescriptions, *Detroit Med. Journal,* Oct.; LYONS, Strychnia Incompatible with Bromide of Potassium, etc., *Ibid;* BAYLES, On the Experimental Use of Amyl Nitrite in Ten Cases of Pertussis, *Va. Med. Monthly,* Aug.; ADAMS, The Use of Bromide of Potassium Hypodermically, *Atlanta Med. and Surg. Jour.,* Sept.; BURNETT, Nitrite of Amyl in Tinnitus Aurium, *N. Y. Med. Record,* Aug. 4.

BOOKS ETC., RECEIVED.

Notes of a Visit to American Institutions for Idiots and Imbeciles: By G. E. Shuttleworth, B. A., M. D., etc., Medical Superintendent, Royal Albert Asylum, Lancaster.

Nurse and Patient, and Camp Cure: By S. Weir Mitchell, M.D. (Reprinted from *Lippincott's Magazine.*) Philadelphia: J. B. Lippincott & Co., 1877. 73 pages.

Fat and Blood, and How to Make Them. By S. Weir Mitchell, M. D. Philadelphia, 1877: J. B. Lippincott & Co.

Report on the Management of the Insane in Great Britain: By H. B. Wilbur, M. D. Albany, 1877. 74 pages.

Transactions of the Twenty-Seventh Anniversary Meeting of the Illinois State Medical Society, Held in the City of Chicago, May 15, 16 and 17, 1877. 258 pages.

Katatonia, a Clinical Form of Insanity: By Jas. G. Kiernan, M. D., of the N. Y. City Asylum for the Insane, Ward's Island. (From the *Am. Jour. of Insanity,* for July, 1877.)

Ein Beitrag zur Aetiologie der Epilepsie: Von Dr. Neftel, in New York (Seperatabdruck aus dem *Archiv. fur Psychiatrie*). 16 pages.

Sons or Daughters? Choose: By Geo. B. Starkweather. Fully Illustrated. Hartford, 1877. 411 pages.

Maternal Impressions. Report of the Obstetric Section of the Toledo Medical Association: By Thos. Waddel, M. D. 1876. 32 pages.

Pompholyx [Cheiro-Pompholyx (Hutchinson): Dysidrosis Tilbury Fox]. A Study, with Special Reference to its Nature and Pathological History: By A. R. Robinson, M. B., L. R. C. P. and S., Edin. (Reprinted from the *Archives of Dermatology*, Vol. III., No. 4, 1877.) New York: G. P. Putnam's Sons. 1877. 17 pages.

Transactions of the State Medical Society of Arkansas, at its Second Annual Session. Little Rock, 1877. 53 pages.

Reports of the Trustees and Superintendent of the Butler Hospital for the Insane. Presented to the Corporation at their Annual Meeting, Jan. 24, 1877. Providence, 1877.

The Fifty-Third Annual Report of the Officers of the Retreat for the Insane at Hartford, Conn., April, 1877. Hartford, 1877.

The Frequent Association of Disease of the Ear with Insanity: By Dr. George C. Catlett (Read before the Association of Superintendents of American Institutions for the Insane, at the Meeting held at St. Louis, Mo., May, 1877).

State of New York. Tenth Annual Report of the State Board of Charities. Transmitted to the Legislature, January 18, 1877.

Thirty-Fourth Annual Report of the Managers of the State Lunatic Asylum, Utica, N. Y., for the year 1876. Transmitted to the Legislature, January 10th, 1877.

Royal Albert Asylum for Idiots and Imbeciles of the Northern Counties, Lancaster. Eleventh and Twelfth Annual Reports. 1875–6.

Cholera Infantum. Treatment of the Cold Stage. A Paper Read before the Shelby Co., O., Medical Society, on June 7th, 1877, by E. F. Wells, M. D., Minster, O. (Reprint from the *Cincinnati Lancet and Observer*, August, 1877.)

Restriction and Prevention of Scarlet Fever. Document issued by the State Board of Health of Michigan.

The Association of American Medical Colleges. History of its Organization, its Constitution, By-laws, Articles of Confederation, and List of Members. Detroit, 1877.

Ueber die Spastisebe Spinal-paralyse (Tabes dorsal spasmodique, *Charcot*). Von Dr. Wilh. Erb, Professor in Hiedelberg. (Seperatabdruck aus Virchow's Archiv LXX. 1877.)

THE FOLLOWING FOREIGN PERIODICALS HAVE BEEN RECEIVED SINCE OUR LAST ISSUE.

Allgemeine Zeitschrift fuer Psychiatrie und Psychisch. Gerichtl. Medicin.
Annales Médico-Psychologiques.
Archiv fuer Anatomie. Physiologie, und Wissenschaftl. Medicin.
Archiv fuer Path. Anatomie. Physiologie, und fuer Klin. Medicin.
Archiv fuer die Gesammte Physiologie der Menschen und Thiere.
British Medical Journal
Bulletin Générale de Thérapeutique.
Centralblatt f. d. Med. Wissenschaften.
Dublin Journal of Medicine and Surgery.
Deutsche Medicinische Wochenschrift.
Edinburgh Medical Journal.
Gazetta Frenocomia di Reggio.
Gazetta Medica de Roma.
Gazette des Hopitaux.
Glasgow Medical Journal
Hygiea.
Hospitals Tidende.
Journal de Médicine et de Chirurgie Pratiques.
Journal of Mental Science.
La France Médicale.
Lancet.
Le Progrès Médical.
Lo Sperimentale.
L'Union Médicale.
Mind.
Nordiskt Medicinskt Arkiv.
Norsk Magazin for Lagensvidenskabens.
Psychiatrisches Centralblatt.
Rivista Clinica di Bologna.
Rivista Sperimentale di Freniatria e de Medicina Legale.
Revue Médicale du Nord-Est.
Revue Mensuelle de Médecine et de Chirurgie.
Schmidt's Jahrbuecher der In. und Anslandischen Gesammten Medicin.
St. Petersburger Med. Wochenschrift.
The Practitioner.
Upsala Lakareforenings Forehandlinger.
Vierteljahreschrift fuer die Prakt. Heilkunde.

The following domestic exchanges have been received:

American Journal of Insanity.
American Journal of Medical Sciences.
American Journal of Obstetrics.
American Journal of Pharmacy.
American Medical Weekly.
American Naturalist.
American Practitioner.
American Psychological Journal.
Atlanta Medical and Surgical Journal.
Boston Medical and Surgical Journal.
Buffalo Medical Journal.
Canada Medical Record.
Canadian Journal of Medical Sciences.
Chicago Medical Journal and Examiner.
Clinic.
Cincinnati Lancet and Observer.
Detroit Medical Journal.
Indiana Journal of Medicine.
Maryland Medical Journal.
Medical Brief.
Medical News and Library.
Medical Record.
Medical and Surgical Reporter.
Nashville Journal of Medicine.
New Remedies.
New York Medical Journal.
Peninsular Journal of Medicine.
Pacific Medical and Surgical Journal.
Pharmacist.
Philadelphia Medical Times.
Quarterly Journal of Inebriety.
Richmond and Louisville Medical Journal.
Sanitarian.
St. Louis Medical and Surgical Journal.
St. Louis Clinical Record.
Toledo Medical Journal.
Virginia Medical Monthly.